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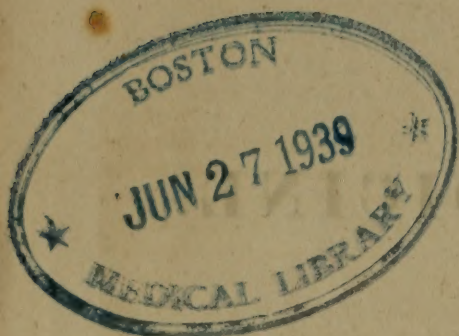
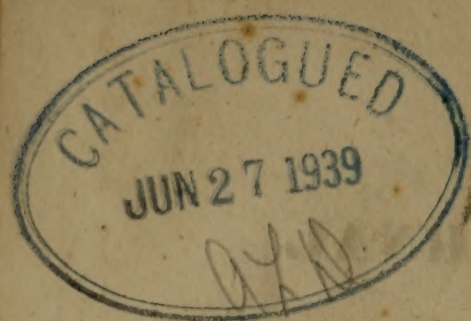
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**OBSERVATIONS, PATHOLOGICAL AND PRACTICAL, ON WHITLOE.** By DAVID CRAIGIE, M. D.

By many whitloe is regarded as a complaint too trifling to merit serious attention. When a person complains that he cannot use his hand because a finger is swelled and painful, he is recommended, in imitation of the older surgeons, to apply the balsam of Arcaeus, or Phioravant, or warm oil of turpentine; to plunge it, after the manner of Homberg and Callisen, in boiling water; perhaps to envelope it in a poultice;—or, according to the compressive method of Parry, to apply a tight bandage. If, under this management, the pain and swelling go off, nothing more is heard of the complaint; and the cure is ascribed to the powers of the turpentine or boiling water in suddenly arresting morbid action, to the emollient effects of the poultice, —or to the influence of compression in contracting enlarged vessels. If, however, the disease proceeds, and the skin giving way, unhealthy fungous granulations arise, with discharge of bloody ichorous matter, the red oxide ointment is applied partly as a stimulant, partly as an escharotic; and the finger is allowed to recover or to become worse, according to the mildness or the severity of the disease.

These notions, which are prevalent not only among patients, but to a certain extent, if a judgment may be formed from what is not unfrequently observed, among surgeons, are somewhat favoured by the phenomena and effects of certain varieties of the disease. In others, however, they are entirely inapplicable; and if I am not mistaken, to this want of discrimination in the various forms which the disease assumes, may be traced much of the inert and undecided practice with which it is too often treated. The mild varieties may recover under almost any treatment; the severe are very generally equally unfortunate under all; and almost invariably terminate either in caries of the phalanges, or ankylosis of the digital articulations, so as to render the finger, if not the hand, nearly useless.

That this was not at all times the case I am well aware. The writings of Ambrose Paré, Vol. II.—A

of Wiseman, of Gouey, of Garengot, of Heister, of David, and many other surgeons, show that whitloe was always apprehended as a disease which was not only exceedingly severe at the time, but might terminate in the destruction of useful parts. I know not that I would be justified in asserting that the instructions and precepts of these authors are entirely overlooked by surgeons. But I think I have seen some reason to infer, that, if they are not altogether forgotten, they are by no means so thoroughly understood among the generality of surgeons, as to be steadily applied in practice. I may further be allowed to say, that if the apprehensions of the older surgeons, which were founded on an idea of malignant character and morbid acrimony, were carried in some instances to a vicious extreme;—the indifference manifested by several modern practitioners, from whatever causes it originates, is not much less reprehensible.

It is almost superfluous to say, that when any part of the finger becomes painful, swelled, and unusually hot, with or without redness, it is said to be affected by a disease which has been described by surgical authors under the names of whitloe, *paranaritis*, *panaris*, and *paronychia*. There may be reason probably for inquiring whether every inflammatory disorder attacking the fingers is to be accounted whitloe;—whether it consist in inflammation modified by the texture and anatomical arrangement of the parts in which it occurs;—or whether it be disease of a peculiar kind, and the fingers are actually subject to inflammatory conditions of a different nature. I shall attempt, in the following observations, to examine this subject in such a manner as to elucidate, if not to resolve these questions, and to place in a clearer light the pathological relations of this disease.

This is not the place to inquire into the merits of all the divisions of the several forms of whitloe which have been at different times proposed by surgeons. When I mention that Gouey\* admitted five species, that Dionis recognised only one,† Wiseman

\* Chirurgie Veritable, p. 307.

† Cours d'Operations de Chirurgie. A Pa-

two,\* and that the Garengéot admits four,† while Heister refers them all to three,‡ it is easy to see that they were not regulated by a uniform principle of arrangement.

In general the French surgeons have followed the division adopted by Garengéot, which has the advantage of great simplicity in referring the four forms of the disease to the skin, the subcutaneous cellular tissue, the sheath of the tendon, and the vessels between the periosteum and the bone. From this the division of Heister differs only in converting the two first into one species;§ while that of Callisen, the learned professor of Copenhagen,|| may be quoted as the counterpart of this, since he adopts the division of Garengéot, with the addition of another species, the *paronychia subunguis*;—by which he evidently refers to the disease described by Mr. Wardrop under the name of *onychia maligna*.

The division of Wiseman, on the contrary, who referred the varieties of whitloe to two kinds, the *benign* or mild, seated in the skin, and the *malignant* or severe, affecting the ligaments and tendons, has been more or less generally adopted by the few English authors who have given the subject of whitloe any consideration. By Latta it is distinguished into two sorts, according as it is induced by an external cause, or comes on spontaneously;¶ while in the short and rather imperfect sketch of Benjamin Bell, the distinction is derived from the situation of the effused fluid.\*\* By one ingenious author indeed, distinguished for the precision with which he writes, the division of Wiseman has been modified on a singular principle. Mr. Pearson, in his *Principles of Surgery*, places the benign in the cellular tissue, thus distinguishing it from the cutaneous; while he enlarges the list thus formed by the addition of that which may on some rare occasions be traced to the operation of the syphilitic poison.†† This division has

the disadvantage of being founded on two different principles, which coincide neither naturally nor accidentally. When one division is derived from the kind, severity, or degree of morbid action, it is not altogether accordant with the principles of correct arrangement to mingle with this another distinction derived from difference of affected tissue. With much less justice can the influence of a remote cause be admitted in the same line of distinction.

To criticise the divisions of other surgeons, however, is not my object; and I have mentioned these modes of distinguishing the various forms of the disease, chiefly to show that the one originally proposed by Garengéot is superior to any of those which have since been given; and that surgical authors, in departing from this, have in general lost sight of the nature of the disease, and of the principles on which a just division should be attempted. The division of Garengéot, indeed, is liable to one objection, viz. that of superfluity, in representing one form of the disease to be seated between the periosteum and the bone. I trust that I shall show, before these observations are finished, that the disease is never primarily found in this situation, and that it passes, by a direct and easy transition, from the synovial membrane of the flexor tendons to the subjacent periosteum, which is then affected in a secondary manner only.

Every form assumed by whitloe, however varied in degree, in kind, or in extent, may be referred, I conceive, to the following three simple heads:—1st, That which is seated in the *corion*, or at least in its outer surface; 2d, That which affects the subcutaneous cellular tissue; and, 3d, That which, commencing in the synovial sheaths of the flexor tendons, passes not only to the cellular substance and skin, but to the subjacent and adhering periosteum of the phalanges.

1. The cutaneous whitloe, (*paronychia dermatica*;) is constantly limited to the skin of one, or at most of two of the fingers. It is almost never found on the skin of the hand; and it is generally restricted to one finger, or to the corion of one phalanx at a time. Though it may occur in persons of all ages and habits, the individuals particularly liable to it are in general adult, rather advanced in life, persons of meagre, asthenic, and anxious aspect, and pursuing occupations in which the fingers are more or less exposed to the operation of morbid agents.

It commences with a distressing sensation of painful searing heat, as if the finger had been scalded, referred to one phalanx, and not unfrequently with great accuracy to a single spot; while a slight streak of redness may be seen on some part of the skin. After this has been experienced for a space of time, varying in different individuals from ten to fifteen hours, considerable diffuse swelling has

ris, 1714. Démontrées au Jardin Royale, par M. Dionis. 8 Démonstration, p. 602.

\* Chirurgical Treatises, &c. Book i. chap. xi. London, 1676.

† Traité des Operations de Chirurgie, &c. Par R. J. Croissant de Garengéot Maître, &c. A Paris, 1731. Tome iii. chap. viii.

‡ D. Laurentii Heister Institutiones Chirurgiæ, &c. Pars. ii. Amstelædami, 1739. Sectio vi. chap. clxx. p. 1142.

§ Ibid. Pars. i. Sectio vi. chap. clxx. p. 1143.

|| Henrici Callisen, M. D. &c. Systema Chirurgiæ Hodiernæ. Hafniæ, 1815. Volumen I. Pars Altera, Classis i. Ordo iv. Genus iii. Species 10. Sect. 631.

¶ A Practical System of Surgery. By James Latta, Surgeon. Vol. III. Ch. iii. Sec. 8, p. 312. Edinburgh, 1795.

\*\* A system of Surgery. By Benjamin Bell, Member, &c. Vol. I. Chap. ii. Sec. ix. p. 173. Edinburgh, 1801.

†† Principles of Surgery for the use of Chirurgical students. By John Pearson, Surgeon

to the Lock Hospital, &c. London, 1788. Chap. iv. Sec. i. p. 82.

taken place, accompanied with much redness of the integuments. This discoloration, which gives place to pressure for a moment only, is seldom at the commencement accompanied with lustre; and it presents different shades as it extends along the finger. General heat and pain may occupy the whole finger, and even a portion of the hand. The swelling is confined to the finger; but the redness and the peculiar searing heat are limited to a definite space, which is generally marked by a distinctly circumscribed edge. As the disease proceeds, this edge advances regularly on the natural skin, but may be always recognized to be equally abrupt and circumscribed as at first.

When these phenomena have continued for the space of ten, twelve, or twenty hours, in some instances less, the cuticle begins gradually to be elevated, and to present a change of colour. It may be yellow or whitish, but most commonly assumes a tint bordering on purple, and at the same time becomes remarkable for its shining aspect. These appearances, which in general continue to extend from the spot originally affected over the whole finger, are occasioned by the interposition of a watery fluid between the *corion* and the *epidermis*, by which the latter is elevated from the former. This detachment generally takes place at first in small isolated patches, forming separate blebs or *phlyctænæ*. But in the course of the disease, unopposed by remedial measures, the whole cuticle surrounding the finger may be detached; and in this manner the acute or active stage of the disease naturally terminates in the course of the third or fourth day, when it is not controlled by remedial interference.

The suffering occasioned by this form of whitloe is always more alarming than seriously injurious. It is accompanied through its whole course with so much pain and tenderness of the integuments, that the patient is utterly precluded from making any use of the finger; and even the hand sometimes cannot be moved. The slightest touch I have occasionally seen produce a most distressing sensation of faintness, from the effects of which recovery took place only after a considerable time. Sleep is always more or less prevented; the appetite is impaired; and the individual is distressed with a sense of restlessness and anxiety quite disproportionate to the cause on which it depends.

These symptoms are in some degree alleviated by the effusion of fluid beneath the cuticle; but they are never effectually removed until it is discharged by spontaneous rupture or artificial opening. When this is done it is found to be a turbid serum, sometimes yellow, generally of a purple colour, thin, but seldom transparent. The cuticle is dead; and the surface of the corion is covered with a reddish, flocculent substance, and semifluid, or of the consistence of thin jelly. From opening cases of cutaneous whitloe at different stages of progress, I am satisfied that this semifluid matter is the result of a process of partial coagulation of the fluid effused by the inflam-

mation of the corion. Secreted at first in small quantity, it is very quickly separated spontaneously into serum, and a thicker coagulable matter. The former is the thin purplish fluid, which escapes when the cuticle is punctured. The latter is the thicker jelly-like matter, by which the cuticular surface of the corion is covered. Beneath it in general the surface of the corion proceeds, by secretion of proper epidermal matter, to form a new cuticle; and in a few days the original situation of the disease can be recognized only by the extent of the recent membrane, through which its tenuity allows the red colour of the corion to be distinctly seen.

In the preceding account I have been more solicitous to describe the progress and phenomena of this variety of whitloe from cases which have fallen under my own observation, than to trust to the scanty and sometimes imperfect notices to be found in authors, many of whom have allowed their observation to be misled by the descriptions of their predecessors. The cutaneous paronychia is represented by Garengeot as commencing sometimes at the root of the nail on one side, and spreading all round to the opposite corner,—“a circumstance from which,” he adds, “it has received the name of *Tourniole* among the people.”\* This view, which I find is repeated by Mr. Pearson,† of the situation and progress of the disease, though correct as to many instances of it, does not apply to all. In several cases of cutaneous whitloe which I have had occasion to see or to treat, the inflammation certainly spread from the point at which it first appeared, and seemed very often to creep round the finger; yet it also spread continuously over the whole skin of the finger, affecting not only the ungual phalanx, but the middle, and occasionally part, if not the whole, of the metacarpal phalanx. This, it will be found, was the progress of the disease in a case which I shall very soon detail. From various cases, in short, which I have seen, I infer, that, though the description of Garengeot and Pearson is correct, so far as it goes, it presents too narrow a view of the disease. I doubt even whether its commencement is confined to the root of the nail; for I have seen several examples of it originating, so far as the patient's history could be understood or credited, in a different part of the finger. In such cases, however, it invariably spread more rapidly towards the nail than in the opposite direction towards the metacarpal region.

The pathology of cutaneous paronychia is to be understood by connecting the knowledge of the anatomical peculiarities of the digital corion with that of the phenomena which are observed in the course of the disease. So far as the skin of the fingers resembles that of

\* *Traité des Operations de Chirurgie*, Tom. iii. chap. viii. p. 286.

† *Principles of Surgery*, chap. iv. sect. i. p. 83.

other regions, it must manifest the same or similar living properties, and be liable to the same morbid conditions which are common to all other parts of the animal frame. The cutaneous texture of the fingers, for example, may be occupied with the same eruptions as are found to attack that of other parts. When the skin of the fingers is burnt, wounded, or torn, the same effects result as proceed from the same causes operating on other parts of the skin. So far the process of cutaneous whitloe differs not widely from inflammation of the tissue of the corion. The disease consists of inflammation of the outer or cuticular surface of the corion, spreading along that surface continuously, and producing the usual morbid products. In this respect, it resembles the genuine erysipelatous inflammation in other regions of the skin. Like that, it shows no tendency to confine its action to one or more points, to cause the secretion of coagulable lymph or proper purulent matter; but spreading in every direction along the corial surface, it causes it to secrete a serous fluid containing a small portion of albuminous matter.

The digital corion, however, presents certain peculiarities which impress a particular character on its morbid states. It is, in the first place, exceedingly vascular, more so than in any other part of the person, scarcely excepting the face. So far as can be judged by inspection, and the effects of mechanical means, it is much more dense and compact than in any other part of the body. Thirdly, Its proper sensibility, or what Bichat terms its organic sensibility, is very considerable; and that which it derives from the minute ramifications of nervous filaments is so great, that several physiological anatomists have ascribed to this circumstance the high sensibility and nice tact of the digital corion.

To these circumstances, which are well known to anatomists, may be partly ascribed the readiness with which the digital corion assumes the peculiar inflammation occurring in whitloe. That it is in part only, I infer from the fact, which I have taken pains to verify more than once, that the paronchial inflammation, when affecting the skin, is considerably different from the pure erysipelatous inflammation, as exemplified in a scald of the same parts. In the former, the disposition to spread is conspicuous. It betrays also a greater tendency to sloughing, disorganization, and other marks of very unhealthy action. The action partakes very much of that which produces the phlyctænoid *bullae* of cutaneous gangrene; and indeed I have seen cases of cutaneous *paronychia*, which, but for their limited and local situation, presented every character of gangrenous *phlyctænæ*. The disease can further claim no essential character from its benign or malignant nature. From various circumstances it may not be severe; but it is, I am well assured, never entirely free from malignity.

Though cutaneous whitloe is not a very rare disease, in its genuine and unmixed cha-

racter it is not frequent. Occasionally it is complicated, from causes which I am unable to explain, with the species next to be mentioned.

In the following case it was as pure and uncomplicated as I have ever seen it.

1. William Attershank, cooper, aged 54. June 6. On the middle finger of the left hand, the cuticle is entirely detached from the subjacent parts by the interposition of a watery fluid, from the root of the nail to beyond the articulation of the first and second phalangeal bones, over the whole posterior and lateral aspects of the finger. The cuticle is of a leaden colour, intermixed with purple patches, and seems to be much thickened. The cuticle immediately behind the nail of the ring-finger is in like manner detached.

The whole hand is affected with considerable diffuse swelling and redness; and the pain, which is constant and very severe, with a sense of scalding in the finger, extends up the arm, sometimes to the axilla. It has been less severe, however, since the cuticle began to change its colour. He can assign no cause for this complaint, unless it be occasioned by the introduction of a splinter of wood;—of which, however, he cannot speak with certainty.

The vesication was opened by the abscess lancet, and a quantity of reddish fluid discharged. He was ordered to apply an emollient poultice for one night.

On the following day, when the finger was much less painful, and the surface of the corion was seen suppurating, the dead cuticle was entirely removed, and the exposed surface was dressed with simple ointment.

Two days after a minute splinter of wood about two or three lines long, found impacted at the root of the nail, was removed by forceps; and the following day a new cuticle was seen forming on various parts of the injured surface.

This was an instance of cutaneous paronychia, originating apparently in the irritation of a mechanical agent. Yet it is easy to see, that, though this had some share in the production of the effect, its influence must have been small. In the first place, a splinter of wood is by no means invariably followed by whitloe; and it is probable that this man, as well as many others, must have had splinters run into the fingers repeatedly without being followed by such a consequence. Secondly, it is to be noticed, that not only the finger in which this foreign body was fixed, but the ring-finger also become paronychia. The latter appears to have been of the sort mentioned by Garengeot, as known among the common people in France under the name of *Tourniole*.

The appearance of this man was indicative of a deranged state of the general health. His complexion was dingy and sallow; his look anxious and haggard; his figure meagre and exhausted; and his whole appearance bore the marks of extreme misery and distress. His digestive organs had been in disorder; and steps were taken to correct them while under

treatment. In other respects his case was favourable.

The treatment of this variety of whitloë is simple, and may be comprised in few words. As soon as it is recognised, the cuticle should be freely divided by a lancet or sharp bistoury, or a *cornea* knife. The detached cuticle should then be removed entirely, or at least divided crucially, so as to allow the flaps to be reflected. After this has been done, it in general heals readily under the application of simple ointment, or the ordinary calamine cerate. If, however, it still betrays a tendency to spread, free removal of the detached epidermis, followed by the application of the resinous ointment, will rarely fail to effect a cure. In some instances early division of the skin by the scalpel is of much consequence in checking the progress of the disease.

II. The subcutaneous or cellular whitloë, (*paronychia subcutanea, parenchymatosa, paronychia talæ cellulosaë*;) or that seated in the cellular tissue is confined, in general, with very great accuracy to the pulp of one of the fingers. Whether it commences suddenly or slowly, it is easily distinguished by this circumstance, and by throbbing pain confined more particularly to a single spot, than in the cutaneous whitloë. Not unfrequently, however, especially when it spreads laterally, it is complicated with more or less corial affection, the cuticle rising in a semi-transparent vesicle or bled at one part, while the rest imitates the circumscribed swelling of inflammation of cellular tissue. General pains extend over the hand, and occasionally spreads up the arm.

When these symptoms have continued for a day or two, occasionally several, according to circumstances, the cuticle gives way by rupture at a single point in general; and a small quantity of blood-stained serous fluid escapes. When it is next examined, a soft spongy brown red-coloured body is protruded through the opening in the cuticle, by the margin of which it appears to be almost strangled; and by pressure a little blood-coloured fluid, mixed occasionally with particles of lymph matter, may be expelled. If at this period the cuticle be removed, in the corion will be found an irregular opening containing a soft brownish-red granulation, and leading to the subcutaneous cellular tissue, in which an irregular cavity is thus formed. The pain, though with less throbbing, continues notwithstanding; and every attempt to examine the finger, or ascertain the extent of the disease, produces most acute suffering.

The duration of this form of the malady is influenced by several different circumstances of age, habit of the individual, exciting causes, and above all, by the mode of treatment.

In young subjects paronychia is comparatively a rare disease. In them the influence of morbid agents is less powerful than in persons more advanced in life, and whose mode of living is less calculated to resist the influence of disturbing causes. When, however, the paronychial action is developed in a young person, it almost invariably assumes the paren-

chymatous form; and its progress is then sufficiently quick. It is not improbable that this duration of the morbid process is occasioned by the peculiar susceptibility of the tissue which, under these circumstances, becomes the seat of the disease. I am not prepared to prove that the skin and the synovial sheaths are at an early period of life less liable to inflammatory action than the cellular texture of the fingers; nor would it be easy to discover on what properties, anatomical or physiological, such a peculiarity depends. From a considerable number of cases, nevertheless, of the different species of whitloë, I think I am justified in the inference, that every example of subcutaneous whitloë which we observe is found in persons under the age of 30 or 35. Several of the cases which I have seen or treated were in young women between 18 and 25; and I do not at present remember more than one instance of genuine subcutaneous whitloë in a person beyond 40. While I hazard this affirmation, however, as a general inference, it is not my intention to say that individuals under this age are exempt from the other kinds of whitloë. Before this period of life certainly they are rarely seen; but examples, especially of thecal paronychia, as I shall show, not unfrequently present themselves.

The subcutaneous whitloë, then, which is in a great degree peculiar to early life, is exceedingly rapid in progress. I have seen it terminate in resolution in forty-eight hours; and the space of five or six days is a common period in cases in which little or no matter is formed. The cuticle is then detached and thrown off dead. Even in the worst circumstances the duration of the disease seldom exceeds eight or ten days; for the patient then has recourse to surgical assistance, and by the use of appropriate remedies the career of the disease is arrested, or its progress to destruction of texture is at least moderated.

Previous to this time, indeed, a termination of the disease may be effected either by resolution or by the formation of more or less purulent matter; and as this is much influenced by the treatment, we occasionally meet with cases in which the symptoms continue for a much longer time, without tending decidedly to one event or another. It often happens that the symptoms of subcutaneous whitloë, either from constitutional peculiarity, or from the periodical alleviation of which the disease is naturally susceptible, cause so little inconvenience to the patient, that he continues to use the finger through the great part of the course of the disease. The throbbing pain is not constantly of the same degree of severity; and though a general sense of soreness and uneasiness in the finger and hand prevents the patient from forgetting his disorder entirely, the piercing severity of that in the pulp of the finger is liable to intervals of remission. Under these circumstances, some convenient domestic remedy is applied; and if this takes place, as is most natural, when the paronychial throbbing and piercing are at the worst,

the slight alleviation which follows is naturally ascribed to the operation of the remedy. After this alleviation, the patient again proceeds to take liberties with the finger, till the return of the urgent tearing pain obliges him once more to try the effect of his remedy. In this manner the disease is in many instances alternately palliated and aggravated for days successively, without being brought to any positive termination; and it may continue for weeks even, until it is either, by adhering to some decided measure, permanently cured, or terminates in irregular suppuration and sloughing of the cellular membrane.

When it proceeds to this extent, an irregular sore is formed in the skin and cellular tissue, with much swelling of the surrounding phalanx. The sore is generally sinuous, with a narrow orifice, from which soft, fungous, unhealthy granulations protrude, with much pain, and a blood-coloured sanious discharge, or at least purulent matter, crude and imperfectly secreted. If, in this stage of the complaint, it is still neglected or treated inertly, the first result is in general the loss of the nail, with much pain and suffering; and the second is the extension of the disease to the periosteum of the ungual phalanx, which then becomes carious, and causes a tedious train of complaints, unless the unsound bone is expelled by the process of nature, or removed seasonably by the interposition of art.

Subcutaneous whitloe, as thus described, is a much less formidable disease in the outset. Seated originally in the subcutaneous filamentous tissue, and partly in the adipose cushion, which forms the pulp of the finger, its pathological peculiarities depend chiefly on the anatomical and physical properties of these tissues. The adipose layer here is peculiarly dense, compact, and granular. Something also seems due to the tenseness of the skin of the fingers, which less easily yields to a distending force than in other parts of the body. This at least appears to be one reason why subcutaneous whitloe, if not resolved by prompt and judicious treatment, invariably produces more or less death of the cellular tissue, and not unfrequently of the corion itself. In this respect it is not dissimilar to the furuncular inflammation, from which, however, it differs in commencing, at least in the subcutaneous cellular and adipose tissue. Afterwards it certainly affects the corial tissue, and, if neglected, it passes in the reverse direction to the phalangeal periosteum.

Of the causes of this form of whitloe I confess I know little. Some cases I have traced to mechanical injury, a puncture with a needle or pin, or a splinter of wood, a bruise, and in some instances a lacerated wound. The proportion of cases, nevertheless, in which no circumstance of this kind can be recognised is so great, as abundantly to show that it is entitled to no other rank than that of an occasional exciting cause. In many instances I have seen it connected with symptoms of constitutional disorder. Several cases of subcutaneous paronychia which have fallen under my obser-

vation have occurred in young females, whose general health was delicate, and liable to frequent interruption from gastric, gastro-intestinal, or uterine irritation; and it is worthy of remark that the disease appeared to be as much under the control of the remedies ordered for the general disorder as of those employed for the local complaint.

The treatment of this disease is too often neglected till it has attained the suppurative, or rather the fungating stage. If it is taken at an early period, it may be cured with the greatest certainty by one good longitudinal incision through the skin and cellular membrane; and, if continued to the periosteum, it does no harm. The vessels of the parts are thus enabled effectually to empty themselves; the tension of the skin and cellular tissue are completely taken off; and the wound heals up, almost by adhesion, in a day or two, without any permanent inconvenience or deformity.

Many, I am aware, object to this practice as harsh and unnecessarily severe for a disease so slight as whitloe; and a poultice is too generally conceived the appropriate application. By others again the incision is condemned as irrational in the extreme, where there is no matter to be evacuated, and recommend it to be postponed till this consummation is effected. No reasoning can be more erroneous than that which dictates the treatment in this manner. It should be the duty of the surgeon to procure resolution, and to prevent the formation of matter; for if once this result takes place, the part which he has to perform in affording relief is that of a passive spectator only. In many instances of whitloe I never yet knew the poultice adequate to effect resolution, or to exercise any influence whatever on the disease, save that of relaxing the skin, and enabling it to be distended, or rather softened, with a very little less pain. The most powerful and certain resolvent, and the only one which I have found never to fail, is the longitudinal incision of sufficient depth, always understanding that it is not to be deferred till suppuration takes place. Though still useful, it then operates in a different way.

I am further aware that it is impossible to convince timorous patients of the necessity of the incision for the cure of whitloe; and in such circumstances a less formidable remedy is expedient. In one or two instances I have thought I succeeded in procuring resolution by the application of leeches to the affected part, and promoting bleeding by the emollient poultice. To this remedy, however, it is by no means safe to trust; and in the majority of cases it will at last come to the incision, when much less efficacious. The leeches, in short, may be recommended only in those cases in which John of Gaddesden found palatable and costly medicines expedient.

It is by no means, however, always the fault of the surgeon that resolution is not obtained. Many persons never think of applying for surgical advice till suppuration is either far advanced, or a bad and unmanageable sinuous sore, with fungous granulations, is

established. The most effectual treatment in such circumstances I find to be division of the affected part by a deep longitudinal incision by the scalpel, so as to allow the application of local remedies to the seat of the disease. If to this purpose the longitudinal incision be inadequate, it should be crossed by a transverse one not less deep. After this it is of little moment what applications be used. The most generally suitable in the trials which I have made is the ordinary resinous ointment. When the process of sound granulation seems slow, the best remedy next to and after incision is the free application of the lunar caustic in substance. This is more especially indicated if the unguinal phalanx is felt rough and irregular by the probe. In one or two instances in which this was the result of neglected subcutaneous whitloe, after a free incision down to the bone, followed by the application of the resinous ointment, healthy action took place, and the granulations healed firmly over the bone. In other cases, however, the unguinal phalanx is so much injured that it must be removed.

It was my intention to illustrate the general principles above stated by the detail of several examples of this form of whitloe. I find, however, that this would exceed the limits within which this sketch must be confined; and I therefore mention briefly the following only.

2. January 21, 1817.—M. W., a young woman aged 22, has a large unshapely diffuse swelling affecting the unguinal and middle phalanges of the index finger of the right hand. The swelling is greatest in the site of the unguinal phalanx; and near the centre is a red prominent spongy mass, by the side of which the probe enters a short space obliquely through the skin and cellular membrane. The resistance of a bone is felt, but the surface is not rough. The sore discharges thin blood-coloured matter, and is the seat of much pain, which has been aggravated every night of late.

This succeeded an attack of much painful swelling, which first appeared about twelve days ago. Six days after the skin gave way; and in a few days more the spongy granulation appeared. She used poultices; but during the last three or four days she received from an apothecary some red precipitate ointment, with which she was ordered to dress it.

A deep longitudinal incision was made to-day through the skin and cellular membrane of half of the middle and the whole of the unguinal phalanx, passing through the sinuous opening; and a slip of *charpée* immersed in resinous ointment was inserted into the wound. The second day after, when the general swelling was much abated, it was dressed simply; and in three days more it was healed up.

3. January, 1819. J. T., aged 28, servant. The unguinal phalanx of the middle finger is much swelled, and so painfully hot and throbbing, that she finds relief only from immersing it in ice-cold water. The cuticle is elevated like a blister; at one part dark-co-

loured, as if ecchymosed, and the pain and swelling at the margin of the nail is severe to an extreme. The finger first felt painful about five days ago, soon after a contusion with an axe.

A deep incision was made through the whole pulp of the finger, dividing the skin and cellular membrane. The wound bled freely. After immersion in tepid water, a slip of *charpée* dipped in resinous ointment was applied over the wound for two days. On the third it was dressed with adhesive plaster; and she returned after six days with the finger quite well.

4. John Johnstone, labourer, aged 50. October 21. The middle finger of the right hand is swollen, painful, and a little discoloured. On the palmer surface, corresponding to the middle of the unguinal phalanx, the cuticle is detached; and near the middle a single opening, which emits a discharge of sanguineo-purulent matter, leads the probe directly inwards to feel the resistance of the bone, but without encountering a denuded surface. The pain, which extends over the whole hand and up the arm, though pretty constant, is intolerably severe during the night. In consequence of an injury received, as he thinks, five weeks ago in reaping, the finger began to be affected with swelling, painful tension, and much heat. These were generally so much aggravated during the night as to prevent sleep completely. Without any other remarkable occurrence, they went on till about three days ago, when they became less severe, in consequence of the spontaneous opening which previously took place.

A deep incision made passing through the opening, followed by the application of a poultice. He was also ordered an anodyne at night. In two days it was dressed lightly with resinous ointment; and in a few days more the wound healed up, leaving only slight stiffness and occasional pains. These were probably the effect of the long time during which the disease had been suffered to go on uncontrolled.

5. In October, 1820, I was requested by a medical friend to look at the finger of a girl, whose name has escaped me, in the village of Morningside. It was a distinct case of subcutaneous paronychia; for the pain and swelling of which, affecting not only the finger but the whole hand, she had been under the necessity of quitting her situation. By the use of poultices which she had been applying sedulously for several days, the cuticle was loose, macerated, and whitish. But the pain was little abated, and the swelling was on the increase. The whole hand had a distressing sensation of painful weight, which obliged her to avoid using it, and sit by the fire with the arm suspended in a sling. No matter was yet formed.

I made along the middle and unguinal phalanges a deep longitudinal incision, from which a good deal of blood and some serous fluid flowed. After immersion in warm wa-

ter, a slip of charpée, moistened with resinous ointment, was placed between the lips of the wound. In the course of eight days after, under the usual treatment, the finger was completely restored.

6. In the following case resolution appeared to be effected under the use of leeches. A young lady had painful throbbing swelling of the ungual phalanx of the index finger of the right hand for eight or ten days. Poulitices, Goulard extract, acetous and spirituous embrocations, had been alternately applied, with the effect of procuring temporary relief. But the painful sensations always recurred in a more aggravated form. When I saw it the finger was hot to the touch, red, tensely swelled, and so tender that it was difficult to make the necessary examinations.

I was desirous to employ incision immediately. But to this the patient was so reluctant, that I found it requisite to recommend, as the only alternative, the application of five leeches. Four were instantly applied, and drew a good deal of blood. The finger was afterwards immersed in warm water, and then enveloped in a poultice composed of the flowers of *Anthemis nobilis* well bruised. From this time the swelling subsided rapidly, the pain and heat went off more slowly, and the finger was finally restored.

Other cases of the same description which have since occurred to me, it would be too tedious to adduce. They illustrate the general principle which I wish to establish,—that the surgeon should never, if possible, allow the disease to proceed to suppuration; that he may always prevent it by seasonable incision; and that it is an erroneous principle to confine this measure to those cases only in which matter is formed. By the prompt employment of this method much painful suffering is avoided; time is saved; and the probable destruction of the nail and ungual phalanx is prevented. The effect of the incision is to relieve tension and empty effectually the overloaded vessels.

III. The third form of whitloe, (*paronychia thecalis*, *p. synovialis*,) the felon or malignant whitloe of Wiseman, or that which is seated in the synovial sheath of the tendon, is not only the most painful and the most difficult to subdue, but is too often allowed to proceed to such an extent, before any active measure is taken to retard its progress, that the textures forming the delicate and complicated mechanism of the fingers are either seriously injured, or for all useful purposes irreparably destroyed.

Showing itself first in painful stiffness and diffuse tense swelling of the metacarpal and middle phalanges, it may either continue confined to these, or extend up along the lower surface of the palmar fascia, under the annular ligament of the wrist, by its tendinous arcades and sheaths, and beneath the fascia of the fore-arm to the neighbourhood of the *pronator radii quadratus*.

If it were of much practical importance to distinguish this form of whitloe, I should be

inclined, from the cases which have fallen under my observation, to say, that, when confined to the finger, it is almost always an acute disease, proceeding with considerable rapidity to effusion of sero-purulent fluid; when affecting the palmar fascia it may be acute, but is often chronic; and when in the annular ligament, it is generally the effect of the disease of the finger being long neglected, or inertly treated.

In the tendinous sheaths its phenomena and progress are nearly the following. After a sense of throbbing pain and heat felt deep in the finger, and impeding its free motion, diffuse swelling, with more or less redness of the integuments, is observed. With various intermissions, especially of the pain, swelling continues, spreading over the hand and to the adjoining fingers, for three, four, or five days; after which one part of the finger generally becomes more prominent than the rest; and in two or three days more the finger has become so œdematous, that surgical interference is thought necessary. The skin does not often give way spontaneously; but the surgeon is too frequently tempted to wait, till under the use of poulitices he conceives the matter is ripened. An incision is then made with a lancet, and a little sero-purulent fluid is discharged; and under the subsequent use of the poultice an ash-coloured piece of dead tendon makes its appearance at the opening. In this state the inflammation has effected death of the tendon, which eventually sloughs out by a long and tedious process; while, by adhesion and granulation within the sheath, its cavity is obliterated; the phalanges are either permanently incurvated, or rigidly and immoveably straight; and the use of the finger as an organ of prehension, is utterly lost.

This may be represented as the simplest form of thecal *paronychia*, in which its action is confined chiefly to the sheath of the flexor tendons. It by no means rarely happens that the disease passes from the sheath to the subjacent and attached periosteum, when the inflammation established in this membrane speedily proceeds to caries, or death of the enclosed bone, with more or less unhealthy suppuration. This result, which is said by surgical authors to be most common in the ungual phalanx, requires, however, some modification.

According to the original spot of the tendinous sheath which the inflammation affects, and according to the subsequent direction which it takes, it may affect either the articular capsule of the middle and ungual phalanges, or it may affect, which I have more than once seen it do, the whole extent of the middle phalanx. In the former case it almost constantly produces caries of the contiguous articulating ends of the middle and ungual phalanges. In the latter case, death of the whole middle phalanx is not uncommon. In other instances, however, partial death of part of the bone may be produced; and to this is then superadded caries, as an effort of the sound bone to rid itself of the

dead portion. These states are easily recognised. One or more sinuous openings admit the probe to the bone, the surface of which is felt denuded, irregular, and grating; and not unfrequently the entire phalanx is more or less loose.

This last form of the disease has been described by Garengéot, by Heister, and others, as distinct from that which is seated originally in the synovial sheath of the flexor tendons. It is easy, however, for the anatomist to show, that inflammation commencing in the synovial sheath of these tendons, must, anatomically speaking, if not subsiding spontaneously, or checked by art, pass speedily to the periosteum of the phalanx, or the articular capsule, to which it intimately adheres. It is not my intention in this sketch to enter minutely into the details of the anatomical arrangement and characters of the textures composing the mechanism of the fingers. That has been already done by Camper with such accuracy and perspicuity, in allusion even to the very disease of which I now speak, as to render any description from me superfluous indeed.\* I shall only recall, however, to the attention of the surgeon one or two points, which are necessary to maintain the simple principle for which I here contend.

The direction and insertion of the superficial and deep flexor tendons I presume to be well known. Their transition through a fibro-synovial canal attached to the *phalanges*, I also understand to be familiar. Let this sheath be divided longitudinally by the scissors; and its connexion, formation, and structure, will be manifest. It will then be seen that it consists of two parts, one fibrous, opaque, exterior; the other synovial, transparent, and interior. The fibrous exterior layer, consisting of decussating fibres connected by cellular tissue, adheres to the lateral margins of the metacarpal and middle *phalanges*, and is lost, with the tendon of the deep flexor, on the upper half of the digital *phalanx*. This fibrous layer further, when traced carefully, is found to pass insensibly into the periosteum of the phalanges, and in truth may be said to adhere to it intimately. The transparent synovial or interior layer, which is exceedingly thin, and is most distinctly demonstrated by boiling, or immersion in hot water, lines this fibrous case above and laterally; but below, that is on the anterior or palmar surface of the phalanges, it lies in immediate contact with the phalangeal periosteum. Its connexion with the articular capsules is too obvious to require particular notice. The effect indeed of the arrangement is such, that the entire synovial sheath is covered below by periosteum or articular capsule, and on the sides and above by a fibrous membrane. Altogether it may be represented as a *fibro-serous* or rather *fibro-synovial* membrane.

This synovial membrane, however, is not limited to the extent now mentioned. At the insertions, respectively, of the deep and the superficial flexor tendons, it may be traced from the fibrous layer backwards or upwards along the surface of the tendons themselves, and through the slits of the tendons of the superficial flexor. With some care this synovial investment may be traced upwards, at least as far as the tendons are lodged on the digital phalanges, when, corresponding to the lower extremity of the metacarpal bones, they are surrounded with the celluloso-adipose layer of the palm. It hence results, that on the palmar surface of each phalangeal row there is situate a synovial membrane, which, in the manner of a shut sac, not only lines the exterior fibrous sheath, but is reflected, in the language of the anatomist, over the contained organ.

The tissue of the parts now described is the seat of this form of paronychia. Inflammation, originating in the synovial sheath, is speedily propagated, not only over its surface, but to the enclosing tissue, viz. the fibrous case above, and the phalangeal periosteum and articular capsules below. It is easy to see how readily it must, from this arrangement, pass to the tendons also, which, partly from this cause, chiefly from a degree of strangulation resulting from the diminution of the caliber of the cavity by the swelling of the synovial membrane, both of the sheath and of the tendon itself, are speedily killed. In short, the anatomical arrangement of the digital synovial sheaths explains at once the two principal phenomena for which thecal paronychia is distinguished,—death of the tendons, and caries of the phalanges, or their articular extremities.

The connexion between these sheaths and the palmar fascia, and the annular ligament, shows the facility with which the transition of the inflammation to these latter parts may be effected. The palmar fascia, indeed, connected at its lower margin with the digital sheaths, and at its upper with the annular ligament, constitutes a very direct line of communication between the former and the latter; and, in point of fact, whitloe established in the finger speedily passes to the lower surface of the fascia, and in no long time to the synovial canals of the annular ligament. This, which was seen by Garengéot, by David, and by Sue, I have myself witnessed more than once; and it constitutes a variety of the disease, not only obstinate and troublesome to manage, but at all times severely injurious in its effects, attended with extreme pain and suffering, precluding sleep, and impairing the general strength. By passing to the articular capsules, it is often in danger of inducing caries of the carpal bones.

On the treatment of this form of whitloe some difference of opinion has prevailed; and, so far as I can discover, no very steady principle has been held in view in the several methods proposed. It was early recognised by Guy de Chauliac and John de Vigo as a

\* Petri Camper, A. L. M. Phil. et Med. Doct. *Demonstrationum Anatomico-Pathologicarum*. Fol. Amstelædami, 1760.

malady painful and dangerous in the extreme, from the constitutional disorder which it causes, and even as occasionally fatal. In imitation of these surgeons, Paré recommended, with proper medical treatment, incision, deep and long, in order to give an outlet to poisonous and virulent matter.\* Wiseman, aware that, if left to suppurate, the bone infallibly corrupts, or is affected by caries, with the view of giving the tumour what he terms a breathing, cut deep into the part most affected.† Both by Dionis‡ and Garengéot,§ the necessity of opening the sheath of the tendon by incision to evacuate the contained matter is inculcated; and the same measure was afterwards strongly impressed by M. David in his elaborate memoir on the Nature and Treatment of Abscesses, not only to give vent to matter, but to diminish the tension and strangulation of the membranes.¶ The justice of this doctrine has been in general admitted by surgeons. I shall show, however, that it is not conducted on the proper principle; or at least that the principle has not been understood.

It has been often observed that the incisions practised as recommended by Dionis, Garengéot, and David, were not followed by the subsidence of the disease and the restoration of the finger. On the contrary, it too often happened that after incision the disease seemed to proceed quite as violently as before, and in some instances to advance with increased rapidity and severity to destruction of the texture of the fingers. It was, therefore, argued that incision, if not bad, was at least not applicable to all cases of the disease, and that to the cure of many it was quite inadequate. M. David indeed himself had remarked that incisions which penetrate only to the seat of the sero-purulent or puriform matter, however long, are rarely sufficient to bring the disease to a favourable termination. The skin, cellular membrane, palmar aponeurosis, and tendinous sheaths, he remarks, are so swollen, that the traces of the incisions are soon lost. The parts rise above them, and conceal the cavity of the inflammatory process; and the purulent matter thus confined, he thought, becomes acrid and corrodes the bone. To prevent all these evils, therefore, he proposed, instead of merely dividing the sheath,—the operation

then in use,—to remove by one stroke of the bistoury, down to the tendon or the bone even, a portion of the incumbent soft parts; and thus by a wide and extensive wound to give free vent to the thick, gray, curdy matter which the abscess contains. Thus he conceived the seat of the disease would be completely exposed to the access of remedial means; its further extension would be arrested; and caries of the phalanges so often observed was to be prevented.\*

Whether this method really answered the object contemplated by its author I know not; for I have never met with surgeons in this country who practised it, and I never myself found occasion to do so. M. Sue states that its excellence is confirmed by the daily experience of practitioners in France.† M. David himself appears not to have put it to actual trial.

Another method of obviating the evils of thecal paronychia, without having recourse to incision at all, was proposed and practised by M. Foubert. This surgeon, distinguished for ingenuity and a thorough understanding of the principles of his art, was led by theoretical views, which this is not the place to explain, to think that escharotic substances presented an infallible means of checking the pernicious action of thecal paronychia; and with this view he applied, at an early stage of the disease, pastils of corrosive sublimate to the affected finger, and even on the extremity of the flexor tendons.‡ The method of Foubert, though imitated and defended by his pupil Fabre, seems nevertheless to have fallen into disuse, if not into complete oblivion; for in 1788 its merit was again discussed before the Academy of Surgery by M. Dubertrand in contrast with the method by incision, which was defended by M. de la Porte.§

After comparing such cases as are recorded by the most eminent French surgeons with those which have come under my own observation, I think I am justified in the inference, that the true theory of the use of the incision is either overlooked or imperfectly understood. All of them proceed on the principle to which I have already adverted, that incision is premature while there is no proof of matter. It is conceived to be a measure not only cruel, but irrational, to augment the severity of a very painful disease by incisions, when there is no matter to be discharged. It is therefore said the surgeon must wait until he detect

\* Opera apud Uffenbach Thesaurum, p. 184.

† Chirurgical Treatises by Richard Wiseman, book i. chap. xi. p. 56. Lond. 1676.

‡ Cours d'Operations de Chirurgie, &c. par M. Dionis. Paris, 1714. 8tieme Demonstration, p. 603.

§ Traité des Operations de Chirurgie, &c. Par R. Jacques Croissant de Garengéot. Tome 3ieme. A Paris, 1731. Chapit. viii. art. ii. p. 310.

¶ Mémoire sur les Abscès. Par M. David.—Memoires sur les Sujets proposés pour le prix de l'Academie Royale de Chirurgie, Tome ix. A Paris, 1768.

\* Mémoire sur les Abscès chez Memoires pour le Prix, tome ix. section 3ieme, p. 260.

† Reflexions et Observations Pratiques sur le Panaris. Par P. Sue aîné. Memoires de la Societé Médicale d'Emulation. Seconde année. A Paris, An. vii. p. 303.

‡ Essai sur differens Points de Physiologie, &c. Par M. Fabre. A Paris, 1783. p. 157.

§ Reflexions et Observations Pratiques sur le Panaris. Par P. Sue aîné. Memoires de la Societé, &c.

fluctuation; and then only should he use the knife.

This reasoning, which is founded on a loose analogy derived from the general rules of opening abscesses, is erroneous in the extreme. It is the delay till suppuration takes place that renders incision of little or no avail. The interior of the digital synovial sheaths is by no means similar to a portion of cellular membrane. They are fitted for containing the flexor tendons; and the very circumstance which enables them to give these tendons support and resistance in the motions of the fingers, viz. their dense and unyielding character, prevents the sheaths from doing more than merely containing them. The moment inflammation commences in their cavity, as it spreads quickly over the whole, the swelling which ensues diminishes the cavity so much as not only not to contain easily the tendons there placed, but to induce quickly an extreme degree of compression. The unyielding nature of the fibrous case causes it to operate like a tight ligature over the whole course of the tendons. It thereby arrests mechanically the circulation within them; and this, as in other textures, speedily terminates in death of the tendon. This happens so invariably in thecal whitloes that the fact cannot, I conceive, be questioned. Perhaps the low degree of vital power which tendon is known to possess, may contribute somewhat to this effect. But that inflammation, as inflammation, is not the sole cause, I infer from the fact, that tendon is often inflamed in wounds and other injuries, without being completely killed. In short, it is a fact of which I have satisfied myself by examination of several cases of thecal paronychia, that, long before any suppuration takes place, more or less, sometimes the whole, of the flexor tendon is completely dead. It is, I conceive, no exaggeration to say, that ignorance of this fact is the reason why the surgeon waits until suppuration takes place, and thereby allows the season for checking the disease with effect to expire unimproved. Mortification of the tendons and extension of the disease to the palmar fascia and annular ligament, which is imagined to follow the incision, had taken place long before that event. Even before incision the periosteum is too often affected in sufficient time to produce caries, or even death of the enclosed phalanx.

For these evils the natural remedy, I conceive, is much earlier employment of the incision. To be efficient, indeed, the principle of this measure requires to be completely changed. Instead of using it with the view of opening an abscess and discharging purulent matter, it should be employed, first, as powerful and efficient means of local depletion; secondly, as the means of preventing that compression which, by its mechanical power only, must speedily arrest the circulation in the tendons, and thereby produce mortification of their texture.

It is chiefly from remarking the unfavourable terminations of cases of whitloes, treated

on the rules of Dionis, Garengot, David, and Pearson, left to the use of the poultice, or even neglected entirely, as often happens with patients who will not apply for professional assistance, until the disease is no longer tolerable, that I was led to employ incisions deep and long at an earlier period than is generally thought necessary. The effect was invariably more favourable; sometimes that of complete resolution. More than once, however, I have been interrogated on what principle I pursued this practice, and at first I was unable to give any other reply than the favourable result. The explanation above given is, I conceive, the true one; and if it be so, it demonstrates most indisputably the rule which I wish to establish, that not a moment should be lost in dividing, from the one end to the other, the tendinous sheath of the affected finger. It is almost superfluous to say, that the precept of Dionis of incision by the lancet, and that of Garengot by the bistoury and grooved probe, are equally to be abandoned as inert and inadequate. The scalpel is the instrument which is to be used by the surgeon who wishes to subdue the disorder by one decisive measure.

I am not unaware that against this method, as against others, objections may be urged by those who view the disease through its symptoms only. Some, I dare say, may still ascribe the death of the tendon and the extension of the disease to the incision; and others may look upon the division of blood-vessels or even nerves as an objection. These I do not conceive it necessary to answer. I am satisfied from many facts, and the observation of wounds of tendons and tendinous sheaths, that no incision necessarily produces death in these textures. The division of blood-vessels is the best method of checking inflammation; and I am certain no hemorrhage from this source can ever cause inconvenience. If a vessel is partially wounded it should be divided entirely; and if that does not suffice it must be tied. This, however, I never had occasion to do but once; and I am disposed to think that a little time, with a piece of charpée, would have answered the purpose equally well. I may add, that this did not occur in the finger, but in the palm, where the anastomosing branches of the volar arch had been wounded. In the fingers the longitudinal incision, instead of dividing large vessels across, slits them longitudinally; and if bleeding from such a source goes to an extreme, the transverse division of the vessel or vessels furnishes a ready method of suppressing it.

Camper indeed has taken pains to remove all fears on this head, by showing that the nerves and vessels run by the side of the tendinous sheaths, and that the incision along the middle of the sheath can never be attended with danger. "*Scalpellus autem mediam digiti partem dividere debet, vaginam, ligamenta, tendines compescentia ad os usque; neque nervorum aut vasorum laesione perterritus esse debet Chirurgus. Hallucinantur igitur qui a latere sectionem faciendam proponunt.*"

Media pars tutissima est, quemadmodum exposuimus.\*

After division of the sheath it is scarcely requisite to do more than dress it simply and lightly. The stimulating dressings of the old surgeons, as the balsam of Arcaeus, of Phioravant, or *du Commandeur de Perne*, are not requisite unless the tendons are partially or entirely killed; and indeed these hot balsamic applications are liable themselves to operate as escharotics, by inducing an extreme action in the vessels of the tendons. Much less is the proposal of M. David, of torrefying the surrounding parts by hot oil of turpentine, to be adopted. It is only when incision has been deferred until the tendons are mortified, that these stimulating applications may be used to facilitate the process of sloughing by ulceration and granulation; and to this the warm dressing (*linimentum terebinthinatum*) composed of turpentine and resinous ointment, or even resinous ointment alone, is amply sufficient.

The free division of the tendinous sheath will not, when deferred, be adequate to prevent mortification of the tendons; nor do patients always apply in time to enable the surgeon to put in practice this efficient measure. The moment, however, the disease is recognised, no time should be lost in waiting for suppuration, or on any other pretext. If the incision be not in time to save the tendon, and preserve the motions of the *phalanges*, it may at least arrest the progress of the disease to the annular ligament, and, above all, to the periosteum of the phalanges and their articular capsules, and to prevent caries of these bones. When this takes place, of course excision or amputation is the most speedy and effectual remedy; and perhaps in nine cases of ten it will be most expedient. In some instances, however, I have seen a free incision down to the bone, and dressing with resinous ointment, followed by consolidation of the parts. The carious portion of bone in such circumstances must have been small, and was probably removed not by exfoliation, but absorption.

These observations have extended to such a length, that I find it impossible to introduce all the cases which it was my intention to have done. The following, however, may illustrate some of the points which I wish to impress.

7. James Wilkie, labourer, aged 43. March 27. The middle finger of the left hand is considerably swelled, stiff, and extremely painful, with a leaden colour in some parts, a red in others, and a glistening lustre of the integuments. On the posterior surface the fluctuation of matter is obvious. The whole hand, and each of the fingers, is affected with

much swelling, in general of a white colour. This complaint began with pain in the middle finger, which had been previously contused, and exposed, as he thinks, to cold. He has applied nothing but poultices.

An incision made on the back of the finger discharged some blood and much matter, and discovered an extensive space of the metacarpal phalanx denuded. The next day the swelling was diminished, the leaden colour disappearing, and the general appearance improved. Another incision made on the palmar surface discovered the flexor tendons dead, and part of the metacarpal phalanx denuded. To the proposal of removing the finger the man would not submit; and deep and extensive incisions were made, and the warm dressings used. Under this treatment, the soft parts of the posterior surface first adhered by granulation with the bone. At the anterior the flexor tendons sloughed away, and union was at length effected without the loss of the bone. The motion of the finger was, however, lost, and the different phalanges remained permanently semiflexed. Some months after, when I saw this man quite well, he regretted that his finger, which was of no use, was not removed.

The following resembles some of the cases of Garengoet.

8. In a woman aged 65, the whole hand was swelled, tense, acutely painful, especially when touched, and the seat of a sensation of throbbing heat, most remarkable in the thumb. The painful tension and swelling were so general, that the motions of all the articulations were much impaired. She could assign no cause for these complaints, except the introduction of a small splinter of wood into the anterior part of the thumb near the last articulation. She applied nothing but poultices. An incision immediately made on the posterior surface of the thumb was followed by diminution of pain, and better nights.

Between this time, 14th April and the 20th, abscesses formed in various places, both in the palmar and antipalmar regions of the hand. They were opened by free and extensive incisions.

On the 1st May she had been restless during the night, with pain in the hand and wrist, which was much swelled with redness and tenderness of the skin. An incision discharged blood mixed with purulent matter; and a swelling which appeared the following day on the opposite side began to diminish. In ten days more the articulation at the thumb was found preternaturally moveable; and there was much pain and tension in the palm. A free incision made through the palmar fascia longitudinally was followed by complete alleviation of the pain and swelling there; but the articulating extremities of the phalanges of the thumb were rough and grating. The diseased bone was removed by amputation. The articular ends were found quite carious; and an ulcerated opening was found near the middle of the articulating surface of the last phalanx.

\* Petri Camper A. L. M. Philosoph. ac Med. Doctoris, &c. &c. Demonstrationum Anatomico-Pathologicarum liber Primus, Continens Brachii Humani Fabricam et Morbos. Amstelædami, 1760. Tab. I. fig. ii. chap. i. p. 6.

In the following case the disease was chiefly in the palm.

9. I was requested, on the 13th December 1819, by a medical friend, to see Mrs. D., aged 69, whose hand had been swelled and painful nearly three weeks. The left was the seat of considerable diffuse swelling, great tension, and hardness; much pain, aggravated by pressure or motion of the fingers, with a sense of burning heat. The colour of the integuments was dull red, slightly disappearing on pressure, without lustre or œdema. Near the centre of the palm, upon careful examination, a soft elastic eminence following the long direction of the hand was felt. Several leech bites were seen in different parts; and a small circular purple spot, about one and-a-half line diameter, but without wound of the cuticle, was observed towards the metacarpal region of the ring-finger, where the pain was first felt. The fingers were retained in the semiflexed position; and any effort to move them was accompanied with extreme pain. The pain extends up the arm to the arm-pit. Pulse 110, full and strong. A puncture by a lancet was made last night without discharging any thing but blood. I instantly made a deep incision through the palm; and dividing the fascia to the extent of an inch and-a-half, about half an ounce of thick purulent matter, not uniform, but consisting of thick lymphous pus, floating in thin fluid, was discharged with a little blood. The relief was instantaneous. But in the course of a few minutes the patient fainted, and was some time in recovering sensation.

A poultice was applied for a day or two; and afterwards, under the use of the resinous ointment, and enlargement of the incision, with proper management of the general health, recovery was slowly effected in about ten or twelve days.

In the following case, which is well marked, and with inflamed lymphatics, resolution was at once effected by incision.

10. J. B., a young man of 23, applied to me in December 1817, for a complaint of the left hand, which had come on very suddenly. The middle finger was the seat of much swelling, tension, constant throbbing pain increased during the night, and redness of the integuments. From the finger a red streak ran up the back of the hand and the anconal surface of the radius, with an abrupt, though irregular and well-defined edge,—varying in breadth at different points, but broadest at the bend of the arm. Above this it becomes narrower, and ultimately terminates in a painful enlarged axillary gland. The lustre of the skin of this red tract is not more than that of the natural skin, rather less; and the redness marks exactly the boundary of pain, heat, and swelling, within which the skin presents a doughy sensation, quite different from that communicated by the sound skin. The motion of the elbow-joint, hand, and finger, is much impaired. The tongue is furred; the pulse 100, sharp and strong.

The finger was first affected with pain on Sunday (five days ago.) Yesterday the red-

ness and pain of the arm came on, and was soon followed by affection of the wrist and gland.

An incision was made freely and deeply by a scalpel; and the wound was allowed to bleed, which it did most copiously. Next day the finger was better; and the second day after, the affection of the absorbents had disappeared. In a few days more the wound was entirely well.

This patient was also ordered to take a dose of a saline purgative.

Several similar cases, when presented in time, I have treated in the same manner, and with the same result. These it is unnecessary to detail, as they all of them present the same characters. The above I have selected as one that is at once well marked, and accompanied by affection of the absorbents. Upon the whole, I conceive it may be established as a correct principle, that in cases of thecal *paronychia* incision should be made at once through the sheath of the synovial cavity; and that this is the only effectual mode of arresting the disease, and of preventing those injurious consequences which too often result from the practice of waiting till suppuration is established.

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From the Edinburgh Medical and Surgical Journal.

**OBSERVATIONS ON THE BEST METHOD OF ACCOMPLISHING DELIVERY,** in *Presentations of the Superior Extremities, where turning is unadvisable or impracticable.* By ROBERT LEE, M. D., Member of the Royal College of Physicians, London, Physician Accoucheur to the British Lying-in Hospital, &c. &c.

The operation of turning in presentations of the superior extremities, though occasionally performed prior to the close of the sixteenth century, was not until that period introduced into midwifery as a general rule of practice. This important improvement, after meeting with much unreasonable opposition, was at last, and without having undergone a rigorous examination, implicitly adopted, and extensively abused. From the writings of the different authors on midwifery of the seventeenth century, it appears that they considered the operation of turning as applicable to all cases without exception of arm presentations, and that to accomplish this object, they had recourse to a degree of violence, which must often have produced fatal contusion, or laceration of the structures of the mother.

Though such unwarrantable force as that employed by La Motte and his contemporaries, in altering the position of the child, is now less frequently exerted by enlightened practitioners, yet there can be no doubt that an equal degree of violence is still occasionally used, and is even inculcated by some of the most distinguished teachers in Europe, as justifiable and necessary, where turning can-

not be more easily effected.\* In this country at the present time, the opinion is very generally entertained, that we must change the position of the child, at whatever hazard, in all cases without exception, of arm presentations.

It is from a conviction that late writers on midwifery have not treated this subject with the degree of attention which its importance demands, and from having witnessed in several instances the fatal effects of injudicious attempts to turn the child in presentations of the superior extremities, that I have been led to a more careful examination of the circumstances which should influence our conduct in these cases, and of the best mode of accomplishing delivery where turning is unavoidable, or absolutely impracticable.

The chief object of turning, as appears to me, is the preservation of the life of the infant; and I must therefore say, that so serious an operation in regard to the mother is not justifiable, in cases where we have such positive proofs of the death of the fœtus, as want of pulsation in the umbilical cord, or the presence of unequivocal signs of putrefaction in the body of the child. I am of opinion also, that the operation is not to be resorted to, where we are certain that the pelvis is so deformed as not to admit of the passage of a living child at the full period, or where other circumstances present in the case render it very improbable that the life of the infant could be preserved, even if it were turned.

When we have recourse to turning, under the most favourable circumstances, as when the membranes are unruptured, the active contractions of the uterus feeble, or altogether absent, and its mouth dilatable, the operation is not even then altogether free from danger to the mother. I have seen it followed by severe and even fatal abdominal inflammation, though performed in the most cautious and dexterous manner.

Turning, then, whatever may be asserted to the contrary by some practitioners, being always more or less hazardous to the mother, it follows that her life should never be put in jeopardy by its adoption, where the child is dead, or where we cannot entertain a well-grounded expectation of saving the child's life by the operation.

But its difficulty and danger are greatly increased in cases where the *liquor amnii* has been evacuated for many hours, where the arm presents, and the uterus is contracting forcibly around the body of the child. I would ask, if, under these circumstances, it be not highly probable that the child's life has been destroyed by the great and long continued pressure of the uterus around its body; and it cannot be denied, that, in a large proportion of such cases, where delivery is effected by turning, the children are still-born? In these cases, the danger resulting from not

interfering, or from our interfering, if turning be the means we employ, is very great; as rupture of the uterus in either case may be the consequence. If this state of violent contraction has existed for several days, as sometimes happens before our attendance is requested, the shoulder and chest have become so deeply and firmly impacted in the pelvis, that it has been found impossible to raise or move the child from its situation, so as to pass up the hand into the uterus to lay hold of the feet. The works of the best writers on midwifery contain cases of this description; and Dr. Denman has admitted, that, prior to the discovery of the process of spontaneous evolution of the fœtus, this impossibility of turning the child had, to the apprehension of writers and practitioners, left the woman without any hope of relief. The nature of this process is now perfectly understood. But I conceive that the interests of both mother and child, where the latter is ascertained to be alive, would be best consulted, by determining to place no reliance on a resource which nature so rarely exerts, and which, when exerted, can only produce the expulsion of a still-born child.

Blood-letting and opium, it must be admitted, have considerable effect in controlling inordinate uterine contraction, and thus facilitate the process of turning. But these means, even when largely employed, too often fail in producing that quiescent state of the uterus, which will enable us to effect our purpose with safety to the mother.

In some cases which I have witnessed, notwithstanding the employment of these remedies, I have found it impossible to pass up the hand beyond the presenting part into the uterus, without exerting a degree of injurious violence.

Wherever, therefore, the valuable life of the mother is in danger, and no good can result from the operation of turning, so far as the child is concerned, I propose to do by art, that which is effected by nature, in cases of spontaneous evolution of the fœtus. I separate the arm from the body, perforate the thorax, and having fixed the crotchet on the pelvis or lower part of the spine, make such a degree of traction as may effect the delivery, without laceration or contusion of the soft parts of the mother.

In the following cases the above method was adopted with the most perfect success:—

On the 15th October, 1824, I was called to visit a patient of the Westminster General Dispensary, residing in Great St. Andrew Street. I found her in the following condition. The membranes had been ruptured fourteen hours, and the *liquor amnii* had entirely escaped. The right arm, much swollen and livid, was protruding out of the external parts, and the shoulder and a part of the thorax were firmly impacted in the pelvis, while the contractions of the uterus were violent and incessant. The pulse was quick, the face flushed, and the soft parts lining the pelvis were hot, dry, and very tender. Thirty ounces of blood were

\* Douglas on Spontaneous Evolution, of the Fœtus.

drawn from the arm, and sixty drops of laudanum administered, before any attempt was made to alter the position of the child. After waiting for half an hour, when the pains had somewhat diminished in violence, I attempted slowly to pass up my hand, but the pains were immediately renewed with redoubled force; and after persevering for upwards of an hour to turn, I was compelled to abandon the intention. Another practitioner then saw her, when other twenty ounces of blood were drawn from the arm, and forty drops of laudanum were administered. He waited some time, in the hope that the actions of the uterus would cease; but this not taking place, he proceeded to endeavour to pass up his hand into the womb. This attempt again excited the most violent bearing down pains; and after long and fruitless exertions, he also was compelled to desist from the threatened danger of rupture of the uterus.

Two hours having elapsed after this second attempt to turn, and the pains still continuing undiminished, I separated the arm from the body at the shoulder joint, laid open the thorax by means of the crotchet, and, passing it through the opening thus made, fixed it on the lower part of the spine; and on dragging down with a steady force, the child passed out of the external parts doubled. Though there was great distention of the parts at the outlet of the pelvis, no laceration of these took place.

The superior aperture of the pelvis having been considerably under the ordinary dimensions, some resistance was offered to the passage of the head; but this was overcome without much difficulty. This patient speedily recovered, and has since been delivered by me of an eight-months child, where the breech presented, and where the life of the child was lost, from the time and force required to bring the head through the confined brim of the pelvis.

On the 1st May, 1827, I was requested to visit Mrs. Kagen, Charles's Street, Drury Lane, also a patient of the Westminster General Dispensary. She had been two days and nights in labour, and was extremely exhausted with fatigue. The left arm much swollen was presenting, and around it a loop of the umbilical cord, which did not pulsate. There was great thirst and restlessness, and the abdomen was tense and very painful on pressure. The pulse was extremely quick. The uterus was contracting with great force, and I found it quite impracticable to pass up the hand, or to push back the presenting part, so firmly was it impacted in the pelvis. Sixteen ounces of blood were drawn from the arm, and an opiate administered at 4 A. M. At 7 o'clock the pains had almost ceased, but were instantly renewed on attempting to turn. The child being dead, I did not persevere long in my efforts to turn, but delivered without much difficulty in the manner already described. Here also there was contraction of the brim of the pelvis, of which a lamentable proof existed in a fistulous opening

between the bladder and vagina, reported to have been caused some years before by a protracted labour, which was terminated by the use of the forceps.

On the 14th May, 1827, I was called to a patient of the same institution, in King Street, Drury Lane. The left arm presented, and the shoulder and thorax were forced deeply into the pelvis. The umbilical cord was hanging without the external parts, and did not pulsate. The contractions of the uterus were strong, and were much increased on attempting to introduce the hand. The delivery was accomplished with the utmost ease, and in a very short time, as already described. The extraction of the child was effected very slowly, to allow of the dilatation of the internal parts, and to prevent laceration of the perinæum. On the second day after delivery, this patient experienced a slight attack of abdominal inflammation, which readily yielded to one copious bleeding and cathartics.

In another case which has since occurred to me, and which, in all essential circumstances, resembled the three cases now detailed, the same method of accomplishing delivery was adopted, and with similar success.

I do not consider it necessary to attempt to define more clearly the cases to which the above practice ought to be applied, as it is hardly possible for any one, after the observations I have made, to misunderstand the object of this communication, or to suppose that the common operation of turning should be abandoned where there is a reasonable hope of saving the child's life, and that of the mother.

The method of effecting delivery above related, I was led to adopt, from reflecting on what takes place in cases of spontaneous evolution of the fœtus; and it may be perceived, that in all the foregoing instances nature had begun, and was striving, though ineffectually, to complete this process.

Since the occurrence of these cases, I have had an opportunity of perusing the essay of Dr. Douglas on this subject, and have been gratified to find that he has recommended the same mode of treatment, and has been also forcibly impressed with the impropriety of turning in all cases of arm presentations. Dr. Sims, in the 40th volume of the Medical and Physical Journal, stated similar views, but did not lay down any specific rule of practice in such embarrassing cases.

Dr. Davis, in his Elements of Operative Midwifery, p. 326, concludes some observations on this subject with the following words. "If, therefore, we suppose the child to be already dead, or the circumstances of the labour to be such as to make it impracticable to bring it into the world alive by means of turning, or even to perform that important operation at all without exposing the mother to extreme danger, it would then, in my opinion, become the unquestionable duty of the practitioner to effect the delivery by embryotomy."

Instead of extracting the child double, he recommends "that it should be divided into

two principal parts, head and body, by passing a properly adapted cutting instrument across, and through the entire structure of the neck;" and he has delineated in his valuable work instruments for this purpose.

Notwithstanding, however, his ingenious invention of Craniotomy forceps, and of the power which they confer upon us of extracting the head, or any other part of the child from the uterus, still I should be disposed to avoid, if possible, the occurrence of the head remaining in the cavity of the uterus after the extraction of the body, as it must be extremely difficult to find it afterwards for perforation, and quite impossible to accommodate it to the diameters of the pelvis in passing.

The difficulty of reaching the neck when the shoulder and thorax are thrust deep into the pelvis, and the head of the child is tilted up over its brim, appeared to me in the preceding cases so great as to be almost insuperable, setting aside the disagreeable process of passing up cutting instruments so high within the uterus.

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From the London Medical and Physical Journal.

**OBSERVATIONS ON FEVER.** Addressed to a Medical Friend, by Dr. Bow, of Alnwick.

(Continued from page 502.)

In this stage, the two constant and prominent symptoms are increased heat of the body and increased action of the sanguiferous system; but whether increased heat of the body precede increased action of the sanguiferous system, or vice versâ, is a point which has not been sufficiently attended to, and that, perhaps, because the practitioner is seldom called on until the fever is fully developed. I may say, however, that it is generally believed and inculcated that increased action of the sanguiferous system is the prior symptom, and also that it is generally believed that this increased vascular action is the cause of the heat. I have been subject to fever resembling ague, and in my own case I can assert that there were sensations of heat on various parts of the body before I could recognise any alteration of pulse; but I must observe that I have had only one paroxysm since it occurred to me to watch the priority of these symptoms, therefore I shall lay no stress on my own case. In various authors we meet with remarks touching this point. Dr. Hamilton, in his observations on the seats and causes of diseases, says that increased heat of the body, or at least of particular parts of it, is sometimes felt independent of increased action of the sanguiferous system. And still more to the purpose is the testimony of Dr. Carmichael Smyth. Whilst this physician was inquiring into the nature of the fever which attacked the Spanish prisoners at Winchester in 1780, he was himself attacked, and, in the description of his own case, he says that, after a long exposure to the contagion, although he had felt no inconveni-

ence within the house (where the sick were,) he afterwards found himself very giddy; with a considerable degree of nausea. These symptoms, however, soon went off: he ate his dinner as usual, and went to bed seemingly in perfect health; but, about the middle of the night, he awoke from sleep with symptoms of the most violent fever, the oppression of the precordia exceeding all description, whilst the sensation of heat gave him the idea of liquid fire spreading from his stomach across his breast, along the course of the pectoral muscle, and down the inside of his arms to the extremities of his fingers. But, notwithstanding these dreadful sensations, his pulse was regular, and the frequency of it by no means in proportion to the degree of heat; but this was not the case in after periods of the disease.

If increased heat be sometimes felt independent of increased action of the sanguiferous system, it is plain that the latter cannot be looked upon as the cause of the former, and therefore we must look for some circumstance, the effect of primary disorder, which must intervene, and act as the immediate cause of excitement.

I think I can trace this cause, and in it I can see nothing but what is natural and simple,—to an effect of impaired energy of the brain, which I purposely omitted before, to save repetition. We look upon the effect of the remote cause of fever to be impaired energy of the brain, and, from the very sudden invasion of fevers in many cases, and from the very sudden effect of certain gases when inhaled, we are led to believe that the remote cause acts immediately on the extremities of the nerves with which it comes in contact. We recognise this impaired energy by the symptoms to which it gives rise, and if slightly impaired energy cause listlessness, dejection of spirits, and muscular debility, it is but reasonable to conclude that the function of secretion must also in some manner be effected: nor is this a gratuitous assumption, for in the cold stage of fever there is evidence enough to prove it. That the secretions in general are deficient in quantity during the cold stage of fever, no one will deny; yet it may be said that as the capillary arteries are less active in that stage, the secretions may be defective in quantity owing to the want of material for their production; but is that which is secreted perfect in quality? The white and clammy state of the tongue shows an altered condition of the mucous products; the loathing of food shows that the gastric juice is not as it ought to be; the appearance of the stools points to an imperfect state of the bile. If a product of secretion in any one part of the body be altered in its nature in consequence of impaired energy of the brain, we may or must infer that the products of secretion throughout the body will also be affected more or less from the same cause. And when we consider that there is no part of the body in which the operation of secretion is not going forward, and that each and all of its products may become

sources of irritation, we cease to wonder that fever should be a disease "affecting the whole system."

In the first stage of fever, the secretions must be regarded as deficient not only in quantity, but also in quality; and these defective secretions again can only be looked on as foreign matter, which after a time must irritate the nervous extremities with which it is in contact, and it is this irritation which, in my humble opinion, determines the hot stage. Diminished sensibility is observed to follow impaired nervous energy; it is therefore reckoned amongst the phenomena of the first stage; and the duration of the first stage is in every instance longer or shorter according to the degree of diminution of sensibility, or the morbid state of the secretions. If sensibility be but little diminished, the cold stage will be of short duration, for in that case the impression from the imperfect secretion is soon conveyed to the sensorium, and the brain being thus roused, nervous influence in greater force is diffused throughout: if, however, sensibility be greatly diminished, then will the cold stage continue until the secretions acquire vitiation enough to irritate. "The sensibility of the skin differs very much in its different parts, but in its general extent it may be considered as possessing the most acute degree of feeling of any of the structures of which the body is composed;"\* and it is on that account that the symptoms of reaction first appear on the surface. It is the acute degree of feeling which the skin possesses in its general extent, which gives the disease the form of fever; for were sensibility confined to a part of small extent, the determination of nervous influence to that part would be in force sufficient to excite inflammation; whereas, owing to the general extent of sensibility, the irritation becomes general, and consequently the determination of nervous influence must be general also. But as hereditary taints, former disease, or intemperate habits, may have impaired the functions of some organs, or even altered the structure of others, we frequently meet with topical inflammations occurring at the onset of the second stage, and which in many cases completely alter the form of the disease. The peculiar state of an organ, whether we regard its structure or function, may render it more liable than other parts to be affected by causes operating upon the system at large: for example, the fluid destined to lubricate the pleura may become sooner or later more completely defective than the secretions of other parts, and, by its irritating nature, cause a determination of nervous influence to that texture, before the imperfectly secreted products of the skin can, by irritation, elicit a determination to the surface; or, though this fluid may not become more defective than other secretions, the pleura, owing to former lesion or other causes, may be more sentient than when perfectly sound, and consequently more ob-

noxious to irritation; in either of which cases inflammation of that membrane will manifest itself at the commencement of or during the three first days of the second stage. On the other hand, a part may be defective in such a manner as to be less sentient than when sound, or than other parts, and which, during the three first days of the second stage, may attract no particular attention; but, when nervous energy in general begins to decline, then will the energy of this part decline in a greater ratio, or, in other words, this part will lose tone sooner than other parts: hence will follow that topical inflammation termed "sub-acute inflammation," being sanguineous congestion owing to want of energy in the part. To illustrate more fully,—there is a state in which the system is rendered highly susceptible of impression from a typhoid miasm, attended with local circumstances which dispose the disorder to assume an inflammatory form: I mean the puerperal state. The excitement attending parturition soon gives way, and is followed by the debility of exhaustion, in which condition the slightest morbid agent is sufficient to involve the system, which but a few hours before might have opposed a causation of tenfold greater force.

The disease is ushered in by a cold fit, during which the secretions are imperfectly produced, and as with puerperals there is one texture, the peritoneum, which may be regarded as in a morbid state, we at once can pronounce the cause of its being the seat of inflammation. The peritoneum, during its extension, must have its vessels and nerves either enlarged or multiplied, or both: therefore, when relaxed, it must for a time be supplied with a greater force of nervous influence and a greater quantity of blood than is necessary: hence it is more sensitive, and the exhalation from its surface more abundant, than necessary. In this condition is the peritoneum more liable to be irritated; but the superabundant secretion from its surface becoming imperfect, in common with the other secretions of the body, acts like foreign matter applied to its whole extent, and involves it in inflammation before the irritation on the other textures of the body can determine general reaction. Indeed, in some cases of the true puerperal fever, where the severity and extent of the peritonitis are great, we may look in vain for general reaction; the determination of nervous influence to the irritated or inflamed surface being so powerful that nothing can divert its course: hence the languid energy in other parts, giving rise to the "pellucid eye, the pale and shrunk countenance, the indescribable expression of anxiety."

Before I leave this part of my subject, let me call your attention to the nature of the heat, or rather the sensations which accompany the heat evolved in fevers. I am sure you will at once agree with me, that it is a heat depending on irritation in the part in which it is felt; and, as a proof of this, it may only be necessary to quote a few of the adjectives employed by authors to denote that it is a heat of

\* Bostock.

no common kind: for instance, we have ardent, pungent, burning, penetrating, and a heat like liquid fire. As the heat in fevers cannot be the effect of increased action of the sanguiferous system, inasmuch as the former has been observed independent of the latter, neither can the inordinate action of the heart be the effect of the heat; for, in like manner, inordinate action of the heart has been felt unaccompanied by heat such as is experienced in fever. When reaction in general is taking place in consequence of irritation from the imperfect secretions throughout the body, and particularly of the skin, there can be little doubt but that the heart will suffer an increase in its action; but I suspect that the inordinate action is attributable to the absorption and commixture with the blood of the imperfect products of secretion.

In this stage (of excitement) commence those symptoms which led to the idea that fever is an effort to expel something hurtful, either ingenerated or introduced from without. Such was the opinion of Hippocrates, of Galen, and of Sydenham; and it is a pity that this hypothesis, which stood the test of so many ages, should have been at length neglected, or that physicians should have been blinded by the ingenious but unsubstantial speculations of later days; for, although the disease does not originate from a morbid change either of the blood or fluids secreted from it, yet a change in the latter does take place in consequence of a prior morbid action; and it is this change, and the action which follows as effects, that give form to the disease, and entitle it to be called Fever.

The doctrine of the Greek school was derived from observation, and it is one which would naturally follow, from "beholding a violent commotion in the system followed by an evacuation from the skin and kidneys, with which the paroxysm terminated." It would seem that one objection to this hypothesis must fall to the ground if we could in any plausible manner explain how morbid matter is generated internally; for Dr. Good says that there are many fevers produced evidently by cold, fear, and other excitements, as well mental as corporeal, in which most certainly there is no morbid matter introduced, and wherein we have no reason to conceive there is any generated internally; while the disease, limited perhaps to a single paroxysm, closes nevertheless with an evacuation from the skin or kidneys.

Now, in such fevers, the first effect of the remote cause is impaired nervous energy; there is a cold stage, during which the secretions are disturbed, whereby we have great reason to believe that imperfect products are formed, which, though not morbid, are morbid and generated internally, and which, being absorbed, are thrown from the system through the skin, kidneys, or bowels. As fevers have been known to have been suddenly cured by a hemorrhage so moderate in quantity as to be incapable of carrying out any considerable portion of matter diffused over the whole mass of

blood, and as Dr. Good cannot conceive how such diffused matter can collect itself at a focal point, or pass off at a single outlet, he considers the hypothesis incorrect. When, however, foreign matter is purposely introduced into the system, we are witnesses to the commotion it excites, and the animal only recovers after some critical discharge. With regard to not conceiving how offending matter can collect itself at a focal point, or pass off at a single outlet, I would ask if it were more preposterous to suppose the blood-vessels endowed with a power of casting from them that which is offensive, than absurd to give to the lacteals the power of selection or rejection; yet no one disputes their possession of such power. That there is a law in the animal economy which determines offending matter to the points irritated, I think cannot be denied, for in our daily practice we behold the effects of its operation; but to explain how it operates may be a difficult task. If, from whatever cause, menstruation be suddenly checked, how often then do we see erysipelatous inflammation follow an incautious exposure to cold; and is not erysipelatous inflammation in every instance caused by a deposition of irritating matter from the blood. It is said that wounds of the pericranium are oftener followed by erysipelas than other wounds, but it does not follow, as is supposed, that erysipelas obtains in consequence of the pericranium being wounded. In most cases, a blow sufficient to wound that membrane is concussive enough also to derange the operations of the brain: hence for a time the secretions are imperfectly produced,—they are absorbed and deposited round the wound, the point irritated. If the concussion be violent, and the operations of the brain for a longer time deranged, then the secretions become so vitiated that, before they are absorbed, they irritate generally, by which reaction is brought about, and the absorbed offending matter is at length gradually discharged from the wound in the form of pus. When concussion proceeds from violence not producing lesion, then is there fear of irritation or inflammation of the brain or its membranes; for, there being no wound or external irritation to which the absorbed offending products can be determined, they are attracted towards the brain, the seat of the injury, and there produce irritation and inflammation: or, as in fevers, it may be thrown off by a critical discharge. Indeed, I very strongly suspect that that form of fever denominated "sympathetic" is also an effort of the system to throw off something hurtful generated internally; or, I would rather say, is produced by offending matter circulating with the blood. For, if irritation from an external cause be continued, nervous influence is directed in particular to that part, and inflammation is the consequence; but whilst so great a force of influence is directed suddenly to one point, other organs or textures are, on that account, proportionally deprived of it, and consequently the secretions of such parts become imperfect, are absorbed, and give rise to the fever. Here there

is offending matter which must be thrown off. If it be thrown off by the bowels, kidneys, or skin, we have that termination of inflammation termed "resolution;" but it oftener happens, where the inflammation is severe, that the offending matter is determined to the inflamed point, and deposited there in the form of pus; hence suppuration ought to be regarded rather as the crisis of the fever, than a termination of inflammation. Is not menstruation itself a strong proof that the system has the power of collecting offending matter at a focal point, or of discharging it at a single outlet; and that this power is not confined to one part, or particular structure, is evident from the fact that, by irritation, the menstrual discharge can be diverted from its natural channel. We have many instances on record wherein it has made its exodus by an ulcer on the leg or arm. If irritation, then, such as is produced by an ulcer on the leg or arm, can withdraw a discharge from its natural channel, need we be surprised that, when offending matter is circulating with the blood, that irritation should determine its outlet?

The increased energy which marks the second stage generally begins to decline on or after the third day of it; after which time (provided there be no local determination of nervous influence, caused by an irritation greater than that which a few leech-bites might create,) I have frequently seen fevers yield, to an hemorrhage caused by two or three leeches applied as near as possible to the same spot; or to the hemorrhage from one bite promoted by a cupping glass. If benefit be therefore derived from the loss of so small a quantity of blood, we must impute the favourable result to the nature, and not the quantity of blood lost. Indeed, the effects of leeches at a certain period in fevers, have often been so favourable as to excite amazement even in the practitioner; and, in some cases, so efficacious has the operation been, as to induce some to believe leeches possessed of endowments beyond drawing blood. If we examine the action of some of our favourite remedies in fever, we may trace the benefit derived from them to their establishing an outlet for offending matter. Cathartics, administered daily during the first stage, not only empty the gut, but, by keeping up increased intestinal action, they secure a drain or outlet for offending matter; and hence the cause why the early administration of cathartics, for the most part, insure a short second stage.

Although I set out with the determination of meddling as little as possible with the opinions of others, I cannot help stating that I think it is the presumed *modus operandi* of cathartics that has led to the idea that fever, in its first stage, is an affection of excitement. The evacuant plan, as it has been called, is supposed to be productive of benefit by clearing the gut, and by lessening the volume of the circulating mass. After a brisk cathartic given in the first stage, the patient may, and often does, feel renovated, while a person in

health, after a similar dose, feels exhausted: hence it is argued, if fever be an affection otherwise than of excitement in its first stage, the patient's feeling, after a cathartic, would be that of increased weakness.\* Impaired energy of the brain from a febrile miasm, does not imply an exhaustion of nervous influence, such as follows great bodily fatigue; the sensibility and energy of the brain are obtunded, by which it cannot sufficiently acknowledge the usual impressions which regulate its operations: requiring, therefore, stronger impressions to rouse it to action, it is in a manner paralysed. The first effect of a cathartic is irritation of the gut, and this impression, being stronger than that produced by the natural irritants of the canal, is acknowledged. The brain being thus roused, nervous influence, though in particular directed to the gut, is diffused throughout: hence the patient feels himself renovated, and it is diffused throughout, because the brain, being roused, becomes again more sensible to the natural impressions throughout the body. It is this feeling of renovation after a purgative in the first stage, conceiving it to be produced by a diminution of the circulating mass, which I think, with great deference to so high authority, led Dr. Armstrong to view fever as an affection of excitement in its first stage, and to consider the nervous appearances, even from the first attack, as only secondary to vascular disorder. That vascular disorder will occasion nervous appearances, cannot be doubted; for we know that the vascular and nervous systems are positively dependent on each other; but that the vascular system should become deranged, unless as an effect of nervous derangement, I cannot believe. I have imputed rigours to sanguiferous congestion; and therefore that nervous symptom I conceive to be secondary of vascular disorder; but I hold that retrocession of blood from the surface and internal accumulation can only happen in consequence of nervous disorder. Dr. Armstrong maintains that the debility in the first stage of typhus is chiefly dependent upon preternatural accumulation of blood in the vessels about the head, heart, liver, and other internal parts, and that this debility is only apparent. But general internal accumulations of blood can only happen in consequence of debility and lessened irritability of the vessels themselves. In the first stage of fever, the state of the surface of the body shows that debility extends to the small vessels of the skin, which, if it were confined to those vessels alone, instead of manifesting itself in an empty state of the vessels, would do so in a state of engorgement; for the heart and larger arteries, continuing in a state of vigour, would propel the blood into them as usual, whilst they wanted the power to forward it. This vascular debility, however, being general, atmospheric pressure determines the internal accumulations; therefore these accumulations, however much they may increase debility, are themselves depending on it.

With regard to the debility of the body being

apparent, it is, in my opinion, as direct as if it were the consequence of exhaustion; for the debility in the first stage of fever depends on nervous influence being withheld; debility from fatigue depending on its being exhausted; the cause of debility in both instances being the want of nervous influence. Yet there is a difference, and a very material one in a practical point of view, and that difference lies in the state of the brain. The debility depending on influence being withheld may be soon removed by whatever rouses the brain into action, while that depending on exhaustion is increased by attempts to rouse the energy of the brain. When general reaction takes place, the internal accumulations are removed, and still the debility continues.

Having imputed the debility to vascular disorder in the first stage, Dr. Armstrong thinks it necessary to blame vascular disorder also for the second stage, although the disorder in the first stage plainly consists of want of excitement in the heart and arteries, and in the second stage of over-excitement. It certainly in both stages depends on the same cause, viz. diminished transmission of nervous influence.

This cause of debility may seem as paradoxical as vascular disorder, since in the first stage nervous influence is evidently withheld, and in the second stage it is as evidently transmitted in greater than natural force. But, though thus transmitted from the brain, it is still withheld from the muscular apparatus; for, owing to the superior sensibility of the skin, the surface of the body is the part first irritated by its imperfectly secreted products, and consequently the determination of influence is to the surface: for the same reason is it withheld from the gut, and hence the constipation which generally accompanies the second stage.

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**ON THE ELEMENTARY NATURE OF ANIMAL STRUCTURES.** By S. D. BROUGHTON, Esq.

"There are more things in Heaven and earth, Horatio, Than are dreamt of in your philosophy."—*Hamlet*.

The revival of the microscope in its application to minute structure, seems to hold out inducements for proceeding with the investigations of animal textures, and to present to our view prospects more promising than those which terminated the labours of Lewenhoeck and Hewson, and emanated from the more recent researches of Sir Everard Home and Mr. Bauer.

The speculations of the mechanical school, derived from microscopic observations, have not tended to the advancement of physiology, and, consequently, the blame has been transferred from the philosophers themselves to the instruments they employed. The demonstrations of lenses, therefore, sunk to the level of popular amusements, and "the wonders

of the microscope," have been almost exclusively devoted to infantile recreation.

Some of the earliest observations on minute structure appear to have originated with Vicq-d'azyr, Hooke, Swammardam, &c. and their accounts of the globular structure of muscular fibrillæ were corroborated subsequently by Mr. Bauer, Messrs. Prevost and Dumas, and others, both in this country and France, excepting some points relating to the forms and dimensions of the globules, and the cylindrical shape and nature of the fibrillæ which they constitute. The mere existence, therefore, of a knowledge of such particles, is not of modern date; and though, perhaps, the hopes and expectations of physiologists have been disappointed, it is to be wished that the subject should not be neglected, more especially when the latest observations to which we can refer place it in a light at once novel, promising, and interesting. It is with this view that I am induced to employ my pen in endeavouring to excite the attention of physiologists to certain hints which may possibly be culled from a more extended and comparative investigation of organic bodies, both solid and fluid, and in the hope that some general principle may be established calculated to illuminate the darkness which obscures the phenomena of organization, and still leaves the conversion of nutriment into animal textures a mystery in need of some more successful clue than has been derived from the otherwise meritorious labours of animal chemistry.

It must not be concealed that microscopic investigations are liable to error, and that optical delusions have occurred, and will ever continue, probably, to falsify, more or less, the evidence of our vision. Constant and repeated observation will, however, tend to diminish the weight of this objection; while the practised eye and master hand in time acquires the tact and management necessary to the use of glasses.

Two general rules appear to be useful in directing the collateral observations of different persons: one, the mutual adoption of similar instruments, and equal focal powers; the other, the precaution of not employing a too highly magnifying power. By the first rule we strengthen the testimony of others, whether corroborative or not of our own; and by the second we are more likely to avoid preternatural distortions and false representations.

Having been of late in the habit of devoting an hour every week to some physiological discourse, before the pupils of the St. George's and St. James's Dispensary, and others, I have had occasion to turn my attention to this subject, though very sceptical as to some of its points. The instrument employed is one of great power and small compass—Gould's pocket microscope, sold at Carey's, in the Strand. The power which I found best calculated for my objects was that which magnifies them four thousand times; the highest power of which the microscope is susceptible being six thousand four hundred.

With this instrument I was enabled to demonstrate, by candle light, the elementary structure of the membranous, muscular, and medullary textures of the body; the globules of the blood, &c.; together with the minute fabric of plants, and the animalculi of vegetable and other infusions.

The points to which I imagine the attention of physiologists may be most usefully directed, belong to the striking similarity between all the elementary particles of animal textures and fluids, and the resemblance of these to the simplest-formed animalculi of various organic solutions. Upon these points we appear to possess evidence in favour of a common identity of primitive organic particles, and some clue to the perpetuity of organization through the nutritive functions. But further investigation is required to sanction ideas apparently so unsupported by facts of sufficient tried strength at present, and we are not yet warranted in making hypothetical positions in any other form perhaps than that of a suggestion, which may lead to more inquiry and reflection.

It has been long known to naturalists, that, under certain circumstances, and in the spring season, watery infusions of all kinds of vegetable matter, and the liquid contents of gutters, ditches, and ponds, rectified spirits of wine, &c. &c. team with life in various forms of organization, of which myriads float about a single drop of the liquor, invisible to the unassisted eye, but when placed in the field of a microscope, they are seen moving to and fro with great rapidity; and these minute beings apparently thread their course through their native element in the full enjoyment of spontaneous motion, and probably proportionate to that which the gigantic forms of the grampus and the squalus maximus may experience in darting through the seas, though the bodies of the former be invisible to human eyes, and their sphere of action limited to a single drop—to them a boundless ocean.

These animalculi have been regularly classed and figured, and placed by Cuvier at the bottom of his classification of known animal species, together with upwards of thirty different kinds of animalculi observed in the seminal fluid of various animals. These accumulated facts are very curious and interesting; but what appears still more so, is the gradation of the animalculi, to which Cuvier gives the name of infusoria, or the products of infusions, from a simple minute globular body, called the monas, to the more complicated forms of the rotiferæ or wheel animalcules. The last and simplest order of the infusoria contains the monades and volvoces. The latter is so called from a peculiar action of turning themselves upon their bodies. The monades and the volvoces are mere apparent vesicles—minute globular forms, similar, to all appearance, to the globules of the blood and of the fibrillæ of muscle. Then, as the scale ascends, comes the proteus, with its ever changing figure; and, by degrees, as in the seminal animalculi, a tail is added to the globular bodies.

Still higher in the scale another order of infusoria is furnished with a mouth, and a more complete digestive organ, &c. till we arrive at that singular being the wheel, so called from its front being provided with an apparatus, the teeth of which resemble a wheel, and are perpetually vibrating with a revolving motion. In some of these higher orders the tail appears to be articulated. The wheels are supposed to be organs of respiration. But it is very remarkable, that in the lower order there is no distinct moving organ visible, although the species enjoy spontaneous motion freely. Some of the infusoria are perfectly globular, some flat, some oval, and others oblong. They all belong to the class of zoophytes.

When chemically examined, these animalculi afford a predominance of animal matter, in the form of gelatin, the ultimate principle of which most prevailing is nitrogen. They not only, therefore, possess the peculiar vital principle of contractility, but are animalised in their composition. No nervous system is to be detected.

If we refer to the modes of their development, we find that these animalcules may be brought into activity from any organic substance, probably whether of vegetation or animal structure, provided absolute dissolution or putrefaction has not taken place; so as to break up the primitive and ultimate texture, and disperse the elements of which its particles are formed. Boiled potatoes, macerated in water, yield them quickly. Rectified spirits of wine exhibits myriads of the simplest kinds, in such active motion as to create a tide in the drop of spirit. They may be produced from dry cantharides and pepper, macerated in water; but no pure water contains them. The water must have been in contact with organic matter before any animalculi appear. There seems to be no kind of vegetable matter whatever which is incapable of yielding animalculi when it is moistened either with vinegar or water. As to animal substances macerated in water, it appears that these have exhibited floating globules in motion also, from recent microscopic observations. Some tact and management seems to be requisite in calling these into a state of activity, and want of success evidently has depended upon ignorance of the proper arrangements necessary to produce the phenomenon. Veal and other animal substances have been stated to furnish globules, moving spontaneously when liberated by maceration. I soaked some hairs from a horse's tail in water, and there very shortly appeared a moving mass of animalculi, similar to the figures of the volvoce and monas: many of these appeared in the latter form exactly resembling the globules of the blood, but exerting spontaneous motion, darting here and there, and changing their direction frequently. The border of a hair exhibited the fimbriated edges (first described, I believe, by Bichat) in a state of active movement, as if teeming with life. Differently-sized globules, some extremely minute, and like the particles

of muscular fibrillæ, were crowded together, and vibrated to and fro in proportion to the room which they enjoyed. Farther patiently conducted observations may probably tend to corroborate the testimony of others as to the independent motion of animal particles, when liberated from their attachments in the primitive organization of different textures.

Dr. Milne Edwards, of Paris, seems to have detected the existence of animalculi in vegetable matter removed from darkness to the solar influence, when the previous whiteness of the substance is converted into green. He states, that he distinctly observed the green matter to be formed of moving globules like the monades. When the matter examined adhered to the glass on which it was laid, from deficient moisture, the particles were stationary; but on adding a drop of water, they showed evident signs of possessing spontaneous motion. This author has carried his microscopic observations to a great extent, and the accounts which he has detailed are marked with a character of considerable veracity, accuracy, and intelligence; and related with all the simplicity and clearness that is calculated to produce confidence in his results. His conclusions are decidedly in favour of the existence of one uniform primitive basis, common to every species of animal texture, in the form of minute globular bodies, invisible to the unassisted eye, but evident under the magnifying powers of a good microscope, and assuming universally a certain determinate arrangement. Thus in the membranous texture minute filaments are seen, forming a net work, and each filament is composed ultimately of globules, in distinct rows, their lines of direction intersecting each other in an apparently confused manner, but upon close observation always assuming similar series of globules in succession. The fibrillæ of muscular fibres are composed of the same kind of globules; but these, instead of assuming bent and intersecting lines of direction, as in the net work of membranes, are formed in straight and parallel rows of globules, separate and apart from each other. In the medullary texture, the same globules appear; but here they form lines less like net work than in the membranes, but departing from the parallelism of the muscular fibrillæ, not so straight as the one, and not so convoluted as the other.

In the three elementary textures of the animal structure, of which all its fabric is composed, the ultimate particles are alike in their original form and size, the individual textures differing only as to the lines of direction in which their globules are placed, but always assuming a definite degree of uniformity, characterising each particular modification of organized texture.

Now, in order to exhibit this ultimate globular arrangement of the three principal textures of the body, a thin slip of either, nearly transparent, and in a state of moisture, should be brought into view upon the field of a microscope magnifying about four thousand times; and when the fibres are pulled asunder

to the utmost extent of their divisibility, the ultimate fibre will be displayed, each (in muscle) lying parallel with the one next to it, but unconnected with it; and in this manner five, six, or more, appear lying alongside of each other, like rows of beads, each bead apparently connected to its fellow on either side by some fatty or gelatinous semi-fluid. According as the substance examined is disturbed by the effort to display its minute structure, the rows of globules will be slightly bent aside, perhaps, or some of them lie across the others, and so on. Also, by maceration, many of the globules are separated from their attachments, and float about the drop of water in which the fibrillæ lie, putting on the exact appearance of the animalculi termed monades.

In this manner Dr. Milne Edwards has examined the cellular and other membranes, tendon, muscle, nerve, brain, skin, gland, and the second coat of the arteries. In the whole of these substances the ultimate particles have been found just as described above—uniform in size and appearance, but formed in differently directed rows, according to the individual species of texture.

In the glandular texture, the ultimate globules were observed to be agglomerated together in a confused mass, to all appearance assuming less definite lines of direction than in the other textures, if any, and constituting the parenchyma of glandular bodies which occupies the cells of their cellular membranous structure, as the interstices of the medullary net work are filled up in the brain and nerves.

Thus Dr. Milne Edwards imagines he has established the demonstration of one elementary, primitive, and organic particle, as the basis of every form of animal structure in common, and, resembling those minute globules observed among vegetable infusions, and classed under the title of *monades*, or the first appearance of life and organization in its simplest form; and the first produced in vegetable and animal infusions, when their texture is loosened or destroyed by maceration, previous to which they appear connected together in single rows, and apart from each other, some bent, some straight, some twisted, others irregular, and others again formed into a net work.

So far a similarity of ultimate structure is shown to belong to the organic elements of all solid parts of animals. As to the fluids of the animal body, the observations of Sir Everard Home and Mr. Bauer have been verified abroad by Dr. Milne Edwards and others.

Some difference has, however, occurred in the individual descriptions of the globules of the blood given from time to time. Jurine calculates their diameter at  $\frac{1}{5240}$  part of an inch, and at another period  $\frac{1}{1940}$ . Mr. Bauer states them to be  $\frac{1}{1700}$ . Dr. Young  $\frac{1}{6060}$ . Dr. Wollaston  $\frac{1}{5000}$ . Captain Kater  $\frac{1}{4000}$ , and subsequently  $\frac{1}{8000}$ . Dr. M. Edwards computes them at  $\frac{1}{7500}$  part of an inch in diameter. Moreover, there is some difference in

their alleged forms, although Dr. Edwards has not perceived any sensible variation among the elementary globules of organic matter, either in aspect or dimensions, but found the animal molecules constantly alike, solid and organized, assuming a constant and determinate form, whether examined in the blood after the removal of their vesicles, in the serum, or solid parts of the body. Dr. Young and others have described the sanguineous globule as having a depression in the centre. Mr. Bauer agrees with Dr. Edwards in thinking them perfect spheres; but the former author found them variable in diameter. Thus, in the coloured globules of the blood he found the diameter greater than in those divested of their colouring matter, as appearing in serum. In the coagula of old aneurismal sacs they measured  $\frac{1}{2800}$  part of an inch; and in the colourless fibrin of inflamed blood, and the lymph poured out from an inflamed surface, the globules measured were of the same size. Serum also, kept closed some time in a stopper bottle, spontaneously produced similar globules.

We have the globules observed in the blood, in pus, and in milk, uniformly the same as Dr. M. Edwards finds, and agreeing with the elementary particles of vegetable and animal textures universally. His examinations have extended to the four great classes of mammalia, birds, fish, and reptiles, wherein the uniformity is traced; but, in the molusca, the sanguineous globules seem to be larger than usual, according to his account, and this is attributed to a nucleus within a vesicle.

The leaves, slips, and stems of plants, exhibit a similar kind of net work to that of animal membrane, their ultimate filaments being formed of rows of globules; and these molecules agreeing with those of animal matter, both solid and fluid. Mr. Bauer, indeed, has imagined that the globules of the solids differ in diameter from those of the blood; but Dr. M. Edwards and others gives a contrary opinion.

M. Dutrochet has made this subject his study, and he has arrived at the same conclusion as those of Dr. M. Edwards, &c. as to the globules, excepting that he seems to have noticed a different ultimate conformation of the filaments of the nerves compared with those of the brain. Dr. M. Edwards supposing no primary difference to exist, the only apparent variation being in the packing together of the fibres, and the directions of the rows of globules of which they are formed. M. Dutrochet, on the contrary, supposes these globules to be larger than those of the brain, and capsular, as in the molusca, and containing a substance within their capsules of a medullary or nervous kind in lesser globules. So, M. Dutrochet considers the principal difference between the minute structure of the brain and nerves to consist in the latter *not being composed of simple rows of globules, but ultimately of diaphanous cylinders, the surfaces of which are studded with globular molecules; and these are represented to be sometimes in contact*

with each other, and in rows; and sometimes separated from each other. And, since he observes them covering the surface of the cylinders without, he concludes that they also exist within, and constitute the medullary substance of the nerves. The object of M. Dutrochet, in thus destroying the harmony insisted on by Dr. M. Edwards, as pervading the primitive structure of animal textures, by supposing the ultimate construction of the brain to be *corpuscular* and that of the nerves *fibrous*, is the introduction of an hypothesis built upon this assumed difference, regarding the brain as the *source* of nervous power, and the nerves as the organs of its transmission through tubes filled with a peculiar fluid, as the transmitting medium; and associating the structure of the brain itself with the agglomerated molecular arrangement in secreting glands, forming that substance termed their parenchyma.

In other respects, M. Dutrochet confirms the existence of rows of globules constituting the elementary structure of animal and vegetable matter; though he considers the corpuscles of the intervertebrated animals as larger than those of the vertebrated, the former containing cells of lesser molecules; whence he concludes that those of the vertebrati are also cellular, and enclose smaller bodies.

It is impossible to view M. Dutrochet's positions without perceiving at once, that his object is to carry his microscopic views into more minute divisions of matter than hitherto observed, and that he does not rest contented with the simple evidence of globules, as detailed by Dr. M. Edwards and others. His hypothesis of muscular contraction rests upon his supposition of still more minute corpuscles than those which accord with the general opinion; and where he apparently deviates from Dr. M. Edwards's system, the facts on which he grounds his reasoning want confirmation, as much as they obviously depart from the simplicity of modern physiology, and that conviction which Dr. M. Edwards's style so forcibly carries with it.

In a more recent paper than that in which Dr. M. Edwards published his last microscopic observations, he observes, that there is less difference between his own and M. Dutrochet's remarks than appears at first sight; and he finds, on carefully reviewing all the circumstances, that in reality they agree in the facts mainly, though they differ in the reasoning and interpretation.

Dr. M. Edwards seems to account satisfactorily for the appearance of the cylinders which M. Dutrochet supposes form the ultimate structure. Having properly prepared some nervous matter, he submitted it to microscopic observation, and apparently detected the cause of the alleged cylindrical structure. Sometimes the globules appeared floating in the liquid, and sometimes as an homogeneous mass, with the aspect of cylinders; and it was difficult to determine at times whether the globules covered some portion of the cylinders or whether they themselves consti-

tuted the intimate structure of the ultimate fibres, as generally supposed.

Dr. M. Edwards digested the substances examined with acetic acid, when their elementary globules were transformed into a gelatinous mass, which he imagines afforded the appearance of diaphanous cylinders to M. Dutrochet. He mingled a little acetic acid with the water in which the portion of nerve examined was dipped, when the nervous matter was rendered transparent; the parts nearest the surface became detached, and globules were seen separating themselves from the cylinders, till by degrees an immense number of them floated in the liquid, the others uniting in little transparent masses, presenting on their edges the aspect of a white jelly, diaphanous, and intermixed with globules.

Dr. M. Edwards states the globules of the nervous fibres to be most clearly evident in the lower animals; but that in the vertebrati, especially long after death, it is difficult to detect them, as the nerves are apt to acquire a transparency, and exhibit homogeneous masses, concealing from view the ultimate globular structure of the fibrillæ, and affording some appearance of cylindrical fibres, detached from the globular molecules.

Thus, it appears, that M. Dutrochet's facts and observations do not so much differ from those of Dr. M. Edwards as one would be disposed to imagine at first sight. He (Dutrochet) acknowledges the organic nature of the globules: he sees them distinctly, but finding some to be vesicular, and containing lesser globules, he reasons that they are all so. Moreover, being apparently deceived by an aspect of cylindrical filaments, forming the primitive basis of nervous matter, he contrives to force his facts to the support of theories, which require him to see farther than others have been able to penetrate through the darkness of that minute world, which is only to be rendered sensible to our sight by the aid of human invention and ingenuity, and even then not unaccompanied with difficulty and error.

It appears, from numerous observations on the blood, that its colouring matter resides in the capsules containing the ultimate molecules, and that the latter are identified with solid organic elements of the common textures. This colouring matter is represented by Mr. Brande to be a peculiar animal substance, acting as a dye to reddish parts, the serous globules being smaller than those of red blood, and colourless; and form, in the opinion of Messrs. Prevost and Dumas, when cohered together in parallel rows, the elementary constituents of fibrin. Whatever little varieties may strike the eye of the microscopic observer, Dr. M. Edwards does not think there exists any appreciable difference between the sanguineous particles, and the globules of which the solid substances of the animal fabric are primitively composed, notwithstanding Mr. Bauer has given a contrary opinion.

Referring to the green globules resembling

*monades*, as mentioned by Dr. M. Edwards to have appeared in the field of the microscope from the influence of the solar rays on white vegetable matter, there seems to be some analogy here to the phenomena of the coloured particles in blood. To this apparent analogy some attention may be perhaps usefully directed, and every circumstance which tends to associate vegetable and animal structure together, in their ultimate organization and elementary bases, will have its due weight in the arguments which may arise from the farther prosecution of this interesting inquiry, now only in its infancy. Mr. Brande refers, in speaking of the blood, to the vegetable colouring matter as a peculiar substance, having affinity with the colouring matter of the blood, since both are capable of uniting with bases applicable to dyes when mingled with certain mordants, by which the colour becomes fixed.

In the present state of this inquiry, Dr. Edwards's observations certainly seem to tend towards satisfactory grounds for believing that the globules composing the different elementary textures of the body, do not sensibly differ one from another in either general aspect or dimensions. He states that he found them constantly alike in all the individual parts of the animal system; as solid, primitive, and organized bodies; of a constant and determinate character;  $\frac{1}{7500}$  part of an inch in diameter, as in the blood; and also, that the inferior developments of animal life agree in all essential characters with the elementary particles of organization in general; and, if the evidence of the microscope is to be credited, the globules forming the minute structure of vegetable and animal textures not only assume the same appearance as the monades derived from the maceration of vegetable and animal substances, but under certain circumstances evince powers of spontaneous motion. By the action of potash, acetic acid, and other reagents, we may, at pleasure, separate the elementary globules of the muscular, cellular, fibrous, and other animal textures, and render them evident to our sight as bearing the closer resemblance to the globules of pus and milk, and those animalculi which are first produced from vegetable and animal infusions. Liquids from the stomach have been, by the addition of acid, seen to separate particles resembling those of the solid textures: electricity, heat, and chemical reagents, coagulated albuminous fluids, and rendered distinct globules, like the colourless molecules of pus, milk, and serum. The serum of the blood, by adding to it hydrochloric acid, or alcohol, and by evaporation, separated immense quantities of globules. The same phenomenon has attended the solution of fibrin in potash, when the globules appeared in every respect to resemble those mechanically displayed in finely dissected muscular fibrillæ.

If all the collateral experience of microscopic observation be gathered together, it seems gradually tending to the support of that generalising, but simple system, adopted by

Dr. M. Edwards; and notwithstanding the apparent dissent of M. Dutrochet from his conclusions, the facts brought forward, (*by Dutrochet*), cannot, in their present form, be considered as shaking Dr. M. Edwards's testimony upon the constant similarity existing among the ultimate particles of organization, however hypothetical the conclusions drawn from his premises may appear to others.

But if Dr. M. Edwards escapes so easily from M. Dutrochet's cylinders, he will probably experience more difficulty with Dr. Hodgkin; for, armed with a microscope of mighty powers, his observations completely turn the current of those now advocated. Dr. H. roundly denies the spherical form of the globules of the blood, and their central nucleus, as insisted on by so many of the old and present physiologists, both in France and England. This author represents them as *flattened transparent cakes, and when viewed singly, nearly, if not quite colourless, with rounded edges, and depressed in the centre on both surfaces; though this depression is said to be so slight as to be unobservable universally*. Such is Dr. Hodgkin's account of the particles in human blood, the diameter of which he estimates at  $\frac{1}{3000}$  part of an inch. As to other kinds of blood, he agrees with Messrs. Prevost and Dumas, that the particles are globular in the mammalia, and elliptical in the other classes. He also perceives some deviations from a general similitude in several particulars among the particles. But the central globules, so clearly pointed out by Mr. Bauer and many others, and on which Dr. M. Edwards sets so much value, as so very important a consideration in the animal economy and the basis of primitive organization, these Dr. Hodgkin entirely disregards, supposing the central nuclei and vesicular coverings to be absolutely non-existent. Sir Everard Home's favourite notion too, of the globules of the blood not coalescing in their entire state, is consigned to the same fate as the circular form of the particles, &c.; for he observes, that it is only in their entire state that they do coalesce.

The ultimate structure of animal textures fares no better than the particles of the blood, in Dr. Hodgkin's hands, for, at "one fell swoop," the labours of his predecessors and cotemporaries are alike destroyed.—*Fibres and striæ*, accordingly, are made to supersede the rows of globules so clearly made out by Dr. Edwards and others as the primitive structure of muscle, membrane, and the nervous substance; when the minutest parts of these are subjected to Dr. Hodgkin's powerful scrutiny—powerful through the medium of glasses, of which it is reported that the celebrated microscope of Professor Amici was incapable of magnifying in a higher degree.

Of the "candour and perseverance" of Dr. Hodgkin, in the prosecution of his researches, we have the written testimony of Dr. Bostock, from personal acquaintance; and I may fairly say as much of Dr. M. Edwards, on the same ground. With the merits of other

labourers in the fertile field of microscopic cultivation, the world is too well acquainted to need any special encomium. Many of them now exist only in their works; but not a few are still able to renew their inquiries. Since all observers but one will be wrong, if Dr. Hodgkin's remarks be correct, we must refer the errors of the former to the instruments they employed, or to the influence of prejudice. But, as to the question of where the truth in all probability may rest, I do not exactly feel the force of Dr. Bostock's insinuation,—that suspicion is most to be directed to that quarter where the observations chime in with all the parts of the speculations entertained; at least when we see these built upon a combination of testimony not widely different in any point of view, and agreeing in all essential points. I am inclined to think, that the contradictory conclusion of a single individual, though highly deserving of attention, are, to say no more of them, to be regarded with equal suspicion; more especially, as in Dr. Hodgkin's case, when the instrument adopted is one of such great magnifying powers, through which nature may possibly be viewed in distorted and aggravated forms—caricatured, indeed, rather than truly and faithfully represented to the eye.

Nevertheless, I cordially join Dr. Bostock in rejoicing that Dr. Hodgkin has taken up this subject; and, professing also with him the deepest interest in the search after truth, I sincerely hope that the embers of physiological inquiry will be rekindled among us, to the farther illumination of this subject; and I trust that the rivalry of the French school of physiology will, in the pursuit of this, as well as its other general objects, be ever fairly met and contested in this country. Then only, perhaps, shall we be able to determine, whether, as Dr. Bostock seems to think, the naturalist has indeed derived benefit from the microscope, while the physiologist has not advanced his science, through the discoveries of this instrument, since the days of Lewenhoeck and Hooke, excepting the simple fact of the existence of globules in the blood and some of its animal fluids, and of the spermatic animalculi.

From the little attention which I have hitherto been enabled to devote to this subject, in the examination of animal textures and fluids, my observations lead me to coincide with Dr. M. Edwards in all essentials. Future experience may possibly induce other conclusions; but whether they militate against, or strengthen Dr. M. Edwards's observations, I hope they may be verified by abler physiologists.

In the meantime, I have been much struck with the constant uniformity of size and general aspect of the ultimate globules of animal texture, in all its varieties, compared with the globules of the blood, which appeared to me obviously demonstrated as represented by Dr. M. Edwards, &c.; and their determinate adjustment seemed to be indicative of their alleged organic character and relations lead-

ing to the assumption of the possibility, when all circumstances above detailed are duly considered, that the process of digestion (*operative solely upon organic substances*) separates the ultimate globules of nutriment, somewhat in a manner analogous to the experiments described with chemical re-agents; and the globules so disentangled from the dissolved and broken up texture in the stomach, may possibly be passed into the blood, there to receive their final assimilation, and the important addition of their colouring matter; and in this transition of organic particles, they may be supposed to be wove by the hand of nature, as it were, into the fibrin of the blood, and thus form the basis of the muscular texture, and in a similar manner become the elementary structure of all the other textures, and of animal fluids in general, throughout the animal fabric.

May not a perpetual renewal and susceptibility of life and organization be so supposed to be provided for?—and the veil which has hitherto concealed from our view the mysterious conversion of food into animal textures and fluids, be thus partly drawn aside, and display the primitive simplicity of nature's works, and the means by which the fiat of the omnipotent Creator, by her intervention, is accomplished?

May not every species of the animal kingdom, from the simple monas to the higher orders, and man himself, be formed upon one such uniform principle, in the primitive arrangement of the elementary textures and fluids, of which the animal structure, in all its multiplied forms, is, in all probability, originally modelled?

Thus every thing susceptible of life may derive all its parts originally from one constant and primitive organic molecule, of an uniform character, spherical and colourless, and more or less developed as the animal may be simply formed or otherwise.

But, perhaps, to treat the subject in this manner at present, it might be said, "Twere to consider too curiously, to consider so;" and certainly premature to indulge in such speculations, while the establishment of the facts on which these probably somewhat romantic ideas are formed, cannot yet be said to be as a house built upon a rock, but rather supported by shifting sands in danger of an overwhelming tide, or some hidden and treacherous quicksand, ever ready to sink the edifice, though it be erected with the utmost care and perseverance of which the frailty of human genius is capable.

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From the *Lancet*.

To the Editor of the *Lancet*.

Sir,—The subject of aneurism, has been so frequently discussed of late, that I fear it will be deemed tiresome, now, either to speak or write concerning it. However, as the following case affords me an opportunity of entering

fully into the consideration of the merits of an operation which, I venture to say, is not duly appreciated, I am induced to think the subject matter, which I am about to submit, not wholly uninteresting.

I am, Sir,

Your Obedient Servant,

JAMES LAMBERT.

Walworth, March 10.

**ANEURISM OF THE ARTERIA INNO-MINATA.**—*Observations on the Propriety of Applying a Ligature to the Right Carotid and Subclavian Arteries, in such Cases.*

*Case.*—W. A., a sailor, of dark complexion, muscular, and about forty years of age, was admitted on the 21st of February, into Henry's Ward, under the care of Mr. Travers.

On the 23d, two days after admission, the following notes were taken: the man's general appearance is healthy; and, according to his own account, he has never suffered from serious indisposition: he has been a resident in warm climates, and his occupation, until lately, of a laborious description. About five months ago, whilst at sea, he experienced pain in the right arm and shoulder, and he was not able to pursue his ordinary employment, with his accustomed vigour; when, on his return home, he fell from a considerable height, alighting upon his feet; he was, in consequence, much shaken, but was not conscious of any thing further, than the mere temporary shock. He remained in the same condition, feeling pain occasionally, until about two months prior to admission, when he began to be affected with severe pains about the clavicle and shoulder, and also extending down the arm: he attributed them to rheumatism, and was rubbing the parts when he accidentally discovered a swelling at the sternal end of the clavicle, which, up to the present period, has gradually increased in size.

There is now a firm pulsating tumour, oval shaped, and of about the size of a pullet's egg; the sternal extremity of the clavicle, apparently lies directly over the centre of the swelling; it is found, however, on handling the part, that pulsation is only to be felt above the bone; the clavicle is partly dislocated. The pulsation in the tumour is visible; it is synchronous with the heart's beat, and with the various pulses of the body, as the carotid of each side, the radial, and others. The pulsations are all remarkably feeble.

Upon applying the ear to the swelling, the impulse of the blood simply, is heard at each pulsation; there is no preternatural sound at any part of the chest, and the action of the heart, as far as can be ascertained, is natural. When questioned, as to whether he was willing to undergo any measures that might be considered necessary, there was immediately a quickened and somewhat agitated motion of the heart, but the man remarked that he was "flurred." Firm pressure on the carotid artery, occasions a slight diminution in the size of the swelling, as well as in the strength

of its pulsation, and under compression of the right carotid and subclavian simultaneously, the effect upon the size and pulsation is very manifest. There is a slight increased redness of the integuments covering the tumour, which, in itself, is moderately firm, but readily compressible. The man has no difficulty of breathing; but, upon taking much exertion, he experiences great uneasiness at the part; his general appearance, as before remarked, is healthy; the countenance is free from anxiety, and devoid of livor.

Mr. Travers having expressed his opinion of the impropriety of any surgical interference with the disease, and simply recommended quietude, the man left the hospital in about a week after admission, expressing his intention of returning, if he should find the disease become aggravated.

*Remarks.*—The nature of the case I have related is, I believe, so decided, that no difference of opinion can exist on the subject, it clearly being a case of aneurism of the *arteria innominata*; that the disease is confined to the latter vessel, and does not emanate from the arch of the aorta, is fairly to be inferred from the absence of all preternatural sound in the chest, as well as from the want of various symptoms attendant upon the latter affection, when so far advanced as to present a large tumour externally.\* It is scarcely worth while to inquire whether the disease in question results from an universal dilatation of the coats of the artery, or whether a rupture has taken place. I do not know of any distinctive characters by which we can ascertain when an aneurism ceases to be one from dilatation. Nor is the question of any practical importance; for, as I have before had occasion to argue, *all* the aneurisms of large vessels are, in the first instance, what are termed aneurisms by dilatation, and this owing to the comparatively greater portion of elasticity in their coats; so that, in fact, the "dilatation" is the incipient stage of aneurism in large vessels, and whenever this general yielding of the coats has taken place, strongly indicative of their morbid and weakened condition, we may expect, and do find, that as they are exposed to the same impulse of blood, the disease will be increased, and rupture ultimately ensue. I have dwelt on this point, because there appears to be a vague opinion afloat, that an "aneurism by dilatation," is a distinct kind of disease, under no circumstances requiring an operation. But even Scarpa, who has been most explicit on the subject, and anxious to uphold a distinction between the dilatation of an artery, and aneurism, informs us, that in the former affection, death ensues from rupture of the diseased vessel. If such be the

end, it surely is idle and foreign to the subject to dispute, whether a yielding of all the coats of an artery should or should not be considered an aneurism. If an operation be applicable in one case, undoubtedly it is in the other, seeing that the termination otherwise is the same in both—fatal.

In the case under consideration, which has been abandoned as hopeless, I conceive the measure of tying the carotid artery, and subsequently the subclavian, (if necessary,) should be had recourse to. It is scarcely requisite here to go over the ground again, in respect to the principle of tying an artery beyond the aneurismal tumour; it is the same as in the ordinary method of applying a ligature between the heart and tumour. It has for some time been acknowledged that a diminution simply, of the impulse of blood entering an aneurism, is sufficient, in many instances, for the purposes of cure; but it was supposed that this principle was not applicable to the cases in which the artery was tied on the distal side of the aneurism. Hence Mr. Wardrop, who has the merit of reviving the latter method of operating, observes, in the detail of his first case, that if a distinct branch arise from the sac, or between it and the part where the main trunk is tied, then it may be necessary to secure the branch separately. However, in the case of aneurism of the lower part of the carotid artery, treated by tying the vessel above the tumour, which I published nearly twelve months since, I adduced some arguments on the propriety of applying a ligature to the carotid artery, in cases of aneurism of the *arteria innominata*. I remarked, that the impetus of the blood would be diminished at the upper part of the sac, the course in which the disease was extending; that a deposition of coagulum would ensue, and that we should "build up the sac" at the point where disease was progressing. The dissection of this case, which proved fatal from ulceration of the artery at the seat of ligature, and hemorrhage by reflux, afforded me an opportunity of confirming the truth of my reasoning, for I found that the curative process had fairly gone on in a tumour situated at the base of the carotid artery, and consequently subjected to a constantly strong impetus of blood. In fact, the post mortem inspection of this case, fully established in my mind the important point, that it is sufficient for the cure of an aneurism, merely to lessen the impulse of blood, whether the ligature be applied to the distal or cardiac side of the tumour; Mr. Wardrop's opinion, therefore, appeared to me to be untenable. It is but right to mention, however, that this gentleman has subsequently taken a similar view of the subject with myself.

I may observe that, at the same time I proposed tying the carotid in cases of aneurism of the *innominata*, I was fully aware that instances of this disease are sometimes met with, in which a closure of the carotid has taken place spontaneously, and yet the tumour has not entirely consolidated, still I considered, that by

\* Mr. A. Burns and Sir A. Cooper speak of aneurism of the aorta projecting at the root of the neck, and resembling aneurism of the root of the carotid artery; but these gentlemen tell us nothing of the phenomena of auscultation in such cases.

taking away nearly half the circulation of the innominata, the coagulating process would, in a majority of cases, go on in the upper part of the sac. If the event which I anticipated, namely, the complete consolidation of the tumour, had not taken place, in my case, after tying the carotid artery, I had fully resolved on applying a ligature to the subclavian where issuing from between the scaleni muscles, by which means I calculated that I should reduce the quantity of blood circulating through the innominata, to the portion which must necessarily pass to the branches arising from the subclavian, on the cardiac side of the ligature. There was a patient in Guy's Hospital, under the care of Mr. Key, in May 1826, affected with aneurism of the innominata, as was proved by subsequent dissection; and, in this instance, I ventured to recommend tying the right carotid and subclavian arteries. The case is reported at page 283, in the tenth volume of THE LANCET. The tumour was formed by a dilatation of all the coats of the artery, and death was apparently induced by suffocation, although the swelling had not attained a great size.

The patient lately under the care of Mr. Travers, I consider is in every respect a fit subject for the operation; he has no disease of the heart or of the aorta; his general health is good, and he is not far advanced in life. One thing is certain, that he is affected with a disease which will inevitably prove fatal unless some active means be used to arrest its progress; are we then justified in considering such a case utterly hopeless? The proposed operation certainly is novel, and barring a solitary instance in which I do not think it was admissible, is untried; it is not, however, a blind experiment, but as I have endeavoured to show, is founded upon an admitted principle in the pathology of aneurism; namely, that a mere *diminution* of the impetus of the blood entering an aneurism is sufficient for the cure. Of the precise quantity of motion under which the coagulating process will go on in the aneurismal sac, we are not aware; we are only acquainted with the fact generally. But, if it be argued that after the application of a ligature to the right carotid and subclavian arteries, the circulation would go on through the innominata, in order to supply the vertebral and other branches on the cardiac side of the ligature, and that the curative process would thus be interrupted; even admitting, I say, that such would be the case, (but this we yet have to learn,) then I would recommend applying the ligature to the subclavian, on the tracheal side of the scaleni. The arteria innominata would then be placed precisely in the same condition as the lower part of the carotid artery, when a ligature is placed beyond a tumour situated at the base of that vessel; and if the regular curative process, as was fully shown by my case, takes place in one instance so near to the centre of circulation, it would, *cæteris paribus*, occur in the other. It must, however, be acknowledged that the operation of tying the subclavian

artery, before it reaches the scaleni, is especially hazardous, from the circumstance of its being closely surrounded by so many important parts; but I am of opinion, that securing this vessel in the ordinary situation will be found fully adequate to the purpose mentioned. *Ad extremos morbos, extrema remedia*, is a maxim in surgery applicable now as at the time it was delivered; and surely it is better that we employ an extreme remedy, or even a doubtful remedy, than that we should abandon patients to inevitable death.

From the Lancet.

#### ENLARGEMENT OF THE LIVER AND SPLEEN, with some remarkable appearances of the Blood.

Richard Marshall, æt. 27, was admitted into Henry's Ward, Oct. 4, 1827, under the care of Mr. Lawrence. This patient, who is a blacksmith, and a man of spare habit, has led a temperate life, and till twelve months since, has enjoyed good health. About this time, he was kicked by a horse in the left side of the abdomen; it was followed by excruciating pain, and an unusual fullness was observed near the navel on the right side, a few days afterwards; he also noticed blood in his stools. He was obliged to keep his bed for a week, was bled, and took some medicines. Since that time his health has been much disturbed, and his bowels have been very irregular. The tumour has continued to increase in size. Six weeks ago, he was again struck in the abdomen; this gave him so much pain, that he was obliged to leave off his employment. He voided a considerable quantity of coagulated blood per anum, and a tumour appeared in the left side of the abdomen, and near the navel. Leeches were applied, and he took medicine, which afforded temporary relief. He has continued to void blood, and the pain and swelling of the tumour on the left side have much increased.

October 4.—The abdomen is generally swollen in a slight degree, as if from incipient ascites. A tumour, having a regular surface, hard, and a little painful, is felt in the left side of the abdomen, immediately under the parietes; it extends from the margin of the chest, under which it is continued into the left hypochondrium, to the anterior superior spinous process of the ilium, and occupies the left half of the epigastric region, and the whole of the left lumbar and iliac regions. The bowels are very irregular; faces of a dark colour, and mixed with blood; urine scanty, and high coloured; pulse weak, and frequent. Loss of flesh is apparent in the limbs. Five grains of pil. hydrarg. every night. Eighteen leeches over the tumour.

15.—The pain in the tumour is less, but its size is not diminished. The bowels are very relaxed, and painful on pressure; tongue coated; mouth affected by the pills: discontinue the pills. An effervescing draught, with laudanum, every six hours.

26.—The pain in the tumour is increased,

in other respects he is much the same. Two days ago a tooth was extracted, which was followed by profuse hemorrhage; about a chamber-potful of blood was lost, and it was necessary to plug the socket.

30.—During the last two or three days, he has been much worse. The bowels are more irritable; the secretions dark and foetid. The tumour is more painful. He has pain in the head, with a dry and coated tongue, and frequent pulse.

Mist. camph.  $\mathfrak{z}\text{i}$ .; liq. ant. tart.  $\mathfrak{z}\text{ss}$ .; tinct. camph. comp.  $\mathfrak{z}\text{ss}$ . every six hours.

In the evening he fell sick, and vomited. The pulse was weaker, and countenance changed.

31.—The patient died this morning.

*Examination of the body nine hours after death.*—On laying open the cavity of the abdomen, the spleen was found enormously enlarged, and occupied the left hypochondriac, lumbar, and iliac regions. Its coats, both on the convex and concave surfaces, were at several points converted into cartilage. The structure of the organ, when cut into, resembled in colour and consistence that of the healthy liver. The splenic artery was of its natural size. The liver was at least twice its natural size; the enlargement being observable more particularly in the right lobe. It had pushed up the diaphragm, increasing considerably on the right cavity of the chest; and it extended downwards four or five inches below the cartilages of the ribs. It was rather light coloured, and of natural consistence. There were disseminated through it universally, as thick as they could be arranged, minute dots, none of them larger than a pin's head, consisting of a soft yellow substance. About half a pint of thick sero-purulent fluid, of a dark yellow colour, was found in the right cavity of the chest. The pleura-costalis was a little thickened and reddened. On the upper surface of the superior lobe of the right lung, was seen a patch of yellow lymph, and the serous membrane covering the lung was slightly thickened and opaque; the lung was compressed by the enlarged liver, and contained but little air. About a spoonful of fluid, of a light yellow colour, was found in the left cavity of the chest. All the four cavities of the heart and adjacent large vessels, were enormously distended with blood, so that the pericardium was quite tense, and could hardly be opened without wounding the heart. This blood, as well as that contained in other parts of the venous and arterial systems, had a very remarkable appearance. It was all coagulated, but the coagulum was of loose texture. It consisted in about equal proportions, of red coagulum and of fibrine, free from red particles; and these were intermixed and adherent to each other as usual. The separate fibrine was soft, opaque, and of a bright light yellow lemon colour: the colour and consistence were much like those of baked custard. The red coagulum was much softer and looser; some portions of it were like the ordinary appearance of the blood after death, but the greater

part was of a chocolate brown, and rather dingy colour, with a broken down appearance, as if the whole had been beaten up with a stick or fork. Much of the yellow coagulum was found in the cavities of the heart and aorta. The vessels of the brain were gorged with blood, which had exactly the same appearance as that found in the heart; hence some of the large veins seemed as if filled with pus. The substance of the brain was unusually vascular.

A note, which Mr. Lawrence received from Dr. Prout, contained the following remarks on the blood of this patient: "I could not make out to my satisfaction the nature of the acid in the blood, but ascertained that it was neither the muriatic, nor the phosphoric, nor any other *fixed* acid. The custard-looking substance in which the acid property chiefly resided, possessed most of the properties of albumen, of which principle it was undoubtedly a modification. The blood, in all the innumerable instances in which I have before examined it, has been either quite neutral, or most usually a little inclined to alkalescence, and I have never before even heard of *acid* blood, though it may, unknown to me, have been noticed as a white looking substance, very like that in the present specimen, has been described, I understand, as occurring in the blood in diseases of the spleen and liver."

From the Medico-Chirurgical Review.

#### REMARKABLE DISEASE OF THE MU- COUS MEMBRANE OF THE BLAD- DER. By M. LOUIS.

This distinguished pathologist remarks that, of all other mucous membranes, that of the bladder is the least frequently found diseased. This is the more to be wondered at, when we consider the great variety of alterations to which the urine is subject, from diseases and from diet. M. Louis examined the mucous membrane of the bladder in 500 subjects, dead of various diseases, and found six instances where this surface was affected with simple redness or injection of vessels, but without any softening of structure. In two or three other cases, there were softening and other species of organic lesion. Two cases we shall present in an abridged form to our readers.

*Case 1.* A man, aged 77 years, was brought to the hospital, on the 23d March, 1827, in a desperate condition. It was learnt that, lately, he had been obliged to make water very frequently, but without pain. Two or three times he passed blood in his urine in considerable quantities—and, for a fortnight before he entered the hospital, his water contained more or less blood. He died the next night, presenting gangrene of the lower extremities, &c.

Passing over the morbid appearances in other parts of the body, we find that the bladder was contracted to about the size of a man's

fist, and was slightly prominent above the pubes, being adherent to the transverse arch of the colon by a membranous cord, of about two inches in length. It contained three or four ounces of dark-coloured, purulent liquid. The internal surface of this receptacle was red, and presented an adventitious tissue, better than a line in thickness, and of a filamentous structure, but quite soft and easily lacerable. The urethra was perfectly sound. M. Louis has not been able to find a case of morbid anatomy of the bladder, precisely similar to the above, in any of our pathological writers. There can be no doubt, however, that it was a product of inflammation—and, moreover, that it was rather a morbid development of the mucous membrane itself than a new or adventitious structure formed by disease.

This patient died of a rupture of the left auricle of the heart, within the pericardium—an extremely rare accident.

*Case 2.* A female, aged 45 years, was received into La Charité, on the 15 March, 1827, stating that she had been ill about eight months. In the beginning of that period, she had some uterine discharges, accompanied by pains in the hypogastrium and loins, which she characterized as lancinating and tearing, and with hardly any remissions. The right thigh had lately become the seat of a disagreeable sensation of formication. The urine was passed very frequently, and in small quantities at a time, during the preceding four months. The bowels were constipated—the disease had been left to itself. On examination, there was perceived a swelling in the hypogastrium, rising a couple of inches above the pubes, and accompanied by the most acute sensibility on pressure. There was some inconsiderable discharge from the vagina. No fever; but the vital powers were evidently sinking. She lingered out till the 28th of April, when she died. Latterly she had been affected with diarrhœa.

*Dissection.* Omitting the notice of some unimportant lesions in the stomach and bowels, we come to the urinary organs. The kidneys were pale, and not more than half their natural size, and yet their pelves and infundibula were very much enlarged. The lining membrane of these last parts, as well as that of the ureters, was thrice its natural thickness. The bladder was very small. The internal surface presented a strange medley of morbid productions, which it would be very difficult to describe. There were three layers, as it were, of diseased growths—one appeared to consist of pyriform vesicles, demi-transparent, containing a clear, but yellowish fluid. These were mixed with, or joined to, another set of bodies, bearing more the character of tubercular bodies. The mucous membrane, and the submucous tissue, was in a state of great disease. The uterus presented several scirrhous masses growing about its cervix and body.—*Repertoire.*

The above are curious specimens of morbid anatomy.

We have said that rupture of the left auricle of the heart, within the pericardium, is a rare accident. We lately saw, in the possession of Dr. Somerville, jun. a fine specimen of aneurismal pouch, of aortic origin, but within the pericardium—that is, behind the semilunar valves. It had attained the size of a large goose-egg—and, ultimately, burst into the cavity of the pericardium, causing immediate death, of course.

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From the Medico-Chirurgical Review.

#### ON DISEASES OF THE FŒTUS, *with the Means of Prevention and Cure.* By M. HUFELAND.

The venerable author of this paper has carried his philanthropy beyond the boundaries of this life, as commonly, but erroneously computed from the day of birth, and directed his researches to the connexion of the fœtus with the mother—to the influence of maternal agents on the embryo—to the results of these agencies—and to the means of counteracting noxious by salutary agents.

Whatever may be the evils of a redundant population in this country, it must always be the business of the medical profession to encourage the increase of births over deaths—and, consequently, to protect the fœtus in utero, as much as the independent being at any other epoch of its existence. At first sight it appears somewhat absurd, as well as impossible, to extend the aid of medical art to the embryo in the womb; but this is a false view of the case. Although the fœtus swims in a liquid, and is consequently secured, in a great measure, from external succussions—and although neither nerves nor blood-vessels can be traced from the mother into the fœtal placenta; yet, it is evident that the materials of organization in the little parasite must be derived from the nidus in which it resides, while facts are not wanting to prove that nervous agency or influence may be conducted from the mother to the child, whatever may be the medium or the mode in which it is conducted. Thus, sudden frights have destroyed instantaneously the life of the intra-uterine fœtus. Some experiments made on impregnated animals prove that substances introduced into the blood of the mother will find its way into the veins of the fœtus. Thus, oil was injected into the veins of a bitch with whelp, and, after a certain time, the animal was killed, when oil was detected in the umbilical veins of the fœtus. When the fœtus is detached, and put to the breast of the mother, we find that medicinal substances will act through the medium of the milk, with nearly as much certainty as if given directly to the infant. The itch, in a child at the breast, has been cured by sulphur exhibited to the mother. Hufeland maintains, that nervous and moral affections may be readily impressed on the suckling through the medium of the nurse. We cannot deny that the nutrition

of the fœtus is equally derived from maternal sources, while intra uterum, and during lactation.

That the agency of heat, cold, electricity, magnetism, &c. may be extended from mother to offspring is undeniable. M. Hufeland thinks that metastasis also may play some part in affections of the fœtus in utero. There can, at least, be no doubt that the syphilitic virus may be communicated to the germ before it has assumed an independent existence.

From these and many other considerations, M. Hufeland thinks we are authorized to infer, that we can act on the fœtus in utero in seven different ways:—namely, by the augmentation or diminution of nourishment—by the augmentation or diminution of the afflux of blood to the uterine system—by changing the qualities of the air and aliment of the mother—by mechanical means—by the agency of electricity, &c.—by medicines—and, lastly, by moral influences.

Let us now pass on to the diseases of the fœtal life. The first class comprehends monstrosities, deformities, &c. which our author thinks must proceed either from hereditary disposition or some defect in the primary development of the fœtus, through causes acting on the mother. This, however, is a knotty point to unravel. In the second class, he ranks the feeble state of vital powers, the effect of defective nutrition, and the atrophy of infants when first born. We see infants come into the world extremely small, feeble, and emaciated. The causes are to be sought in diseases of the mother, as immoderate evacuations, fevers, accidents, want, anxiety of mind, &c. In the third class he places hypertrophy, or excess of nutrition in the fœtus, whether of certain parts, or of the whole, thus rendering the birth difficult or dangerous.

The fourth class comprehends the dyscrasiæ. M. Hufeland contends that, as the pabulum of nutrition in the fœtus must be supplied by the mother, so all vices in the blood, the secretions, and the fluids generally, must affect, more or less, the fœtus in utero. Hence we see scrophulous mothers bring forth scrophulous children, &c. There is little doubt now entertained, that the syphilitic virus can be communicated to the intra-uterine progeny—and if this be the case, we cannot wonder that other maladies should be also transmitted. But not only are diseases thus communicated from mother to child, but death itself may be induced in the latter from morbid impressions on the former. A fright—a strong moral affliction, and many other influences, have been followed quickly by cessation of all motion in the fœtus, and ultimate abortion.

A contemplation of these causes of detriment, disease, or destruction of the child, will very readily suggest the prevention, as well as the remedy—where a remedy can be applied. The whole secret, in fact, rests upon abstracting all causes of ill health from the mother—or, if ill health has been actually induced, to

restore her, as soon as possible, to health. It would be well for mothers and accoucheurs to reflect on these things, and preserve, if possible, the innocent fœtus from the effects of irregularities and intemperance too often indulged in, from a false supposition among nurses, that pregnant females cannot live too generously.—*Journ. Complément.*

From the Medico-Chirurgical Review.

# URTICARIA TUBEROSA INTERMITTENS. By Dr. CAZENAVE.

Daubenton, aged 33 years, entered the St. Louis Hospital, on the 2d December, 1825, and was discharged on the 1st November, 1826, after a sojourn of 333 days. Four years previously he caught the itch, of which he was cured by the usual means. After taking a warm bath, at the completion of the treatment, he was seized with a violent rigour succeeded by intense heat, and ultimately profuse perspiration. At the same time, the left elbow and left knee swelled, and presented a number of irregular nodosities, of vivid red colour, and accompanied by insupportable pricking heat. This paroxysm (for so it deserves to be called) lasted eight hours, and then ceased, leaving the parts above-mentioned covered with dark-coloured patches, that disappeared under pressure. On the succeeding evening, at the same hour, Daubenton was seized with a similar paroxysm in all respects, which lasted six hours. The swellings and nodosities in this paroxysm were on the opposite side of the body. From this time, he was regularly seized, every day, with an attack similar to the first, except that almost every part of the body became, in succession, the seat of the swelling, irritation, and eruption. Ulcers formed on the ankles, and he was soon confined to bed, in a most miserable condition. At the end of a fortnight he was carried to the Hôpital de Beauvais, the paroxysms having now augmented in duration and intensity. The tumefaction of the lower extremities was now permanent and enormous, the skin being almost black. Venesection, blisters, sulphureous baths, were employed in this hospital, by which the complaint was much mitigated; but, as soon as he began to move about the wards, he relapsed as bad as ever. After four months residence in hospital, he went out in the above-mentioned condition. He came to Paris, and, after a few months, repaired to the Hôpital St. Antoine, where he stayed nine months, undergoing a variety of treatment, with the effect of reducing the disease from a quotidian to a tertian, and sometimes a quartan form. This amelioration was attributable to bark, which he took in powder. He went out of hospital in this state, and contracted a syphilitic complaint, of which he was cured in the Hôpital des Veneriens, the original malady still continuing, though considerably mitigated. The unhappy Daubenton now supported a miserable existence, by working during the early

part of the day, betaking himself to bed every evening, under a paroxysm of the pristine affection. At length he presented himself at the St. Louis, and there the periodical accessions of the disease, as already described, were unequivocally ascertained. The paroxysms generally lasted four or five hours, accompanied by the swelling and eruption above-mentioned. On minute examination, during the remissions, no organ of the body appeared to be disordered in function or structure. Having now again become affected with itch, he went through the usual regime for that malady, and was cured of it. The sulphate of quinine was then administered, in doses of eight grains per diem. The paroxysms were stopped by this remedy, and he remained eight days free from complaint. He procured wine secretly—committed a debauch—and again the original malady returned. This relapse was accompanied by inflammatory symptoms, which obliged them to omit the quinine, and prescribe antiphlogistics for a fortnight. Again the quinine was administered, and, in twelve days, the paroxysms were stopped. The intermission lasted a month, when the disease returned, without any appreciable cause; but in a much milder form. M. Biett now determined to employ arsenic; and Fowler's solution was administered, at first in doses of five drops per diem. The paroxysms ceased after the fifth day of this treatment. The remedy was obliged to be suspended for eleven days, on account of some enteric affection, but the original malady did not appear in that interval. The solution was then recommenced, and continued for 26 days, at six drops the dose. The malady returned no more, and the patient was retained six weeks in the hospital, after cure, in order to be sure that he was free from complaint.—*Bibliothèque.*

The above we conceive to be a very interesting case, and certainly it does credit to the arsenic, which, in this, as in many other periodical affections, is even superior to the quinine.

From the Edinburgh Medical and Surgical Journal.

**OBSERVATIONS RELATIVE TO THE QUESTIONS, "Is the Fibrin of the blood of lower specific gravity than even the Serum? Is the Blood which has a Buffy Coat after coagulation of lower specific gravity than Healthy Blood; and does it contain a larger proportion of Fibrin?"** By JOHN DAVY, M. D., F.R.S. Physician to the Army.

Dr. Scudamore, in his "Essay on the Blood," has answered all these questions in the affirmative. I shall take the liberty of considering them separately, and the propriety of the conclusions at which he has arrived.

*1st. Of the specific gravity of the fibrin of the blood.*

It seems to be a very natural conclusion to make, that the fibrin of the blood is the light-

est ingredient of the blood, since it is the principal material of the buffy coat which often appears on the surface of the blood of persons labouring under inflammatory diseases: and that it is so, Dr. Scudamore positively asserts. Thus, at page 35 of his essay, he says "Fibrin is lighter than the red particles, and also lighter than serum."

Yet, notwithstanding the appearance in favour of this assertion, the fact is otherwise;—fibrin is really denser than serum. The phenomenon of the buffy coat at the surface is deceptive, as is most easily proved by the simplest experiment. Thus, pour off the serum of the sily blood, and cut off a portion of the buffy coat, and throw it into the serum; it will not swim, but invariably sink, if not buoyed up by air-bubbles.

When the nature of the crassamentum is considered, and the manner in which it forms, the apparent anomaly of the buffy coat being at the surface, though of greater specific gravity, will vanish.

In sily blood, the serum and coagulable lymph are of greater tenuity, or less viscid, than in blood that is not sily; and sily blood is generally slower in coagulating than blood that is not sily, though not universally so. When the former quality alone prevails, the buffy coat which forms is thin; when both qualities co-exist, the buffy coat is thick, from the greater depth of subsidence of the heavier red particles. Now, though the fibrin of the buffy coat is of greater specific gravity than serum, the *liquid* mixture of coagulable lymph and of serum is of less specific gravity than the liquid mixture of red particles, serum, and coagulable lymph, and in consequence the former becomes supernatant. This is not matter of conjecture; it is an almost obvious truth; and it may be proved to demonstration by a simple experiment. Thus, let the fibrin of the crassamentum be extracted by pressure in a linen bag, without the aid of water, and a liquid mixture of red particles and of serum will be obtained, on which fibrin floats, and the specific gravity of which I have found to vary from 1.070 to 1.075; and by the use of blotting-paper, to remove as much adhering serum as possible, it has been raised to 1.087.\* Before coagulation takes place, it is this comparatively dense fluid which supports the liquid fibrin,—and when coagulation has occurred, the substratum of red particles enveloped in fibrin still gives support; and the longer the coagulation is in

\* This probably is very nearly the specific gravity of the red particles. After as much serum as possible has been abstracted by blotting-paper, the residue stains and penetrates the filtering paper, but not so as to drop from it. It is of the consistence of molasses; fluid to a certain extent, but not liquid,—a loose mass of spherical particles like a quicksand, or a bank of volcanic dust,—and it may help to afford some idea of that mysterious state of lava imagined by Dolomieu.

ing place, so much the less fibrin is mixed with the red particles, so much the more rises, and, *cæteris paribus*, so much the thicker the buffy coat becomes. In healthy blood, on the contrary, in which the serum and coagulable lymph are more viscid, the red particles are supported till coagulation takes place, and then they are retained like water in a sponge, and like water may be expressed from the sponge-like texture; or, if in excess, a part of them will separate and fall down, as the fibrin contracts.

I have made many trials of the specific gravity of the fibrin of blood, and I have found it vary, (accordingly as it was more or less contracted,) from 1.046 to 1.057, and even 1.060. The first was the specific gravity of fibrin of healthy blood, or at least of blood that did not exhibit a buffy coat; the second was that of sily blood, found in the right auricle of a man who had died of an acute disease, constituting what was formerly called a polypus; and the third was the specific gravity of fibrin of the buffy coat. Here, too, I find myself at variance with Dr. Scudamore, who asserts that the fibrin of healthy blood is denser than that of sily blood. If by density he means specific gravity, the facts I have mentioned are in opposition to his opinion. If he means any other quality by the expression, I must confess I do not comprehend his meaning.

His notion, that fibrin is lighter than serum, and the lightest ingredient of the blood, has led him into many mistakes, and given rise to many contradictions and inconsistencies in his essay, in which it would be equally useless to particularize, and irksome to criticise.

### 2dly. Of the comparative specific gravity of sily blood.

In looking over the experimental part of Dr. Scudamore's essay, I cannot find a single instance given of the specific gravity of sily blood having been ascertained. He has come to the conclusion, however, that "blood which gives the fibrinous coat in a great degree, has a lower specific gravity than healthy blood;" but he presently adds, "that moderately sily blood, drawn from a person before he becomes weakened, has not commonly a very low specific gravity."

The results of my experiments would seem to indicate, that there is no necessary, that is to say, no constant connexion between the specific gravity of the blood and the presence or absence of the buffy coat. I have made experiments on the subject. At present I can refer to notes of eleven only. In five instances in which the buffy coat was slight, the specific gravities were, 1.047, 1.051, 1.054, 1.055, and 1.054. In five instances in which the buffy coat was moderately thick, the specific gravities were 1.044, 1.038, 1.052, and 1.056; and in one instance in which it was thick, the specific gravity was 1.057.

It may be laid down, I believe, as a general rule, that the blood of persons labouring under acute disease differs in density very little from healthy blood, and is of comparatively

high specific gravity, whether buffed or not; whilst, on the contrary, the blood of persons labouring under chronic disease, attended with debility and vomiting, is comparatively dilute and of low specific gravity: and this, though most frequently associated with the property of having a buffy coat, is not necessarily so.

Nor do I believe that the time required for coagulation is anywise constantly connected with the specific gravity, as Dr. Scudamore asserts, who says, "blood possessing the highest specific gravity coagulates the most quickly," and "that which gives the fibrinous coat in a great degree is slowest in coagulating." The blood, the specific gravity of which I found to be so slow as 1.038, coagulated rapidly. On the contrary, I have known blood which differed but little from healthy blood, and which did not exhibit a buffy coat, remain liquid eight minutes after having been drawn, and not till the tenth minute was its incipient coagulation distinct. When the serum and coagulable lymph are not viscid, and, in consequence, not capable of supporting the red particles, these particles fall down rapidly; if the fluidity of the blood is great, two minutes are sufficient for their subsidence an eighth or even a quarter of an inch, as I have many times witnessed.

No doubt very buffy blood is very slow in coagulating, for the red globules cannot sink far in a very short time; and it is well known that healthy blood, which is of comparatively high specific gravity, coagulates rapidly. I wish merely to be understood to maintain, that the qualities in question are rather accidentally than necessarily connected, and that the general rule on which Dr. Scudamore insists is not founded on a basis sufficiently broad.

### 3dly. Of the proportion of fibrin in buffy blood.

Dr. Scudamore conceives that he has demonstrated by his experiments, that sily blood contains a much larger proportion of fibrin than healthy blood, and that the proportion of fibrin is variable in different cups of blood filled at the same blood-letting.

The majority of his experiments on this subject I am under the necessity of remarking are far from satisfactory, in consequence of the proportion of fibrin not having been estimated for the blood, but for the *crassamentum*. As sily blood commonly affords a contracted crassamentum, in which the fibrin is comparatively condensed, and in which the residual proportion of serum is comparatively small, it follows, that a definite quantity of sily crassamentum *must* contain more fibrin than the same quantity of healthy crassamentum, and yet the healthy blood *may* contain more fibrin than the sily blood.

In seven instances in which he notices the proportions of serum, and crassamentum, and fibrin, five were examples of healthy blood; and in these examples the largest proportion of fibrin per cent. in the blood was .27, the smallest .15, and the mean about .20. Of the two other instances one was of blood, "with

a slight fibrinous coat," from a man having a slight pleurisy, which contained .36 per cent. dry fibrin. The other was an instance of blood with a fibrinous coat from a man ill of fever, which contained .26 per cent. of fibrin, a proportion not quite so great as the maximum in healthy blood. And soon after, in comparing arterial and venous blood, he states the proportion of fibrin in each of these as the same, and as high as .56 per cent. As he does not mention either of them being buffy, it may be taken for granted that neither of them exhibited this quality.

Nor does it appear to me that he has proved in a manner more satisfactory the other proposition, that the proportion of fibrin is variable, and sometimes greatly so in blood flowing in a continuous stream. His experiments relative to this point are deficient in proof, because he has not made the blood the subject of them, but the crassamentum, and has estimated the quantity of fibrin not for the former, but the latter; and the error to which this mode of proceeding is liable I have already pointed out.

Nor are the results of my own experiments in favour of the accuracy of these conclusions of Dr. Scudamore, in the unlimited and strong sense in which he emphatically gives them.

The following table exhibits the results which I have by me on this question.

	Sp. Grav.	Dry Fibrin per cent.
1. Healthy blood,	1.052	.15
2. { 1st portion, slightly buffed,	1.054	.37
{ 2d, after loss of 24 oz. do.	1.053	.34
3. { 1st, pretty strongly buffed,	1.044	.47
{ 2d, after 24 oz. less buffed,	1.042	.37
4. { 1st, slightly buffed,	1.055	.13
{ 2d, after 2 1-2 lbs. do.	1.054	.13
5. { 1st, slightly buffed,	1.054	.17
{ 2d, after 24 oz. less buffed,	1.052	.16
6. Buffy coat thick,	1.038	.34
7. { 1st, moderately buffed,	1.058	.36
{ 2d, after 28 oz. strongly buffed,	1.057	.40
8. Slightly buffed,	1.056	.40
9. Moderately buffed,	1.052	.39
10. Slightly buffed,	1.051	.41
11. Do. buffed,	1.047	.36

In each instance, after the red particles were separated by pressure almost entirely in a linen bag, the residual fibrin was well washed and dried at a temperature of about 212°, till it was brittle.\*

The specific gravity of the blood was ascertained in the ordinary way, using a spherical bottle, with a portion of a thermometer-tube accurately fitted by grinding for a stopple. The blood was weighed after it had cooled; and the very minute quantity of air that entered in consequence of contraction from cooling was expelled by adding a drop of serum.†

\* I satisfied myself that by this process there was no loss of fibrin. The expressed fluid, thrown on a filter, passed through entirely, and afforded no residue.

† I may remark, by the by, that, were carbonic acid gas disengaged during the coagulation of the blood, it would not be an easy matter to ascertain the specific gravity of this fluid. Indeed, it would be almost impossible

From these results, it may be inferred, that there is no constant relation between the appearance of the buffy coat of the blood and the proportion of fibrin which the blood contains. No doubt, *ceteris paribus*, the buffy coat will be thickest when the proportion of fibrin is largest; and in most of the *phlegmasiæ* the proportion of this ingredient of the blood seems to increase as that of the red particles diminishes, but not, I conceive, generally to any very great extent.

Farther, it would appear from these experiments that there is often a minute variation of the proportion of fibrin in the blood as it flows, judging from the quantity extracted from the first and last portions drawn. In all the instances of variation given, excepting one, the proportion of fibrin is smallest in the blood last drawn. In the solitary exception the increased proportion in the blood last drawn was distinctly ascertained. To what this peculiarity was owing is matter for conjecture. It seems to me very probable that it was derived from blood different from the general mass of the circulating fluid, and which had been liberated, as it were, from confinement, and restored to the circulation from a state of congestion in a diseased organ. The patient from whom this blood had been taken had been ill about twenty-four hours with dysenteric symptoms, complicated with pain in the region of the liver; and he was greatly relieved by the venesection.

Having now considered the questions proposed at the commencement, and assigned reasons for answering them in the negative, viz. that the fibrin of the blood is of higher, not lower specific gravity than healthy blood, and that it does not necessarily contain a large proportion of fibrin, I shall conclude, sedulously avoiding entering upon minute criticisms of the various inferences and reasonings connected with the conclusions which I have been under the necessity of opposing; and which, if the premises are proved to be erroneous, must themselves be incorrect.

On such a subject as the blood, so important in physiology—so important in pathology—and hardly less so, as it is commonly considered, in the practice of physic,—it is of consequence that we are not misled by "false facts," and from them deduce erroneous principles; and the higher the authority, the greater far is the danger to be avoided.

*Observations relative to the question, "Is there any heat given off during the coagulation of the blood?"*

This question, it appears to me, has given rise to much more discussion than was necessary, and more perhaps than its importance deserves.

Mr. Hunter, who first considered the subject, made an experiment, conducted in the most satisfactory manner, on the blood of the

to do it with tolerable accuracy. But who ever complained of this difficulty?

turtle, the temperature of which was the same as that of the air, and he could detect no evolution of heat in the act of coagulation.

I have made similar experiments with the blood of the turtle and shark, and with the same negative results.

Other experimenters have obtained different results, their experiments having been made amidst a complication of circumstances, so as to be of little value, and to baffle almost calculation. Dr. Scudamore, I believe, is the last who has given himself the trouble to make these unsatisfactory experiments; and he is of opinion, that, when blood coagulates, heat is evolved.

I shall take the liberty of considering the principal data from whence he draws this inference. They are contained in his "Essay on the Blood," published in 1824, from page 68 to page 84. He made three experiments, or rather I should say, repeated the same experiment thrice, under the same circumstances, on the coagulation of blood, using two thermometers, the bulb of one sunk nearly to the bottom, and that of the other kept about an inch below the surface.

At the commencement, in all the three experiments, the thermometers at the bottom and near the surface agreed. In the first experiment  $92^{\circ}$  was the temperature indicated, in the second  $94^{\circ}$ , and in the third  $94^{\circ}$ . But at the conclusion of the first experiment, after eight minutes, the upper thermometer was  $90.25$ , the lower  $90^{\circ}$ ; in the second experiment, at the end of nine minutes, the upper thermometer was  $92^{\circ}$ , the lower  $90$ ; and in the third experiment, at the end of six minutes, the upper was  $93^{\circ}$ , the lower  $94^{\circ}$ . Thus, at the conclusion, in one instance, the upper thermometer is only one-fourth degree higher than the lower; in another instance it is  $2^{\circ}$  higher, and in the third it is  $1^{\circ}$  lower. Can confidence be placed in experiments in which there is such a want of harmony in the results?

Then consider the rate of increase and decrease of temperature in the second experiments and especially in the lower thermometer. From  $94^{\circ}$  in one minute it rises to  $95^{\circ}$ ; during the two following minutes it is stationary; the next minute it falls to  $94^{\circ}$ ; the next minute it is stationary; and the next it falls again one degree. Does not this call for the same remark as the preceding.

Dr. Scudamore lays much stress on his fifth experiment, in which the blood was stationary for three minutes at  $93^{\circ}$ , and then suddenly rose to  $94^{\circ}$ .

After 5' 30"	it was $93^{\circ}$
6' 20"	$92^{\circ}$
8'	$92^{\circ}$
10'	$90^{\circ}$
11'	$89^{\circ}$
12' 30"	$88^{\circ}$

And, he remarks, "the blood was of jelly consistence in eight minutes, and firmly coagulated in ten." Here, then, the evolution of heat of  $1^{\circ}$  took place; five minutes before the blood was of jelly consistence; and during the

interval of two minutes, viz. from  $8'$  to  $10'$ , when it was firmly coagulating, it fell  $2^{\circ}$ . Does not this experiment, (were it deserving of confidence,) tend to prove, not what Dr. Scudamore infers, but the contrary?

Dr. Scudamore lays equal or more emphasis on the results of his next experiment on very sized blood, in which the thermometer rose "from  $80^{\circ}$  to  $81^{\circ}$ , just at the moment that the concretion of the fibrin began. For a short time before it had been stationary, and afterwards it continued exactly at  $80^{\circ}$  for the space of five minutes." Why, I would ask, was it stationary before the concretion of the fibrin began; and why, immediately after it had risen  $1^{\circ}$ , did it fall  $1^{\circ}$ , and then remain stationary for five minutes?

These are the principal results which Dr. Scudamore obtained, and of the accuracy of which he is quite satisfied, and from which he deduces the inference, "that the most remarkable evolution of sensible heat takes place just at the moment of time when the fibrin first concretes;" and that a "slight extrication of heat continues throughout the progress of coagulation."

On my mind, I must confess, in perusing the author's details, his results have made quite a different impression. They are deficient in uniformity; they are capricious and irregular, and indicate clearly either inaccurate observation, or, what is more likely, considering their nature, the interference of disturbing powers, (from which it is impossible to rid such experiments,) which entirely vitiate the results, and render them useless, or worse than useless, deceptive.

Mr. Hunter's experiment was an *experimentum crucis*. It was a simple experiment. No disturbing circumstance interfered with its accuracy—and that proved that there is no sensible evolution of heat on the occasion in question.

Moreover, abstractedly considered, ought we, *a priori*, to expect that any sensible heat should be produced during the process of coagulation? The coagulation takes place slowly. The greatest degree of contraction that the crassamentum experiences is many hours in being completed, and the quantity of matter that changes its form from liquid to solid is exceedingly minute. I have for this reason always thought, that, if heat be evolved during the operation, the quantity must be too inconsiderable to be detected. I might mention many facts in corroboration of this. I shall confine myself to one, which seems to me to be also an *experimentum crucis*; and, if considered so by others, this question will never again be agitated.

The fact to which I now allude is, that when the serum of blood is coagulated by means of dilute nitric acid, there is not the slightest elevation or change of temperature, as any one may satisfy himself by trying the experiment. It may not be amiss to give the particulars of one trial of this kind. I added an ounce of serum to an ounce of dilute nitric acid, each of the temperature of the air of the

room, viz.  $75^{\circ}$ ;—coagulation was immediately produced, but not the slightest change of temperature was occasioned. The mixture was exactly  $75^{\circ}$ . The dilute nitric acid used contained five parts of water to one of strong acid. The serum employed was of specific gravity 1.030; and, when slowly evaporated to dryness, an ounce of it afforded a brittle translucent residue, weighing 36.7 grains. It was procured from the blood of a patient labouring under a tedious chronic disease; which blood was only of specific gravity 1.047, and an ounce of it afforded 2.4 grains of dry fibrin.

Now, reasoning from these premises, must we not arrive at the conclusion anticipated, that no heat can be given off during the coagulation of blood, where so small a quantity of matter becomes solid, and that slowly, since none is given off during the coagulation of serum, when so large a quantity of matter is solidified, and that suddenly, and which in its nature is so analogous to fibrin.

I have only another remark to add. It should be kept in recollection, that there are many very striking exceptions to the rule of change of form, occasioning, or being associated with change of temperature; and that in every particular instance the question, whether the temperature will change or not change with change of form, can be determined only by experiment.

*Observations relative to the Question, "Is there any free Carbonic Acid in the Blood?"*

Sir Everard Home and Dr. Scudamore, the former in his Croonian Lecture for 1818, the latter in his "Essay on the Blood," published in 1824, have endeavoured to prove that blood, both arterial and venous, contains a notable quantity of carbonic acid gas, uncombined and free, a portion of which always separates when blood spontaneously coagulates, and the whole of which is disengaged when atmospheric pressure is removed by means of the air-pump.

In a note to a paper on pneumato-thorax, which had the honour of being published in the Philosophical Transactions for 1823, I have come to a contrary conclusion,—that blood gives off no carbonic acid gas under the circumstances specified, and that it contains none. My reasons for adopting this conclusion were the following:—

1st. Because the alkali in the blood is not saturated with carbonic acid.

2dly, Because the serum of blood is capable of absorbing carbonic acid gas to greater extent even than water, as I had ascertained by experiment.

3dly, Because during the coagulation of blood spontaneously, and the coagulation of serum by heat, I had never observed carbonic acid gas to be disengaged, when the experiments were properly made in vessels to which air could not have access, as in tubes completely filled with blood or serum, and inverted in blood or mercury.

Lastly, Because I had not been able to

procure carbonic acid gas from blood just drawn from the blood-vessels, and still warm, when placed under a receiver, and completely exhausted of air.

The first reason which I have assigned must be universally admitted. It cannot be controverted. All those who have examined the blood are agreed that it contains free alkali, and that in a notable proportion. This then being granted, is it not contrary to all the rules of chemical science to infer, that blood should contain a free acid, even out of the body when destitute of life, and when undergoing changes in obedience to the laws of chemical attraction acting on dead matter?

The second reason is in harmony with the first, and a confirmation of it. Admitted as a fact (and it is most easily proved) that serum absorbs carbonic acid gas, and even more than water absorbs, would it not be wonderful indeed, if, with its capacity undiminished for this acid, it should give it off, and *that* while the blood is cooling and coagulating, and when it has the power of absorbing carbonic acid?

The third reason assigned is of a piece with the two preceding, and a still stronger confirmation of them;—I allude to the negative result of the coagulation of serum by heat. The experiment I have repeatedly made, and always with the same result. If, then, heat sufficient to coagulate serum does not extricate carbonic acid gas from it in the act of coagulation, can there be any free carbonic acid in the blood?

On the last reason which I have given, the negative result of no air being disengaged when fresh blood was put under a receiver and a vacuum formed, I shall not particularly insist; though it is conclusive as a negative result. Mr. Brande's experiments quoted by Sir Everard Home, and Dr. Scudamore's and Mr. Gardner's experiments referred to in Dr. Scudamore's Essay, already quoted, may be opposed to mine. Let the experiment be repeated by others and these accurate observers, and then the truth will appear. I shall lay no stress on the discrepancies of Mr. Brande's results, and of Dr. Scudamore's and Mr. Gardner's, one obtaining, or at least stated by Sir Everard Home to have obtained, two cubic inches of carbonic acid gas from every ounce of blood; while the others obtained from this quantity of blood only about .04 cubic inch, or from six ounces rather less than half a cubic inch.

I now beg leave to give another reason in opposition to the statement of there being free carbonic acid gas in the blood. I have added carbonic gas to blood and to serum, over mercury, at the rate of one-fourth cubic inch of gas to the ounce, the whole of which has been absorbed, and yet the blood and serum still exhibited free alkali, indicated by the change produced on litmus and turmeric paper; and I have raised the temperature of the blood and serum to blood-heat, and no carbonic acid gas has been disengaged; and I have coagulated both by heat of about  $200^{\circ}$  Fahren-

heit, and even then not a particle of gas was extricated.

If, then, blood and serum have the power of retaining at the temperature 200°, and in the act of coagulation carbonic acid gas added at the rate of one-fourth cubic inch to the ounce, and if this portion of carbonic acid is not sufficient to neutralize the free alkali of the blood, is it probable—is it possible, that blood can possess any free carbonic acid?

On the datum which I have thus endeavoured to prove erroneous, Sir Everard Home has formed an hypothesis of the formation of blood-vessels; and Dr. Scudamore has offered an explanation of the coagulation of the blood.

Were the doctrine correct, it would be incumbent on the inquirer after physiological truth, to scrutinize the speculations of these gentlemen, and endeavour to ascertain, if there is any necessary, that is to say constant connexion between the cause supposed and the effect to be explained—between, in one instance, the passing of a globule of air through coagulated blood, and the production of new arteries, veins, and absorbents; and, in the other instance, between the separation of an acid air, and the coagulation of an alkaline fluid.

From the London Medical Gazette.

*CASE OF BRONCHITIS, from the Pressure of a Malignant Tumour within the Thorax.*  
By EDWARD J. SEYMOUR, M.D.

On Thursday, 15th November, 1827, I was requested to visit Mr. A. B., ætat. 51, who had generally enjoyed a remarkably good state of health. In May last he was attacked with cough and difficulty of breathing, which he attributed to exposure to draughts of cold air; he brought up at this time a little blood by coughing. The ordinary remedies appeared to relieve, if not entirely subdue, the symptoms of the disease, and he was again enabled to take his usual exercise, which was uncommonly violent: walking considerable distances, or riding a young and unruly horse. About six weeks ago the symptoms returned, and he coughed up a small quantity of dark-coloured blood.

He has been exposed to much anxiety.

Mr. Stone, who had observed his state while visiting another patient in the family, had requested him to put himself under medical care.

At present he complains of difficulty of breathing, accompanied with constant, and occasionally violent fits of coughing; expectoration very scanty; his inspiration is difficult, and his attempts to take a long breath are accompanied with a wheezing noise on the right side of his chest, although he fills the lungs on that side completely with air. The ribs on the left side appear fixed, and the left lung impervious to air. Pulse 90, not weak; skin cool; lies with equal facility on either side or on his back; sleeps ill habitually, and this inconvenience has been lately much increased;

tongue clean and moist; no accessions of hectic fever; complains of weight and tightness at the scrobiculus cordis, and want of appetite; bowels regular. Cap. pulv. ipec. ℞. pro emetico statim.

Nov. 16th. The emetic relieved the sense of weight and tightness in the epigastric region. Pulse 100, not weak; expectoration frothy; respiration in the right side of the thorax attended with a peculiar harsh noise like the blowing of bellows; on the left side, the sound is perfectly dull, and the ribs are not raised on inspiration; countenance extremely pale and anxious; no unusual pulsation to be found or pain experienced in any part of the chest; bowels not open. Fiat V.S. ad ℥xvi. ℞. Hydrarg. submur. gr. iv. pulv. jacob. vir. gr. iv. M. ft. pulvis h. s. sumend. haust. sennæ c. m.

17th. Blood drawn yesterday much buffed and cupped: sonorous respiration in right side of the lungs continues; he raises slightly the ribs on the left side during respiration; pulse 96, not weak. Repet. V.S. ad ℥xvi. ℞. Pil. hydrarg. gr. iij. scillæ recentis gr. ii. M. ft. pilula bis in die sumend. ℞. T. camphoræ C. ℥j. mist. amygdalæ. ℥x. muc. gummi acac. ℥j. syrupi ℥j. M. ft. haustus h. s. sumend.

17th. Blood drawn not buffed or cupped; respiration easier, but very sonorous at intervals; pulse 90; expectoration free, frothy. C. C. lateri sinistr. ad ℥x. postea admoveatur emplast. cantharidis pectori contin. alia.

18th. The symptoms the same as yesterday. The remarkable imperviousness to the admission of air in the left lung, the absence of hectic fever, the expectoration being frothy and without the least trace of puriform matter, and the absence of pain, induced me to believe the symptoms to arise from the pressure either of an aneurismal tumour, or one formed by enlarged glands at the origin of the bronchi, especially on the left side. In consequence of this view of the case, the family were informed of my fears and my despair of its favourable termination.

19th. Expectoration has become very considerable; hoarseness much increased; sonorous inspiration not diminished; some sleep during the night; pulse 80, soft and not strong. Omitt. pil. scill. c. pil. hydrarg. ℞. T. Benezoës C. ℥ss. muc. g. acac. ℥j. mist. camphor. ℥x. M. ft. haustus ter in die sumend. Repet. haust. anodynus h. s.

20th. The expectoration of frothy mucus very considerable; symptoms the same. Repet. medicamenta.

The same treatment was continued, with only the addition of occasional laxatives, for a week. On the 28th, the expectoration having become extremely profuse, (three pints in the twenty-four hours,) the patient's strength failing, the respiration continuing sonorous, and some difference being observed for the first time in the pulse at the wrists, that in the left being fuller than that in the right, and occasionally becoming irregular, I warned the family that I feared a fatal result was not far

distant. On the 29th, in the evening I met Dr. P. M. Latham in consultation. Dr. L. was of opinion that my idea of the presence of a tumour was not improbable, and in other respects, after a very careful examination, coincided with me as to the nature of the case. R. Mist amygdalæ. Mist. camph. āā. ℥j. subcarbon. ammon. gr. viij. M. cap. dimidium hujus misturæ 2ndā quâque horâ. Repet. haust. anodyn. c. t. opii camp. ℥ss. h. s.

30th. Better; pulse 86, soft; otherwise the same.

Dec. 1st. He cannot lie down in bed; great anxiety is present; expectoration profuse; pulse 110, occasionally intermitting; his mind wanders at intervals, respiration very loud and ringing. Repet. medicamenta.

2d. Pulse 106, regular; his mind wanders, the countenance is much sunk, respiration less difficult; he swallows with some difficulty.

About five in the evening he complained of great pain in the left side of the thorax for the first time; at nine expressed a desire to go to the night chair, and died instantly on reaching it.

*Examination of the body thirty-two hours after death.*—The examination was made by Mr. Cæsar Hawkins, in the presence of Dr. P. Latham, Mr. Stone, and myself.

A tumour was found in front of the chest, above the heart, situated for the most part anterior to the roots of the lungs, but surrounding the lower part of the trachea and both bronchi, the trunk and branches of the pulmonary artery, the pulmonary veins of the left side, the arch of the aorta and left carotid and subclavian arteries, but leaving the anterior part of the ascending aorta and arteria innominata uncovered. Behind it was in contact with the œsophagus, and below with the upper part and left side of the pericardium; thence it extended into the left lung, so that nearly half of that viscus was occupied with a similar congeries of globular tubercles, the largest about two inches and a half in diameter. The tumour of the lung adhered to the left side of the pericardium, to the diaphragm, and to the vertebræ and heads of the ribs behind, so that the lung could not be removed without tearing through the tumour. Most of the masses composing the tumour were of a white colour, but some were black in the centre, and others had begun to become soft and red in the inside. Where the tumour was in contact with the diaphragm, that membrane had become softer. A rupture had taken place, by which an effusion of blood had occurred, from the centre of one of the tubercles behind the vena cava descendens into the cavity of the pericardium, which contained about a pint of fluid blood. The back part of the aorta had also begun to change in texture, though the alteration did not yet reach its inner coat. The caliber of the superior cava was much lessened by the pressure of the tumour, and in one part its coat had been absorbed, so that a small fungous projection had taken place in its interior.

About an inch and a half of the œsophagus nearest to the tumour had also become thickened and contracted, the change appearing most distinct in the muscular tunic.

The heart itself was healthy, but the cavities of one of the left pulmonary veins was greatly diminished by the growth of the tumour, which had not, however, affected its coats.

The left bronchus and its branches were much lessened in diameter by thickening and pressure, and the remaining part of the lung of the left side scarcely crepitated, being filled with mucus and watery exhalation. The tumour had grown most in the lower lobe, so that very little of the texture remained, which, however, was solid. The pleura covering this lobe was much thickened, and adhered to the ribs. Many irregular white masses of condensed cellular membrane extended from the tumour into the outer part of the lung, thicker and less ligamentous than the bands usually are in cancerous tumours.

The disease did not reach beyond the root of the right lung, which was red and full of fluid, and the pleura contained a number of white spots of a cartilaginous consistence, which were of the size of a split pea.

A tumour similar to that in the chest, and about the size of a small orange, was situated above the head of the pancreas.

The rest of the viscera were healthy.

This case affords a well-marked example of a disease, which, although it is known to practitioners of long and extensive experience, is nevertheless rare.

It appears to have been known to Morgagni, who has described it as a peculiar appearance, as resembling fatty tumours, (*quasi steatomata*,) in Art. 22, Epist. 22. It has been much commented on by modern French writers. Bayle terms it cancerous tubercle of the lungs, and by Laennec\* it is called *Encéphaloids des poudrons*, because in its advanced or third stage, it occasionally resembles in appearance and consistence medullary matter. It is the fungous hæmatodes of English writers.

Bayle has the merit, among the French, of having first described it under the title *Phthisie cancéreuse*; and the following account, which he relates of the appearances on dissection in his first case of that disease, very evidently resembles those in the one which I have described.

"The right lung, (he says) was much more affected; a great number of rounded tumours were observed in it of various sizes, from that of a nut to that of a chestnut; they all appeared continuous with the texture of the lung, their colour was white, they were rather shining, and their appearance sufficiently resembled the fat of bacon. Some capillary vessels were visible in them as in the brain. Among these tumours some were still hard,

\* *Dictionnaire des Sciences Médicales*, afterwards transcribed in his work *de l'Auscultation Médiate*.

and they were the most shining; the others had less consistence, and were of the colour of milk. On pressing these last a white pus was forced out from a great number of points, of the consistence of cream. Some other tumours, were already nearly destroyed."—Bayle, *Recherches sur la Phthisie Pulmonaire*, p. 298.

In the valuable work of Dr. Baillie on Morbid Anatomy, this disease is not described, unless we suppose his account of the soft pulpy tubercle to apply to one stage of it. The editor of Dr. Baillie's works, Mr. Wardrop, has supplied the information omitted by the author.

When this disease occurs in the viscera of the abdomen, the liver, spleen, or kidney, the parenchyma of those organs appears pushed aside in order to omit the foreign growth, the parietes of the cavities in which they are contained adapting themselves to the increased size of the organ; but as the lungs are contained within parietes admitting of very little dilatation, the growth of fungous hæmatodes of the lung is attended with inflammation and condensation of neighbouring textures. Hence the blood drawn in the case described was much buffed; a circumstance which does not appear to accompany the disease in other viscera, and which therefore is to be considered as an occurrence incidental to its progress in that particular situation, and by no means as affording evidence of the inflammatory origin of the disease.

Until the deviation from the laws which govern the animal body in a state of health, which precedes these formations, be understood, we must be satisfied with palliative treatment. By relieving the symptoms which arise from pressure, by moderate venesection, by enjoining perfect rest and strict diet, restraining the irritating cough by mild opiates, and supporting the occasionally fainting powers with diffusible stimulants, we shall contribute to the comfort of the patient, and delay the fatal event.

From the Medico-Chirurgical Review.

#### M. BOUILLAUD ON DISEASES OF THE BILIARY DUCTS.

In a former number of this Journal, we presented our readers with some interesting facts relative to this point of pathology, from the work of M. Andral, the younger:—we shall now bring forward some observations from a zealous cultivator of pathological science, M. Bouillaud.

Among the alterations of structure observed in the biliary ducts, some are common—others rather peculiar. In the former class, we may reckon inflammation and its consequences, as induration, suppuration, ulceration, thickening, &c. Also, dilatation, contraction and obstruction. Of those lesions which are somewhat peculiar to the parts in question, are biliary concretions, and various other morbid changes which the bile itself undergoes, as well in its consistence as in its

chemical composition. It is but seldom that we find any one of these lesions uncomplicated with one or more of the others. Thus, inflammation of the internal membrane may produce contraction or even obliteration of the ducts—and the presence of biliary calculi may induce inflammation, and so forth.

The symptoms indicative of different lesions in the excretory apparatus of the liver are far from being satisfactorily ascertained. Hence the greater necessity for accumulating such facts as may enable future observers to distinguish diseases that are, in their nature, obscure, though, in their effects, extremely distressing and embarrassing.

*Case 1.* Peter Voisenat, aged 50 years, rather embonpoint, entered the Cochin Hospital, on the 12th March, presenting the following phenomena. He stated that he had been ill about three months. The skin and the conjunctiva were of an orange colour—there was evident fluctuation in the abdomen—but no infiltration of the lower extremities. The tongue was red—the pulse febrile. This feverish state persisted for three weeks, at which time, coma supervened, and the patient died.

*Dissection.*—On the inferior surface of the liver, a tuberculous mass presented itself, enveloping the situation of the gall-bladder, not a vestige of whose parietes could be found, by the most minute investigation. Neither could there be discovered any trace of the ductus cysticus, hepaticus, or choledochus. There was a cavity or pouch in the situation of the gall-bladder, containing a purulent fluid, in which were several biliary concretions. The pouch was adherent to the arch of the colon, and here there was a commencing ulceration which would soon have opened a communication with the intestine, and through which, no doubt, the biliary calculi would have been discharged. The vena portæ was completely obliterated.

*Case 2.* B. Lebant, 68 years of age, having experienced severe and long-continued mental anxiety, entered the Hospital on the 4th November. On examination with the stethoscope, it was ascertained that she had valvular disease of the heart, especially of the left auriculo-ventricular opening, of which she died a few days after she was admitted. During this period, there was not any symptom evinced which led to the supposition of disease about the excretory ducts of the liver. On dissection, the convex surface of this organ was found adherent to the abdominal parietes. The edge of the liver descended as low as the iliac fossa on the right side. The substance of the organ was rather gorged with blood, but otherwise healthy. The gall-bladder contained 90 gall stones, of polished surface, light yellow colour, and various magnitudes. The parietes of the biliary receptacle were thickened—its internal membrane red, and covered with mucus.—There was bile in the small intestines.

In this case there was no pain complained of in the hepatic region—nor was there any

jaundice. The auriculo-ventricular opening was contracted and puckered.

*Case 3.* Mary Dumé, aged 38 years, had experienced severe moral afflictions, and entered the Cochin Hospital, on the 5th September. She had been affected with jaundice for eight months previously which she attributed to mental anxiety. After a miscarriage in the month of June preceding, a tumour slowly formed in the right side of the abdomen, unaccompanied by any pain. On examination at the hospital, this tumour was of considerable dimensions, being moveable, and apparently the size of a fœtus, rather attached to the right hypochondrium. There was fluctuation in the abdomen, but no œdema of the lower extremities. The patient complained of great debility, but not of any pain. The skin and eyes were yellow, inclining to a green hue. The urine was of the same colour. The stools were generally white—sometimes black and watery. M. Cayol and others examined the patient, but were unable to determine on the nature of the disease. The patient died on the 25th September, in extreme marasmus.

*Dissection.*—The abdominal cavity contained a large quantity of yellow serum. The liver was of middling size, hard, and apparently infiltrated with yellow bile. On its inferior surface were three large tubercles. The gall-bladder was enormously distended, being the size of a child's head. It touched the spine behind, and bulged out the abdominal parietes in front. The neck of the gall-bladder, as well as the ducts, were encircled by a mass of tubercles extremely hard, in which was involved the head of the pancreas. The duodenum was compressed by this mass against the spine, but not obliterated. The immense gall-bladder was filled with a dark coloured viscid fluid, and contained more than a hundred biliary calculi, of various sizes. The parietes of the gall-bladder were very much thickened. The stomach was enormously distended with half-digested aliment. There was no other disease of importance.

This case will elucidate some observations which we made in a preceding number—especially on a case that occurred in Panton Square.

*Case 4. Obliteration of the Cystic Duct.*—A female was brought to the hospital for the treatment of acute pneumonia, of which she died, in spite of all the means they could use. She was evidently jaundiced—and it was remarked that after each bleeding, for the pneumonia, the colour became deeper. On dissection, the gall-bladder was found much distended with bile, and its duct completely obliterated. There was inflammation of the stomach, and ulceration of the pylorus. Bile was found in the small intestines. It was interesting to observe, in this case, that after each bleeding, the yellow colour of the skin and eyes became more and more intense.

Several other cases are detailed by our author, which we cannot notice in this paper. In one case there was found a great dilatation of the ductus communis—ductus cysticus—and

ductus hepaticus, without any ostensible obstruction in any of these canals. But there was inflammation of the mucous membrane of the duodenum and small intestines, which was probably the cause of the dilatation, by preventing the bile from getting into the primæ viæ.

The author enters his protest against the use or rather the abuse of irritating and often repeated drastic purgative medicines, which, he thinks, produce inflammation of the excretory tubes of the liver—gall-stones—and even hepatitis.—*Journal Complémentaire.*

From the Repertoire General d'Anatomie, &c.

SUR LA DEPRESSION LATÉRALE DES PAROIS DE LA POITRINE. Par M. le baron DUPUYTREN.

This affection has not been altogether unnoticed by authors, and M. Dupuytren cites Van Sweiten, J. L. Petit, and Levacher, as having spoken of it incidentally, though from the cursory manner in which it is mentioned by them, it is evident that they had very imperfect ideas upon the cause, effects, and treatment of this mal-formation.

It is especially observed in the children of lymphatic, scrofulous, or rachitic parents, inhabiting low, moist, and damp situations; those who are badly clothed, brought up on light, unsubstantial food, debarred from wine, &c.—The sternum projects forward; the vertebral column is elevated into a sharp ridge posteriorly; and the ribs are not only flattened, but sunken towards the thorax; as if, when flexible and capable of assuming any form, they had been forcibly compressed from side to side. In some cases, it has proceeded to such a degree, that both sides of the thorax may be embraced by the fingers of the same hand. So greatly is the form of this cavity changed, that the lateral diameter is diminished a fourth, a third, and sometimes even one half of its extent, while the antero-posterior and vertical diameters are increased in the same proportion. But, whether it be that the loss in one direction is not exactly compensated by the gain in another, or that the circulatory and respiratory organs, placed in an unnatural situation, are impeded in the regular performance of their functions, this state of things is always accompanied with great oppression, habitual shortness of respiration and of the voice, a state of inexpressible anguish and anxiety; in very young children, there is much difficulty in sucking, and threatened suffocation when the nipple is retained in the mouth any length of time: at a later period, the speech is short and interrupted. These symptoms increase whenever the patient takes any exercise, especially ascending or descending stairs, and on any mental emotion, as in individuals affected with disease of the heart,—disease, which from the disordered motions of this organ, and the irregularity of the pulse, might readily be suspected, if an attentive examination did not discover, that

these symptoms were in accordance with the respiratory movements only, and the consequence of the constriction they experienced.

During sleep, respiration impeded by the mal-formation of the thorax, and the swelling of the amygdalæ, is always performed with the mouth open and with considerable noise. The slumbers of the patient are frequently disturbed by troublesome dreams, having reference to the state of the respiration, and is often interrupted by cries and startings.

The symptoms just mentioned, and particularly the difficulty of the respiration and circulation, may exist in such a degree, as to prevent the development of the vital functions, and occasion death in the first moments of existence, or at a subsequent period, by depriving the patient of the power of suction, disturbing nutrition and the consequent development of strength. And though death should not result either primitively or consecutively, the children thus affected, remain in a state of emaciation, debility, and incapacity of action, which deprives them of the greater part of their faculties.

It is remarkable that this state of things is almost always accompanied with a considerable enlargement of the tonsils, a circumstance, the connexion of which with the thoracic depression, M. Dupuytren is unable to explain. It will be readily conceived that the enlargement must add to the difficulty of respiration already existing, and to such an extent has it proceeded in some instances, that M. Dupuytren has been obliged to extirpate the glands, with evident mitigation of the symptoms.

Pulmonary catarrh is a complication not less frequent than enlargement of the tonsils, and is greatly to be dreaded, especially when existing contemporaneously with the latter, forming a triple cause of oppression. But of all complications, none is more dangerous than that of hooping-cough. "No disease," observes M. Dupuytren, "has presented to me a more painful spectacle than that of an unfortunate child, with depression of the thorax, enlargement of the tonsils, and violent hooping-cough. At each paroxysm, the oppression is such as to threaten immediate death, which soon actually takes place. Who does not perceive, therefore, the necessity of combating this combination of diseases by the most energetic measures?"

Speaking of the propriety of extirpating the tonsils in children at the breast, M. Dupuytren observes, that there is no one better acquainted than himself with the difficulties attending the performance of this operation, at such an early period of life; but such is the impediment to respiration in some cases, as to render their removal the only means of preserving existence. This operation is greatly facilitated by a speculum oris recently invented by Dr. Lemaistre.\*

In examining the bodies of children who have died of this disease, M. Breschet has observed an imperfect development of the osseous system; the cranial bones separated at a period when union should have taken place, the existence of epiphyses, enlargement of the extremities of the long bones, various contortions of their bodies, and softening of their tissue; in the latter respect they may be compared to bones which have been macerated for some time in diluted nitric acid. The bones were sometimes more readily divided with the knife, than broken. The venous system, generally, was sufficiently developed, and the cellular tissue of the bones, of a deep red colour, presenting a vascular appearance. Dentition was retarded, the teeth changed in form, and the coronæ partly destroyed, and furrowed on their anterior surface.

The lungs were depressed towards the vertebral column, presenting a depression corresponding to that of the thorax; behind they bore the impress of the ribs in such a degree, that they were furrowed by these bones, and the lines in relief corresponded with the intercostal spaces.

In relation to the treatment, M. Dupuytren recommends, as in all cases of softening of the bones depending upon scrofula or rachitis, the cautious employment of a tonic regimen, bitter infusions, &c. It will be necessary also, to call in the assistance of local remedies, and of the latter, none have proved, in his hands, so effectual, as exercises which require the action of the muscles which pass from the arm and shoulder to the thorax; pressure frequently employed in the direction of the antero-posterior diameter of this cavity, is also particularly recommended.

The object of these exercises, he observes, is to elevate the parietes of the thorax, separate them, carry them backwards, and restore them finally to their natural condition; and nothing is more conducive to the attainment of this object, than to raise, several hours every day, a weight suspended by a cord passing over two pulleys. A lever should be attached to one extremity, and at the other a weight proportioned to the strength of the patient. Standing on his feet, and even elevated on his toes to reach the lever, the patient should grasp it with both hands, and calling into action the muscles of the fore-arms, the arms, chest, and neck, to flex at the same time the head, thorax, and body, and incline them towards the ground,—should raise the weight attached to the other extremity: employing thus alternately, the flexor muscles to raise the weight, and the extensors to straighten the body. If it be true, as will scarcely be doubted, that there exist relations between the bones and muscles, so that the latter, acting constantly upon the former, tend to impress upon them a determinate and constant shape,—it is reasonable to suppose that the exercise just mentioned, directing the efforts of the muscles upon the bones of the chest, should gradually bring them to a better form.

"To the above plan should be added pres-

\* Vide the February number of the Journal of Foreign Medicine, vol. i. p. 188.

sure repeatedly applied to the thorax in the direction of its antero-posterior diameter. The conjunction of these two means, with regimen and internal treatment, has been sufficient to effect a cure of mal-formations, which were falsely judged irremediable. Pressure made upon the thorax in the direction above mentioned, by means of a machine which should find a *point d'appui* upon the spine, and by the aid of a spring or any other contrivance, tend to depress the sternum, would possess the inconveniences attending all mechanical means constantly applied; it would occasion insupportable pain, excoriate the skin, &c. The method which I advise, is attended with no such consequences; it consists in making pressure with the palm of the hand upon the most salient part of the sternum, while the back is supported by the hand or knee, or placed against a wall. It should be made during expiration, and then suspended, so as to allow the expansion of the chest in inspiration, a facility of doing which will readily be acquired by practice.

"I have several times observed," continues M. Dupuytren, "the immediate effects of this practice—these consist in a flattening of the projection caused by the sternum, a more or less considerable curvature of the ribs from within outwards, a momentary return of the thorax to a more natural shape, respiration deeper and more complete, and when the pressure is removed, the sudden return of the parts to their natural state—a return accompanied by a long inspiration.

"The pressure should be repeated as frequently as possible during the day, and continued several minutes each time. Its beneficial consequences will be in proportion to the frequency of its repetition, and the length of time it is continued.

"The care of such persons should not be confided indifferently to every one. In the heart of a mother alone will we find the perseverance necessary to ensure success; and with this aid, there is no mal-formation of the kind in question, which may not be remedied. I have seen children who were affected with it in the highest degree, ultimately become robust and well formed persons. Such has been the result in the following case, taken at random from among many others, in which the success was equally striking.

"A female infant, descended from a rachitic mother and scrofulous father, was born with great difficulty of respiration, and a still greater difficulty of seizing and retaining the nipple of its nurse, so much so indeed, as to deprive it almost entirely of the power of suction. The ribs were greatly depressed towards the thorax, with a proportional projection of the sternum and spine in the two opposite directions; there was constant dyspnoea, and irregularity in the circulatory and respiratory movements, &c.—symptoms arising exclusively from the mal-formation above mentioned."

"By dint of great exertion, the little patient passed through the period of infancy, and at-

tained her third year; still accompanied with the symptoms above related—short and hurried respiration; habitual dyspnoea recurring upon the slightest exertion; sleep interrupted by frightful dreams, cries, and starting, and the countenance of a violet red colour, deep in proportion as the oppression was more strongly marked. Upon examination, the tonsils were found so much enlarged, as to leave free scarcely half the natural passage.

The patient was seen by several physicians, some of whom attributed her sufferings to disease of the heart, while the greater number concurred with M. Dupuytren, in referring it to the mal-formation of the thorax. As it had sensibly augmented latterly, it was decided to unite to the tonic plan, which had long been in use, the different remedies employed in scrofula. This was done accordingly, but the increased dyspnoea, soon occasioned the latter to be relinquished; it was frequently attempted afterwards, but always with the same result. At this period, M. Dupuytren advised pressure upon the sternum, in the manner already mentioned; the child became accustomed to it gradually, and the parents and friends, encouraged by its beneficial consequences, repeated it as often as a hundred times a day—the child escaping from the hands of one, to run into those of another. This perseverance was productive of the happiest results. In less than six months, the projection of the sternum sunk down, the spinal curvature lessened, the lateral depression of the thorax disappeared almost entirely, the respiration became slower, easier, and more regular; the patient could move about with much greater facility, and the swelling of the tonsils diminished, as likewise the noise occasioned by the air traversing the fauces.

Six or seven years passed away, during which the little patient acquired a great increase of size and strength; still, however, the mal-formation of the chest was not entirely removed. This part of the body had a round and cylindrical form, there was a slight projection of the vertebral column, and respiration was disturbed by any fatiguing exertion. M. Dupuytren being again consulted at this period, recommended exercise by means of the weight and pulleys, which was continued two or three hours daily, for the space of two years, with the most salutary results. The muscles connected with the thorax assumed a great increase of size; the cavity of the thorax enlarged; the spine reacquired its natural curvature, the respiration became ample, deep, and of ordinary frequency—and the patient is now one of the largest and best proportioned of her sex;—no one in looking on her, could suppose it possible that she had at any time been deformed.

*Second Case.*—Michel Pottier, aged six years, had been troubled for the last three months, with difficulty of respiration. His complaints increasing, the parents were naturally induced to examine the thorax, which they were surprised to find, did not resemble that of other children, and M. Dupuytren was consulted.

Superiorly, the thorax appeared sufficiently well formed, but below, the sternum was carried forward, drawing with it the inferior ribs, and thus giving to the chest, a form not unlike that of a young turkey or the keel of a vessel. On applying pressure to the sternum and spine, it could be restored to its natural condition, and a continuation of this practice was recommended.

Two other cases are detailed by M. Dupuytren, but as they contain nothing that has not been already mentioned, we have not deemed it necessary to translate them.

From the Journal des Progres des Sciences et Institutions Medicales.

#### FORMATION OF THE CELLULAR TISSUE. By M. J. ARTHAUD.

It is not our intention on the present occasion to enter into a detailed account of the cellular tissue, but to make only a few observations on a point, respecting which there is still some diversity of sentiment among modern anatomists. The object in question is to ascertain whether the primitive tissue, to which the term *cellular* has been appropriated by Winslow, Malpighi, Ruysch, Haller, Bergen, Thierry, Bichat, Beclard, and Blainville, be really composed of lamellæ and filaments, which united together, form areolæ of determinate figures, as is asserted by those writers; or whether this laminated and areolar disposition be not rather accidental, and the consequence of the operations performed upon this tissue as was pretended by Bordeu, and after him, Blumenbach, Meckel, Wolff, Autenreith, Prochascha, Rudolphi, Treviranus, and Heusinger.

Meckel, substituting the term *mucous*, for that of cellular, calls it *the coagulable fluid in a state of coagulation*. A definition, which however vague, conveys the opinion of this author, that the cellular tissue consists of a viscous, tenacious, and homogeneous substance—a kind of glue, which forms the medium of union between the parts which it envelops, and when these are separated or elongated, assumes a lamellated or filamentous form, which in its quiescent state it did not possess. The injection of air, to demonstrate the existence of cells, is, according to him, inconclusive; for a cellular appearance is equally produced, when air is projected through a tube into mucus or a saponaceous solution.

Before stating the fact, which appears to us conclusive against the opinion of Bordeu and the German physiologists, it may be remarked, that this negative logic opposed to the result of experiment, must necessarily impede the progress of science. No one, at the present day, would call in question the fibrous structure of the brain and spinal marrow—and may it not with equal reason be contended, that this appearance is deceptive, and the consequence of the manual and chemical ope-

rations which are necessary to its demonstration. The same may be said of the osseous system, and in a word, this mode of reasoning would throw a doubt upon the greater number of facts to general anatomy, which have been supposed sufficiently verified by experiment, and this department of science would return to the chaos in which it was, before methodised and illuminated by the genius of Bichat. If however, it be possible to develop beneath the eye of the observer, a cellular tissue with its lamellated form, the hypothesis of Bordeu must necessarily be abandoned.

M. Serre, while studying the organization of the crassamentum, observed that a lamellated tissue was formed in its substance, which from the position of its lamellæ gave rise to cells of a hexagonal form, when an equable pressure was made upon any part of the coagulated fluid, the coagulum was soon converted into an areolar tissue not unlike a honeycomb, and containing fibrine in its cells. When the parietes of these areolæ were attentively examined, even without the assistance of the microscope, vessels running along their surface could be distinguished. This phenomenon was not observed in the part of the coagulum known by the name of the inflammatory coat. It sometimes happens that this coat being very thin, its transparency renders visible the cells developed in the subjacent coagulum, and would thereby induce the belief that it participated in their formation.

This important fact, the truth of which any one may readily ascertain, not only refutes the opinion of Meckel respecting the structure of the cellular tissue, but may also throw some light on several circumstances connected with pathology; to us, it appears satisfactorily to explain the process of union by the first intention. What happens when the lips of a recent wound are brought again into contact?—blood is extravasated in the interval which separates them; this fluid coagulates; cellular tissue is developed in its interior, and the vessels which form in the parietes of this tissue, anastomosing with the capillaries of the wound, subjects this minute coagulum, which previously possessed only an isolated existence, to the influence of general life. An objection will naturally arise here; it may be said that there is no proof that the cellular tissue which enters into the structure of the cicatrices, is formed in the effused blood, and it is more probable that, which existing on the two surfaces of the wound, unites by the adhesive inflammation. In the first place, we do not believe in the adhesion of two surfaces, without the interposition of some fluid, which becoming organized, serves as a medium of union; and secondly, this objection, which appears to possess some plausibility in relation to parts into the composition of which this tissue largely enters, loses all its weight when we reflect upon the organization of the coagulum, in a medium almost entirely deprived of it. In the substance of the brain, for instance, after hemorrhage, the same phenomena are observed as in the wounds of muscles. M. Serre cites several cases in

which masses of cellular tissue, consisting of filaments and lamellæ were found in the brain, filling cavities, the parietes of which were separated to the extent of an inch. He refers moreover to the case of a young man, who in consequence of a fall, fractured the spine between the ninth and tenth dorsal vertebra; he died in consequence, and on dissection it was found that the space which separated the two extremities of the medulla, was filled with condensed cellular tissue composed of filaments interlacing with each other, and adhering to the divided surfaces and to the pia-mater. It is evident that this tissue had been formed in the extravasated blood.

If the cellular tissue were an unorganized muous, one would not hear, in drawing across it the edge of a scalpel, the crepitating noise produced by the successive division of the filaments and lamellæ of which it is composed; and moreover, the greater number of the morbid phenomena of which it is the seat are utterly inexplicable on the hypothesis of Borden. How are we to account for the regular formation of cells in anasarca, the dispersion of blood in ecchymosis? and the production of cells also, when water, or some coagulable matter, is injected into this tissue? The latter might form a direct or angular route, according to the obstacles which they would encounter; but it is physically impossible that it could impart, to a tenacious or viscous substance, the appearance of an areolar organization.

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#### **PATHOLOGICAL AND PRACTICAL RESEARCHES ON THE DISEASES OF THE BRAIN AND THE SPINAL CORD.**

By JOHN ABERCROMBIE, M. D. Fellow of the Royal College of Physicians, Edinburgh, &c. Edinburgh, 1828. 8vo, pp. 444.

To the readers of this Journal the researches of Dr. Abercrombie have been long well known; and few who take any interest in the progress of pathology can be unaware of the assiduity and perseverance with which he has cultivated this branch of medical knowledge. During several years he has continued to lay before the profession, chiefly through the medium of this Journal, a series of researches on various subjects in the pathology of the internal organs of the three cavities. Of these it is unnecessary in this place to speak in the language of panegyric. If they were entitled to no higher commendation than that of a useful and well digested record of pathological and practical facts, their merit would be great indeed. We know that we express only the opinion of the most judicious part of the profession, when we say, that to the great progress made by pathological science of late years, they have contributed in no ordinary degree.

Though the researches of Dr. Abercrombie have not been confined to one cavity or one

organ, but have been directed to all those organs and textures for the unsound states of which the advice of the physician may be requested,—those incident to the brain, *cerebellum*, spinal cord, and their membranous coverings, claimed a large portion of inquiry and observation. In several essays published in the 14th and 15th volumes of this Journal, Dr. Abercrombie took a survey of the morbid states incident to these parts, under the three heads of inflammatory disorders, apoplectic and paralytic affections, and organic changes. Of such inquiries the chief object is to define the pathological characters of each morbid state, to give a connected view of the mutual relations which almost invariably exist among them, and to trace, if possible, between them and their external effects or symptoms, such connexion as may enable the physician to recognise their existence, and to manage them with success. This it was the purpose of Dr. Abercrombie to accomplish; and it has been generally admitted, that, allowing for the inherent difficulties of the task, his attempt, though the first, has been the most successful.

Various circumstances, indeed, well known to the physiologist and physician, tend to involve this subject in the deepest obscurity, and to invest it with difficulties, which nothing but the most assiduous and accurate observation, conjoined with sound judgment and patient inquiry, can ever hope to surmount. To say that these difficulties are partly, if not chiefly the result of erroneous observation and false theory, does not in the smallest degree render the task of inquiry less arduous. It may even occasionally add to the labour, by giving matters of fact an adventitious colouring, and placing them before the eye of the observer in a false and distorting medium. To expect an undertaking of this description to be entirely successful would be unreasonable indeed. The essays, therefore, already alluded to, though possessed of very great merit, partook necessarily of some of those deficiencies and imperfections from which first attempts can never be wholly exempt. To supply such deficiencies, and correct such imperfections, is the chief object of the work now brought before the attention of the reader.

It is natural to expect that a work published under such circumstances must present many points, not only of coincidence and resemblance, but of perfect identity with the essays in the form of which its essential contents originally appeared. Though this is in truth the case to a great degree, it is not nevertheless to such an extent as to render the present volume entirely similar to his early productions. Were this the case, our task would be easy; and all that it was requisite to say might be comprised in a few words. It is due, however, to Dr. Abercrombie to say, that on many points he has modified his inferences; on several he has taken occasion to correct them; and on all he has extended his researches and inquiries so much as to exhibit a more elaborate and connected, and perhaps a more just view of the several divisions of

the subject of investigation. Having premised these observations, it is competent for us to say, that, though we feel it unnecessary to enter so much into the details of the present work, as if it had never in any shape been before the medical reader; yet, to preserve uniformity, avoid confusion, and do justice to the author, it shall be our study, without more allusion to the first essays of Dr. Abercrombie than is absolutely necessary, to give a succinct account of the principal contents of the present volume, and the most important pathological results to which the inquiries of Dr. Abercrombie lead.

In a short but well-written preface the difficulties of the subject are stated, and the necessity of founding general principles on extensive collections of accurate and well-established facts only, is fully admitted. Strongly impressed by these considerations, while the ingenious author acknowledges that he supports no system, and proposes no new doctrines, he modestly disclaims any other merit than that of collecting and arranging facts; and whatever conjectures he offers either to explain their nature, or account for their origin, are studiously separated, that the real value of the former may not be impaired by admixture with the latter.

In the present work, as in the original essays, the entire subject of diseases of the brain and its membranes is considered under the three general heads of inflammatory affections, apoplectic affections, and organic affections; while a fourth part consists of diseases of the spinal cord. We must presume that the author finds this the most convenient arrangement for his purpose; for it is obvious that it is not entitled to the character of being very clear or distinct. It is an objection of some force, that inflammatory affections of the brain or its membranes not only produce, but often terminate in apoplectic symptoms; that apoplexy itself is, pathologically speaking, very often an inflammatory disorder; and that almost all the organic changes incident to the brain are accompanied or followed by more or less inflammation, either chronic or acute, and almost invariably terminate in apoplectic death. It is further no inconsiderable objection to this division that the principle on which it is made is not uniform, or of general application to its several parts. Thus the first part, or that of inflammatory disorders, is derived from pathological characters; the second, the apoplectic, from symptomatic characters; while the third again is taken from the physical changes which the structure of the parts undergoes. In the fourth part a new principle of division is again employed, in so far as the anatomical situation of the part is selected as the general head, which is again subdivided into a considerable number of sections, partly on pathological, partly on symptomatic, partly on organic characters.

Of these objections the ingenious author doubtless was aware, and would have obviated them if he had thought it important to do so, or had it been consistent with the general plan

of his work. They serve to show rather the difficulty of the subject itself than any imperfection in the manner in which it is treated. It may further be remarked, that the errors of the mode of division are compensated, so far as is practicable, by the distinctness with which the subordinate divisions are treated, and by the instructive matter which is every where brought forward to illustrate the several forms of cerebral or meningeal disease. No distinction is made, no pathological position established, unless on the evidence of facts competent, according to the laws of sound reasoning, to uphold its truth.

After a general view of the symptoms which indicate inflammatory disease within the head, Dr. Abercrombie proceeds to consider the principal seats and terminations of the disease. These terminations vary manifestly according to the texture of the part affected; but with the exception of one form, that of pulpy destruction (*ramollissement*,) Dr. Abercrombie does not attempt to distinguish the terminations according to this principle. Thus, inflammation affecting the textures within the skull, may be fatal in the inflammatory stage, whether seated in the membranes, or in the cerebral substance, though the most distinct cases of this description take place in the centre of the hemispheres. Serous effusion again, which is admitted to be the second mode of termination, may take place within the ventricles, in the sub-arachnoid cellular tissue, and between the arachnoid membrane and *dura mater*. Disposition of false membrane, the third mode of termination, is most common beneath the arachnoid membrane, and is occasionally seen covering the choroid plexus within the ventricles. Suppuration, a fourth result, though often found beneath and occasionally above the arachnoid, is most usual, however, in the substance of the brain, either in distinct abscesses bound by cysts, or in irregular broken down cavities. A fifth result, pulpy disorganization, is regarded as analogous to gangrene in other parts of the body; and a sixth head consists of changes in the thickness of the membranes, contraction and obliteration of the sinuses, and caries of the bones, which are known to take place in consequence of certain *chronic* forms of the disease.

This mode of distinguishing the several terminations of the inflammatory process, when seated within the cavity of the cranium, is obviously more practical than pathological; and perhaps it may be said there is no sufficient reason why it should not be the former rather than the latter. It must be presumed, that under all these circumstances it appeared to the author the most convenient which he could adopt; since his long habits of accurate observation and much reflection on the objects of that observation could not fail to apprise him not only of its objections, but of its advantages. It would be idle to deny that it is free from objections, not the least of which is, that it is deficient in precision, in so far as it arranges under the same general head, different

morbid states and processes going on in the membranes alone, or the brain alone, and which, though occasionally found co-existing, are nevertheless very often separate and distinct. It is not our intention, however, to enter into minute criticism of the propriety of this mode of viewing the subject. It will be more useful to proceed at once to consider the subject in its minute ramifications. This is done chiefly by illustrating his general positions by appropriate and instructive examples of each form of disease.

The first which claims attention is inflammation of the *dura mater*. The rarity of this as an idiopathic affection is demonstrated by the circumstances of one case in which purulent matter was collected between the bone and *dura mater*, with marks of extensive inflammation of the arachnoid membrane. As an effect of external injury, it is greatly more common. But in practice it is often associated with disease of the ear and petrous portion of the temporal bone; and to this Dr. Abercrombie justly directs much of the attention of the reader, and illustrates the nature of the disorder by three apposite cases. He properly represents it to be attended with caries of the petrous portion of the temporal bone, detachment, thickening, and ulceration of the *dura mater*, deposition of purulent matter and lymph, and superficial suppuration of the brain or cerebellum. In a few rare cases it produces suppuration of the substance of the latter.

The connexion between disease of the ear and that of the brain and its membranes was early noticed by Ballonius, Bonetus, Morgagni, and Gontard; and though cases of this nature have since been made the subject of consideration by Mr. Brodie, Mr. Parkinson, Dr. O'Brien, Dr. Duncan, Junior, and the present author, it may be doubted whether their nature and origin is very clearly explained. It was the opinion of Bonetus, that, in cases of discharge from the ear, the purulent matter indicated an abscess in the brain from which he supposed it to proceed. Though this notion was satisfactorily refuted by Morgagni, it was revived by Mr. Brodie, who seems to think the effusion of the brain coeval at least with that of the ear. Knowledge of the anatomical relations of the internal ear, nevertheless, is all that is requisite to explain the origin and cause of the disease. In all cases almost it originates in inflammation of the fibro-mucous membrane which lines the tympanal cavity, proceeds backwards to the mastoid cells, and is continued inwards and forwards through the *fenestra ovalis* and *fenestra rotunda* into the vestibule and *cochlea* respectively. In following this direction it is easy to see with what facility inflammatory action is propagated to the *dura mater* investing the petrous portion of the temporal bone, and especially that part of it which descends into the internal auditory hole on the posterior surface of the pyramid. Here indeed the *dura mater* is almost in direct contact with the fibro-mucous membrane of the vestibule. It hence results that inflammation occurring in the tympanal cavity, and as-

suming, as it often does, a mild and chronic character, under the shape of a mere ear-discharge (*otorrhæa*), is too often neglected until it spreads either into the mastoid cells, or through the vestibule and *cochlea* to the meningeal covering of the temporal bone, when it sooner or later, sometimes very speedily, causes extensive detachment, suppuration, and ulceration of the membranes, and even abscess in the adjoining surface of the brain or cerebellum. These effects, which were originally observed by Morgagni, and latterly by Powell, are well illustrated by the cases of the work before us.

Though it is most common to see the disease confined in this manner to the surface of the brain, in the cases of Bianchi, Mr. Brodie, Mr. Parkinson, Dr. O'Brien, and in one of those of Dr. Abercrombie, suppuration took place in the interior substance of the cerebellum. Under such circumstances it not unfrequently happens that the discharge, after continuing for months or even years at various rates, suddenly ceases, or even is suppressed after surgical treatment; and this suppression marks the commencement of the acute meningeal or cerebral inflammation which terminates life.

The connexion between disease of the brain and discharge from the nose, that is, chronic inflammation of the fibro-mucous membrane lining the ethmoid plates and cells, or the frontal sinus,—a connexion adverted to by Dr. Abercrombie, after Bonetus, Morgagni, and Lieutaud, is to be viewed as part of the same general anatomico-pathological fact. In short, the intimate and almost direct connexion which exists between the fibro-mucous lining of the horizontal perforated plate of the ethmoid bone, and the *dura mater*, which covers its inner surface, is sufficient to show the easy transition of morbid action from the former to the latter.

With morbid states of the *dura mater* Dr. Abercrombie arranges certain changes incident to the venous canals or sinuses interposed between its plates. Though these changes belong perhaps to those of the venous system in general, yet it must be conceded to the author that their occurrence depends much on disease of the *dura mater* or contiguous bone. This is well illustrated in the fifth case, in which the left lateral sinus contained purulent cheesy matter, without blood, and with thickening of its coats to such a degree that its cavity was at one part nearly obliterated. The jugular fossa was carious for the space of a shilling; and the pyramidal portion of the temporal bone was extensively carious. Does such an unsound state of the sinuses ever occur independent of disease of the bone?

Under the general appellation of *Meningitis* are placed inflammation of the arachnoid membrane and of the *pia mater*, which Dr. Abercrombie justly remarks, it is difficult to distinguish either in practice or pathologically. So intimate, indeed, is the anatomical connexion of these membranes, that the circulation of the one almost constantly influences that of

the other; and one of the most usual effects of distention of the vessels of the *pia mater* is effusion of serous fluid in the sub-arachnoid cellular tissue, or from the arachnoid itself, or of albuminous exudation from its surface. This is well known; and the principal modifications of disease, occurring in different regions of the exterior meninx, are well illustrated by cases in the present work. One important circumstance, however, which indeed is by no means generally understood, is not adverted to, either under the present head, or in any other part of the volume; for neither the first nor the fourth subdivisions of the sixth section can be considered as belonging to this head. We allude to the *meningitis* of the internal or central portion of the membrane. The choroid plexus, though known to be of a similar nature to that of the *pia mater*, is, however, seldom regarded as a direct continuation of it, possessing similar properties, performing the same function, and liable to the same morbid states. The choroid plexus also is not confined to the lateral ventricles, and extends not only into the inferior and posterior *cornu*, but into the fourth ventricle, at the bottom and side of which its connexion with the *pia mater* may again be traced. Inflammation is, perhaps, more common in this division of the cerebral membrane than in its outer part. It is this inflammation which is the cause both of the symptoms of *hydrocephalus*, and of the watery effusion occasionally found after death; and it is a circumstance not unimportant in a pathological light, that the inflammatory process, taking place in this membrane, is attended with preternatural dryness of the exterior division,—indicating an alternation of action between the two. It is the same congestion and effusion which takes place in tedious cases of fever and various febrile diseases, with symptoms of affection of the brain.

Inflammation of the substance of the hemispheres, or rather of that of the brain, is illustrated at great length, under the heads of vascular injection, pulpy destruction, (*ramollissement*), either alone, or with purulent matter, undefined suppuration, encysted abscess, and ulceration of the surface of the brain. The three first of these, at least, are different degrees only of the same morbid process.

In the first case, which is also the first stage, a portion, more or less extensive of the substance, generally, of the hemisphere, becomes of a red colour, highly vascular, and in a slight degree less consistent than the surrounding brain. In the second case, this state of vascular injection may be traced passing gradually into that of softening, when part of the substance becomes soft, yellowish, or ash-coloured, much disorganized, and either with or without spots or patches of purulent matter. When the latter is found either in an irregular ragged cavity, or in a cluster of communicating cells, it constitutes a fourth form of the disease, being one variety of cerebral abscess; and when it is inclosed in a membranous cyst, more or less consistent, it constitutes another, as the encysted abscess.

From the observations of Bonetus, Morgagni, Lieutaud, Baader,\* Baillie, Powell, Brodie, Hooper, and the present author, no doubt can be entertained, that, with the exception of the last, the whole of these morbid states are to be viewed as different stages of the same process. Dr. Abercrombie, ascribing these varieties to different degrees of activity in the inflammatory process, is inclined to the opinion, that in the most active form of the disease it advances speedily to ramollissement or suppuration; and that in more chronic forms, in which it proceeds slowly, it may produce urgent symptoms for some time without advancing to an irrecoverable state. After this he thinks there is reason to believe it may pass into suppuration or induration of the cerebral substance, and thus be a cause of organic change.

That the pulpy destruction is an effect of inflammation we formerly attempted to show, when considering the researches of MM. Rostan and Lallemand. Dr. Abercrombie, who had occasion to take the same view of the subject in his original essays, adheres to it, but regards the change as analogous to gangrene in soft parts. This idea is by no means new; for not only was it that of Bonetus, Morgagni, and Lieutaud, but more recently of Jemina and Baillie. Indeed Bonetus, who has repeatedly mentioned the softened state of parts of the brain, seemed to think that the only way of accounting for the preternatural diminution of consistence, was by ascribing it to mortification; and the same idea is currently repeated by many authors and collectors to the present time. Jemina particularly, who saw this change in the bodies of persons cut off by a febrile epidemic, at Montreal, in the Turin territory, in 1783–84,† after describing it, asks, *annon gangrena hujus visceris?* and the same appears to have been the idea entertained by Dr. Baillie.

No doubt can be entertained, we conceive, that it is the effect of inflammation, acute or chronic; for it takes place not only as a consequence of this occurring spontaneously, but also from mechanical injury inflicted on the head, and even as a consequence of the vascular distention which attends tumours, morbid induration, and other organic changes of the brain. That it takes place in consequence of injury, appears not only from the cases recorded by Morgagni, Quesnay, Morand, but from those given by Dease, Schmucker, Abernethy, Dr. Thomson, and Dr. Hennen. This fact, is particularly illustrated by the cases of Schmucker, in which it is quite common, after injury of the head, to find more or less of the brain reported soft, pulpy, and dissolved.‡

\* Josephi Baader, Observat. Med. Obs. 32. Ext. apud Sandefort Thesaurum, Voi. iii. p. 28.

† De Febre, anno 1783–84. Montereale Epidemica, Auctore Marco Antonio Jemina, M.D. &c. Extat in Brera Sylloge, Vol. x. p. 218, 247.

‡ “Die darunter liegende *substantia corti-*

That the same change may take place in consequence of the presence of tumours, may be shown from a case given by Meckel, from one by Dr. Yellowly, from two of Dr. Powell's cases, and from others in the works of Bonetus and Lieutaud. In several cases noticed by Morgagni it appears to have existed for a considerable time. (Epist. ix.) From these circumstances, it may be doubted whether the opinion of the present ingenious author, regarding the resemblance between pulpy destruction of the brain and gangrene, is in all cases applicable. So far as the part of the brain is disorganized it may be said to be dead; and regarding it, in this sense as gangrene, it would not be easy to prove, that, as a consequence of acute inflammation, it was not so. But when it is of long duration, as is the case in tumours, and other organic changes, it is difficult to suppose that part of the brain is dead during any time of the life of the individual. On these objections, however, which we state as difficulties occurring in our apprehension to this view, lest we give them too much weight, we dwell no longer.

On the semeiology of this morbid change, we were much pleased to find Dr. Abercrombie differing entirely from the French writers who have treated this subject. By most of these, pulpy destruction has been represented as attended with, and distinguished by, tonic spasms and twitches of the muscles of the extremities. From cases which have fallen

*calis* des Gehirns war aufgelosst (dissolved;) und gab einen faulen Geruch von sich."—"Bemerkte ich, dass ein Theil des Gehirns sehr verändert war, und bey der Berührung ward ich eine bewegliche Materie gewahr. Ich ofnete diesen Ort, und fand eine hoele, in welcher, uber ein Loth dickes zahes geronnenes Blut enthalten war; unter demselben war die *substantia medullaris* des Gehirns in eine gelbe weiche Feuchtigkeit (obngefahr von der Art, wie sie in einem *meliceride* gefunden wird) verwandelt. Die ganze innere Flaechen der Hoele, war verfaut, und in eine braune Gauche aufgelosst." Chirurgische Wahrnehmungen, Von Johann Leberecht Schmucker, Konigl. &c. Erster Theil, Berlin, 1774.—"The whole right hemisphere seemed to be reduced into a pulpy and fetid mass, composed of a mixture of blood and brain." Abernethy, case xi. p. 51.—"We found the right hemisphere of the cerebral mass reduced to a sort of bloody pulp, still retaining some shape of what it had been, but much diminished in size, and rendering it absolutely impossible to trace its organization." Hennen, p. 43.—"He died at the end of six weeks; and the whole hemisphere of the brain, on the side of the injury, was found converted into a soft reddish substance." Report of Observations by John Thomson, M.D., &c. Edin. 1816. p. 55.—"On the death of the patient, nearly the whole of the right hemisphere was found converted into a soft pulpy mass." Ibid. p. 59.

under our own observation, as well as from those which we see recorded by Morgagni, this inference appeared to us precipitate, and derived from partial observation only. Dr. Abercrombie thinks there is no foundation for it, and remarks that the change in question seems to be distinguished by no uniformity of symptoms. It is indeed to be observed, that no morbid change is found connected with such varied, and sometimes opposite assemblages of symptoms. It is worthy of remark, that in many instances, particularly in the chronic form of the disease, it causes little or no sensible derangement in the muscular motions, and is on the contrary sometimes the cause of fatuity, idiotism, or mental imbecility. This is especially remarked in that form of mental imbecility which succeeds the *coup de soleil*, of tropical countries. In other cases, again, we have seen it connected with hemiplegia and impaired memory; and in several instances, it is the only morbid change found in the brain of epileptic subjects, in whom there were distinct intervals of health. Morgagni himself, states, that it was found in the gray matter of the brain of Marchetti, the anatomist, who, after two epileptic attacks, died apoplectic. (Epist. vii. 14, 15.) Dr. Abercrombie represents them to be attended by convulsion more or less extensive, followed by palsy and coma; but admits that in some instances others are destructive of the cerebral substance, without either palsy or convulsion, and even without coma. It is by no means improbable that subsequent observation may show that this want of uniformity, and the varied character of the symptoms, depends in a great degree upon the part of the organ which is disorganized. It is also to be kept in mind, that many of the symptoms depend, not so much upon the destruction of the part, as on the vascular irritation of the contiguous cerebral substance, resulting from the morbid injection which terminates in the change.

The various forms in which the termination by suppuration takes place, may be referred to three; 1st, an irregular cavity, such as that delineated by Baillie and Hooper, in the substance of the hemispheres; 2d, a small abscess in some part either of the anterior or the posterior lobe; and, 3d, an abscess of various size, but enclosed in a firm cyst. The two former are doubtless the result of acute inflammation. The latter is perhaps, in most cases, the result of a chronic form of the process. Another variety of puriform cavity is that which is occasionally the result of a broken down or suppurated tubercular mass,—a good example of which is given in the work of Dr. Monro, noticed in last number.

From the researches of the present author, it appears that the formation of these collections is attended with not less diversity of symptoms than that of pulpy destruction. It has been indeed frequently remarked by various authors, that purulent collections take place in various parts of the brain, without ever giving rise to conspicuous symptoms; while, conversely, very severe symptoms have

risen from very slight and partial effusion of matter. In the elaborate paper of M. la Peyronie may be found examples of suppuration, either spontaneous or from injury, in almost every part of the brain, without giving rise to any symptoms which were sufficiently urgent to attract notice, till the last hours of life, when life was suddenly terminated by convulsions or coma. The writings of surgeons abound with similar cases, from Morand and Quesnay, down to Abernethy and Sir Everard Home.\* Dr. Abercrombie inclines to the inference, that in the encysted abscess these symptoms are more distinct and severe; and he adduces a variety of instances from Morgagni, Lallemand, Broussais, Bateman, and Powell, to show, that though in these cases various derangements of sensation and locomotion take place, yet it is impossible to trace any uniform connexion between the extent of the lesion and the external symptoms. Much the same may be predicated of those examples of abscess of the cerebellum which have been recorded by Bianchi, Janus Plancus, Stoll, Douglas, Weikard, and the author of the present work.

Ulceration of the surface of the brain, which is properly regarded as rare, has not been seen by Dr. Abercrombie. So far as we are aware, it was first distinctly noticed by Ridley, who saw it destroy the outer gray matter of the convoluted surface without pain or affection of the senses.† Dr. Anderson, of Leith, saw it, after long continued pain of the crown of the head;‡ and Powell, in like manner, met with superficial ulceration of the anterior part of the right hemisphere.§ Most frequently, however, it has been seen in the foliated surface of the cerebellum. Thus Haller mentions an eminent seal-engraver, who perished from an ulcer of the cerebellum, after slow loss of the senses and fatuity.|| Maximilian Stoll, found an instance of it in the cerebellum of a young man of 20, accompanied with redness, thickening, and erosion of the *pia mater*;¶ and two cases of the same character are recently recorded by Scoutetten.\*\* From the circumstances of the necroscopic appearances in these cases, it may be stated, as a general result, that ulceration of the convoluted surface of the brain, or the laminated surface of

the cerebellum, is an effect of circumscribed inflammation of the *pia mater*.

Chronic inflammation of the cerebral substance, which forms the last subject of examination in this section, may prove fatal in the inflammatory stage without suppuration, or it may cause induration of part of the brain, which may then be dissolved in unhealthy suppuration. Induration of the brain has been noticed by Morgagni, and particularly dwelt on by Meckel as a cause of mental derangement in general, and fatuity or imbecility in particular. The latter author, in a selection of interesting cases described in the Memoirs of the Academy of Berlin, found the substance of the organ not only firmer and harder than natural, but, though more elastic, actually diminished in specific gravity, a cube of six lines diameter being upon the average two grains lighter than a similar cube taken from a sound brain. From his incidental remarks, it is manifest that Meckel ascribed this change to a degree of compression, or condensation, resulting from the vessels being inordinately loaded; and it is further manifest from the necroscopic reports, that the cerebral vessels were in a state analogous to inflammation. (Obs. 7, 8, 9, 10.) He, however, allowed his observations to be biased by hypothetical notions on the animal spirits and mechanical views of their compression. Lately, M. Serres has advanced an opinion much the same as that which is mentioned by the present author, viz. that the vascular injection which constitutes apoplexy not unfrequently terminates in chronic inflammatory action and induration of the brain. The view which Dr. Abercrombie gives of its nature is ingenious, and, so far as we can perceive, just. He regards it as the result of low scrofulous inflammation, that is, we presume, chronic vascular congestion occurring in subjects termed scrofulous; and that, though in the early stage it may be under the control of remedial means, the formation of a cyst of coagulable lymph, or its gradual induration give it the character of organic and unmanageable disease. The natural termination of this change, unless the patient is cut off by it early, is represented to be suppurative softening or destruction of the part, or, if near the surface of the brain, the form of ulceration already mentioned.

Inflammation of the central parts, by which are meant the *fornix*, *septum lucidum*, and *corpus callosum*, is illustrated at considerable length, and with much accuracy. According to Dr. Abercrombie it occurs under two different forms; inflammation of the membrane lining the ventricles, by which we imagine is meant the epithelion of these cavities; and inflammation of the white cerebral substance of the *corpus callosum*, *fornix* and *septum lucidum*. It may be doubted whether this distinction holds good; for the difference appears to consist simply in the inflammatory action spreading, in the one case, along the surface, and in the other, sinking into the substance of these parts. Again, it may be doubted whether the inflammatory action of the surface is

\* Transactions of a Society, &c. Vol. iii. p. 94, and Observations on the Functions of the Brain, by Sir Ev. Home, Bart. V.P.R.S. Phil. Trans. 1814, p. 469.

† Observationes quædam Medico Practicæ, &c. Auctore H. Ridley, M.D. &c. Lugduni Bat. 1750. Obs. 34, p. 212.

‡ Transactions of Royal Society of Edinburgh, Vol. ii.

§ Transactions of College of Physicians, Vol. v. Case 6.

|| Elementa Physiologiæ, Vol. iv. lib. x. sect. viii. § xxxvi. p. 351.

¶ Ratio Medendi, pars 3, p. 122.

\*\* Archives Generales, Tom. vii. p. 31.

in any case confined to that without affecting the parts, or conversely, whether, when seated in the substance, it does not affect the surface. Thus, when the *septum* is softened and perforated, or even broken down, which Dr. Abercrombie justly represents as one of the effects most frequently characteristic of the second form of inflammation, we invariably find in the ventricle fluid, sometimes limpid, sometimes turbid, which the author also regards as the product characteristic of the former affection. The same may be said of destruction of the fornix, which is never, so far as we have seen, unaccompanied with more or less fluid in the ventricles. Conversely, when fluid is effused into the ventricles, the *septum* is very frequently, if not always, reticular, perforated, or actually broken down. The only exception to this is, that fluid may be found in the ventricles without much injury of the fornix, and with none of the mesolobe. It must nevertheless be admitted, that the author adduces cases of effusion into the ventricles without destruction of the central parts, and destruction of the central parts without effusion. To counterpoise this, however, he admits that the combination of the two is the most frequent form of the disease named acute *hydrocephalus*, and of this the greater part of his cases are instructive examples. The four last cases which he adduces as instances of simple effusion we are disposed to regard as the effects or products of inflammation of the choroid plexus, which indeed, from what has fallen under our observation, is the most frequent source of watery effusion into the cerebral cavities, especially when that effusion is unaccompanied with pulpy destruction of any of the contiguous parts of the cerebral substance.

In investigating the nature and effects of this preternatural effusion of fluid, Dr. Abercrombie directs the attention of the reader to a pathological question of the utmost practical importance. Many cases recorded by a variety of authors of very competent observation and unquestionable veracity, show that serous fluid may be effused into the ventricles of the human brain without producing any very marked symptoms either in sensation or locomotion, and certain with none in the actions of involuntary motion. Conversely, in not a few cases in which all the symptoms said to denote oppression and compression of the brain have been present, not a particle of fluid has been found; or, if any was present, its quantity bore no proportion whatever to the intensity of the symptoms which during life it was imagined to have caused. The natural inference from these facts, the accuracy of which all persons who have had any experience in observing disease and tracing the state of the organs in the dead body will verify, is, that the convulsions, coma, and other forms of deranged sensation and motion, are the effect not of the effusion, but of the morbid process, which terminates in the effusion; that the presence or the degree of effusion is indicated by no uniform symptom or

symptoms; that death itself is in all probability more frequently the result of this previous morbid process; and that these symptoms are connected with a state of the meningeal capillaries, doubtless inflammatory, which, if attacked with prompt and energetic measures, it is within the power of the physician to control and subdue.

Upon the knowledge of the facts and inferences now adduced, the only question is, whether *hydrocephalus* has been or may be cured? and Dr. Abercrombie properly enough resolves it, by saying, that cases which present all the symptoms known to indicate the disease, we presume in its inflammatory state, have recovered under the use of proper remedies. As to the removal by absorption of the fluid when actually effused, which the learned author seems disposed to believe may take place, we fear no satisfactory proof of the fact can be adduced; and it may be doubted whether, considering the destruction of the contiguous parts, it would avail. It is manifestly not, as mere effused fluid, the cause of death; and the fatal event is determined according to the properties and laws of the animal textures before this effusion has taken place.

The ordinary causes, and the most successful method of treatment, are well illustrated in the seventh and eighth sections, with which the first part of the work is concluded.

We have already adverted to the defects of the mode of division adopted by the author of the present work; and of this the appendix to the part now terminated is an unfortunate example. It is possible, certainly, that the second and third sections, on affections of the cranium and pericranium, may be properly enough referred to this situation. But, with deference to the judgment of the author, we cannot refrain from thinking that the first section, the subject of which is tubercular disease of the brain, would have occupied a much more suitable station in the third part, among organic changes. It is indeed scarcely possible to imagine a more complete example of organic disease than tubercular disorganization. It is true, as the learned author states, that tubercular growths are allied to inflammation, both in the changes which they induce in the circulation of the adjoining texture and in the symptoms. But of what organic change may the same quality not be predicated at some stage of their progress? These remarks are submitted with that respect for the judgment of the author, to which his accurate habits of investigating the nature of disease, show that he is justly entitled. The cases adduced are excellent and instructive illustrations of the general and particular characters of tubercular deposition of the brain, and should be minutely studied by those who wish to be familiar with this modification of organic disease. If we have occasion to refer to them, it shall be done under that head.

The peculiar destruction of the inner table of the cranium, mentioned in the second section of the appendix, is doubtless singular, in so far as the morbid action was not confined

to one spot, but was diffused over the whole inner surface. A case somewhat similar, but not advanced to the same degree, we have seen. In this the inner table was in the progress only of absorption, being removed to some depth over a very great extent of the skull, and with portions of the original bone left above the absorbed surface. In this case the agents of absorption were the vessels of the outer or cranial surface of the *dura mater*. The man had received a blow on the head some months before. Much the same sort of action appears to have taken place in the case recorded by Dr. Robert Evans, in the present number, (p. 288.)

Dr. Abercrombie justly opposes the doctrine inculcated by some surgical authors, that in cases in which the bone becomes diseased after wounds and injuries of the head, it is caused by detachment of the *pericranium* or *dura mater*. It is indeed by no means uncommon for the former membrane to be completely detached from the skull, yet without causing either death or caries of the bone. We have seen even a crop of healthy granulations arise from the exposed surface under those circumstances; and the same, doubtless, must have occurred to others. The inflammatory action consequent on the injury, is justly regarded by the author as the probable cause of caries or death of the bone. It may be further said that this result is most likely to take place when the injury is so severe, or the consequent inflammation so general, as to affect at once the *pericranium* and the *dura mater*. To speak of the affections proper to the former membrane is here superfluous.

The second general division of the work on *apoplectic affections*, is introduced with some general observations on the manner in which the apoplectic seizure takes place. The three heads to which the author refers these several forms of invasion, might perhaps have been rendered more clearly intelligible had they been connected with the pathological states of the organ on which they depend. But as the author must have seen reason for avoiding this, either in its inexpediency or its impracticability, it is unnecessary to urge the matter further.

One of the points in the pathology of apoplexy which has most puzzled physicians, and given rise to so many discordant statements, and so much difference of opinion regarding the treatment to be pursued, is that of cases to all appearance completely apoplectic, that is, with loss of sense and motion, terminating fatally, yet without extravasation of blood or any of those appearances which, since the time of Wepfer, have been conceived necessary to produce apoplexy. The undeniable occurrence of examples of this description furnished ground for the establishment of a form of the disease called *nervous apoplexy*,—a distinction which was in 1780 formally admitted by Zuliani of Brescia, and Kortum of Dortmund, in 1785, and which was some time after recognised in this country by Kirkland, and more recently by Abernethy. In this it

was imagined that apoplectic symptoms might occur, independent of any lesion of the brain or its membranes appreciable by dissection, and purely in consequence of a torpid condition of the organ, or a suspension of its nervous energy, generally to be traced to disorder of the gastro-enteric organs.\*

The present author, without adopting the views of those now mentioned, admits, however, a similar form of the disease, to which, as taking place *without any morbid appearance*, he applies the appellation of *simple apoplexy*; and in confirmation of which he relates four cases of apoplectic death. In the first and fourth of these, indeed, it is stated no abnormal appearance was recognised. In the second, the meningeal vessels were inordinately distended; and in the third, not only were the veins turgid, but the cerebral vessels were highly injected. That the appearances in the latter were adequate to account for the fatal event, we shall presently attempt to show. The former we confess our inability to explain, unless upon the same principle on which, in the bodies of persons cut off by inflammatory symptoms, the absence of distinct traces of the morbid action is accounted for. The morbid action in such circumstances, considered as a living process which had not advanced far enough to generate morbid products, has been supposed to disappear with life;—an argument which deserves some support from the fact, that, even where the morbid products are distinct, the change in organization which had led to it is occasionally very obscure, or even almost equivocal.

Without entering minutely into the merits of this distinction, for the proofs of which we refer to Zuliani and Kortum, we think there are good grounds for believing that it rests on a mistaken notion of the pathological cause of apoplexy. Wepfer, the first accurate writer on the morbid anatomy of the disease, laboured to establish the general inference, that extravasation of blood was necessary to constitute apoplexy;† and to this idea, which was further extended and confirmed by the researches of Bonetus, Hoffmann, Valsalva, Morgagni, and others, may be referred much of the misconception which has prevailed on this subject. When the idea of the necessity of actual extravasation had once occupied the mind, it was difficult, if not impossible, to explain cases of apoplectic death which took place without extravasation.

That this notion of the necessity of extravasation is gratuitous, may be inferred from the following facts and considerations. In the first place, it is certain that complete apoplexy may take place from mere injection only, either of the meningeal or of the cere-

\* F. Zuliani de Apoplexiâ præsertim Nervæ. Lipsiæ, 1780; and Car. G. Theod. Kortum de Apoplexia Nervosa. Gottingæ, 1785, Apud Ludwig, Tom. iv.

† Joh. Jacobi Wepferi Historiæ Apoplecticorum observationibus, &c. illustratæ. Amstelædami, 1724, pp. 210, 211, &c.

bral blood-vessels. Thus not only Morgagni, but even Wepfer and Valsalva, strong as their predilection for the necessity of extravasation was, met with cases in which none had taken place. The physicians of Breslau also in their reports, and Quarin in his commentaries on them, remarked as an extraordinary fact, that apoplectic death might occur without extravasation. The possibility of this was admitted by Cullen and Rochoux, and necroscopically demonstrated by Baillie. "The milder forms of apoplexy," says this physician in his posthumous observations, "depend upon a distention of some of the vessels of the brain, from undue accumulation of blood in them. I have known, however, *one instance of fatal apoplexy* where many of the blood-vessels were found, upon examination after death, to be much distended with blood; but no blood had been extravasated in any part of the brain."\* Secondly, the experiments of Serres on living animals show that, conversely, extravasation of blood may take place to a considerable extent without inducing apoplectic symptoms.†. Thirdly, it has been shown by the same physician, and must have been observed by many besides, that the apoplectic symptoms disappear while the extravasated blood remains. Fourthly, the apoplectic symptoms bear no relation or proportion to the quantity of blood effused; complete and distinct apoplexy being often associated with the extravasation of a minute quantity, while copious extravasation is not unfrequently connected with very mild and temporary apoplectic symptoms.

From these facts it is to be inferred, that extravasation of blood is not necessary to constitute apoplexy; and that the essential anatomical character of this disease is injection and distention either of the meningeal or of the cerebral vessels, or of both. It is manifest that in all cases of extravasation, except those from rupture of a large trunk, this *must* be present; whereas its termination in extravasation is merely a probable effect of the previous injection. It may be further demonstrated, that, in the cases of serous effusion with apoplectic symptoms, commonly named serous apoplexy, the true pathological cause is not the effusion, but the vascular meningeal injection from which effusion arises. This doctrine, which was obscurely conjectured by Morgagni, was distinctly stated by Stoll,‡ fully established by Portal, and newly confirmed by M. Serres and the present author.

It must, nevertheless, be regarded as a defect in the general pathological views of Dr. Abercrombie, that, although perfectly aware of the influence of vascular injection in producing serous effusion, so far as we can disco-

ver, he no where states distinctly the doctrine, that vascular injection alone is adequate to produce, and actually does produce, apoplectic symptoms. We are perfectly aware, that he no doubt felt himself bound to admit the force not only of his own cases, but also of those of Dr. Stark and Dr. Powell. Of the four cases of Dr. Abercrombie we have stated our opinion above. That of Dr. Stark we have always regarded as an example of apoplectic symptoms, depending on vascular injection of the *medulla oblongata* and spinal cord; for though the ingenious author himself admits that the cord was not examined, he states, "that about an ounce or more of bloody fluid ran out from the spinal canal;" a circumstance, it may be remarked, which does not take place unless where the vessels are much loaded. The case of Dr. Powell is certainly more perplexing; and, like the first and fourth of Dr. Abercrombie, we shall not attempt to explain it. But to all these cases the general objection applies,—that as the dissectors, under the impression of the doctrine of Wepfer, always looked for extravasated blood, when they failed to find this, they therefore said that there was no morbid appearance. It is manifest, that, as what is morbid appearance must be determined in relation to what is sound, it is not easy, in any given case, to draw the line of distinction with positive certainty.

But even after the necessary allowances for this source of fallacy, another, by no means either unimportant or imaginary, demands consideration. We allude to that class of cases which have been assembled by M. Serres, under the head of *meningeal apoplexy without effusion*. We are not unaware that some physicians overlook the distinctions of this ingenious pathologist, or hear them mentioned with an incredulous sneer, as the imaginary creations of a wild theorist. To such these remarks are not addressed; nor is it requisite to reason where the ground is preoccupied by prejudice. To those, on the contrary, who are open to conviction, it may be necessary to say, that M. Serres shows distinctly that apoplectic symptoms may, and often do arise from mere vascular injection of the proper cerebral membrane, without effusion, either serous or sanguinolent. Of this description are his 16th, 17th, and 18th cases, in which the vessels of the *pia mater* were more or less injected without other morbid change. Similar cases must have occurred to several practical observers; and we submit to the ingenious author of the present work, whether the 2d and 3d of his cases may not be referred to this head.

For these reasons, it may, without the imputation of trivial objection, be justly questioned, whether there is ground for the nervous apoplexy of Zuliani, Kortum, and Kirkland, or by parity of reason, for the simple apoplexy of the present author. As the matter at present stands, it is wisest to adopt the side which

\* Lectures and Observations on Medicine. London, 1825. p. 167.

† Nouvelle Division des Apoplexies, par M. Serres; Annuaire Medico-Chirurgicale des Hopitaux et Hospices Civiles de Paris. A Paris, 1817. 4to. pp. 241, 260, 261.

‡ Ratio Medendi, Pars I. p. 138.

\* Works of the late William Stark; M. D. &c. Part iv. sect. iv. p. 79.

does not recognise this form of apoplectic disease.

On this subject we have found it our duty to dwell at so great length, that our observations on the others must be brief. The first, indeed, that of apoplexy with bloody extravasation, will not require particular remark. The cases and observations of Dr. Abercrombie illustrate the old doctrine of Wepfer and Morgagni. One or two points only may be brought forward.

The sources of hemorrhage are by Dr. Abercrombie referred to the following heads: 1st, The deep-seated or the cerebral vessels; 2d, the superficial or the meningeal vessels; 3d, ulceration or rupture of an arterial trunk; 4th, the vessels of the choroid plexus; 5th, rupture of a sinus; 6th, rupture of small aneurisms in various parts of the cerebral vessels; and, 7th, from erosion of a vessel between the *dura mater* and the bone, in consequence of caries of the latter. Pathologically this division errs by its multiplicity; and it is further not a little confused. Hemorrhage from the choroid plexus belongs to the same general head with that from the meningeal vessels; and rupture of the aneurismal sac of a cerebral vessel cannot be so different from that of ulceration or rupture of an arterial trunk, as to require a separate head. Of the sources of hemorrhage within the head, a more natural arrangement would be the following. I. Those hemorrhages which depend on previous capillary injection, and take place from numerous minute arteries; the *active hemorrhages* of scholastic writers. Of these there are two sorts, 1st, hemorrhage from injection of the cerebral vessels; and, 2d, hemorrhage from injection of the meningeal vessels. II. The hemorrhages resulting from rupture, laceration, or erosion; 1st, of an artery, either aneurismal or simply diseased; 2d, from a vein or sinus, whether by disease or by laceration;—the *passive hemorrhages* of authors.

Dr. Abercrombie very properly shows, after Dr. Cheyne, that it is impossible to trace the extravasation to particular vessels, and that many capillaries are found to open into the cavity in which the blood is effused. When it takes place from the deep capillaries of the substance of the brain, which is most frequent, it constitutes the true *cerebral hemorrhage* of Hoffman, the *cerebral apoplexy* of Serres.

The second form mentioned by the author, which must also be regarded as an active hemorrhage, is that which results from injection of the capillaries of the proper cerebral membrane,—the *pia mater* and choroid plexus. It presents two subdivisions, according as the hemorrhage occurs on the convoluted, or on the central surface of the organ,—from the *pia mater*, or the choroid plexus. This corresponds with the meningeal apoplexy, *avec epanchement sanguinolent*, of M. Serres, (svi. Cases 20th, 21st, and 22d, from Wepfer.)

Under the first subdivision, or that of hemorrhage from the *pia mater*, in addition to the cases communicated to the author by Dr. Hunter and Dr. Barlow, we may be allowed

to add, that a most distinct and instructive example of the disease is given by Mr. Howship in his 14th case. It occurred in a young woman of 22, who for two years had suffered much from rheumatic ailments, and had at length, after paralytic and vertiginous symptoms, died lethargic. Upon inspection, the *pia mater* was found vascular and red; its vessels increased in number and size; and blood was diffusely extravasated all under the *pia mater*. "The extravasated fluid had formed superficial *coagula*, corresponding to the *sulci* between the convolutions of the brain." "It had taken place very universally, and the effusion seemed to have arisen not only from the capillary arteries upon the external surface of the *pia mater*, but also from those processes of the membrane which dip down between the convolutions forming the *tomentum cerebri*. Several of these deeper seated *coagula* were divided by the knife in the course of the dissection.\*

To the second subdivision, or hemorrhage from the choroid plexus, are to be referred those instances of sanguinolent or bloody extravasation which take place in the ventricles without rupture of their *parietes*. In ordinary cases of cerebral hemorrhage it is not uncommon to find in the ventricle masses of clotted blood, which may always be traced to laceration of the roof, the side, or the floor, by blood issuing from an apoplectic cell in the adjoining cerebral substance. In this, however, the fluid is rather sanguinolent than pure blood; and there is no laceration of the cerebral substance which forms the floor or the wall of the ventricle. As in the serous membranes, it is a hemorrhage by exhalation. In the case of De Haen, mentioned by almost all authors who have written on this subject, the fluid, which was blood, escaped by rupture of the vessels of the plexus.

On the passive hemorrhages it is unnecessary to dwell. We formerly took occasion, when speaking of the researches of MM. Bouillaud and Serres, to examine somewhat at length this source of hemorrhage, as occurring from morbid arterial trunks. It is manifest that, in pathological nature, it differs much from the genuine cerebral hemorrhage of apoplexy; and this difference constitutes a most material practical one. Extravasation from rupture of the lateral sinus we have seen once only occasioned by external injury of the head.

The influence of the morbid state of the arterial tissues of the brain, which has occupied the attention of Morgagni, Vater, Scarpa, Blane, Baillie, Hodgson, Howship, Bouillaud, and Serres, receives also a portion of attention from Dr. Abercrombie. Though he does not so positively as Bouillaud argue for arterial rupture arising immediately from disease of the

\* Practical Observations on Surgery and Morbid Anatomy, illustrated by Cases, with Dissections and Engravings. By John Howship, Member of the Royal College, &c. London, 1816. Section ii. Case 14.

tunies, he admits that this morbid change may alter the circulation so much, as to induce serious complaints within the head. From the cases recorded we think it a legitimate inference to say, that it often produces vascular injection, which may terminate in serous effusion or bloody extravasation, according to circumstances. This is clearly established by the facts formerly adduced, and more especially by the dissections of Mr. Howship, which, next to one or two by Hodgton, are perhaps the clearest and most pointed on record. (Cases 17, 18, 19, 20.) In each of these cases, though the arterial trunks of the brain were extensively and seriously diseased, the cerebral hemorrhage had taken place, not from the diseased trunks, but from the capillaries in the substance of the brain.

This derives further confirmation from what is stated by Dr. Abercrombie in the second division of this subject, to which we now proceed,—paralysis with serous effusion, bloody extravasation of limited extent, or pulpy destruction. Each of these states he shows to be generally connected with an unsound condition of the cerebral arteries, often affecting them to a very considerable extent.

Of palsy in general, the natural history and pathology are delivered at great length, and with much accuracy; and the enumeration of the pathological states of the brain and its membranæ, with which the several degrees and forms of this disease are connected, is evidently derived with much care from an extensive collection of facts. From all these the general result may be stated to be, that, though palsy is an effect of any change in the organization or structure of the central nervous mass, many of these changes may exist without producing it. It is probable, therefore, that the pathologist has still to look for some general pathological circumstance which is common to the whole of these, but the operation of which may be prevented from taking place by circumstances with which we are yet unacquainted. This, it must be admitted, is general, if not vague and unsatisfactory. But it does not appear from the researches of the present author, that it is possible to make a nearer approach to the precise and universal pathological cause of this malady.

The appendix to part second contains some ingenious conjectures regarding the circulation of the brain, and the changes which it is liable to undergo. From the experiments and observations of Dr. Kellie, with other considerations derived from the anatomical peculiarities of the parts within the skull, the author regards it as highly probable, that, in the ordinary state of the parts, no material change can take place in the absolute quantity of blood circulating in the vessels of the brain. The blood moving in these vessels, nevertheless, he thinks, being divided in a certain ratio between the arteries and veins, may be liable to sundry alterations in this ratio; and upon the accurate adjustment of the circulation in these two systems, he conceives the healthy state of the organ may mainly depend. In his

further observations on this subject, he attempts to trace generally the several modes in which, in an organ circumstanced as the brain, derangement of the circulation may be supposed to take place.

The first of these is from the general plethoric state of the person, in which the vascular system at large is understood to be distended with an inordinate quantity of blood, and in which, consequently, the cerebral vessels may be supposed to partake of the general injection.

The second cause of this nature Dr. Abercrombie thinks may be found in any circumstance which may interrupt the return of the blood from the veins of the brain. Little doubt can be entertained that on certain occasions this cause may operate; and the author is perhaps not much mistaken when he suggests the possibility of its taking place in that form of the disease which he names simple apoplexy. It is nevertheless so seldom that the actual existence of the cause can be recognised, that we think it impossible to assign it more than a partial and a probable influence.

To the third cause of this description, diminution of capacity of the venous system, the same observation is applicable. Though in a few rare and curious cases it is possible to recognise some circumstance which may operate in diminishing the area of the *sinuses* and the venous tubes of the brain, in a much larger proportion the difficulty of this is daily felt by the anatomist.

Under the fourth head, that of unusual inanition of the vascular system in general, and of that of the brain in particular, Dr. Abercrombie advances several ingenious conjectures, and illustrates them by reference to cases. Whatever be the precise and intimate state of the vessels in cases of this description, we have had occasion more than once to witness the general accuracy of these observations remarkably verified.

Of these observations the result is, that in tracing the origin of hemorrhage, or rather we should say *hemorrhagic injection* of the brain, much remains unexplained; and the attempt to give any thing like a rational or sufficient theory is beset with inconsistencies so innumerable as to perplex the more accurate inquirer, and difficulties so great as to seem almost insurmountable. This evil, indeed, is not peculiar to the pathology of the cerebral circulation. But while it shares this in common with the vascular system of every texture in the animal body, it possesses peculiarities, which, if we knew their exact influence, may tend remarkably to increase the difficulty of perceiving how the derangement is effected. While we agree with the ingenious author in admitting that the absolute quantity of blood within the cavity of the skull may not be increased, we must admit our incapacity to perceive that this is the question at issue; and it is further very questionable whether that of the balance of circulation between the arterial and venous tubes is requisite in the investigation. In all the instances of deranged

circulation in the textures within the cranium, as elsewhere, whether terminating in inflammation or its products, serous effusion, or bloody extravasation, the morbid state consists in an inordinate quantity of blood being in the capillaries of the organ or texture. These capillaries are, anatomically speaking, neither arterial nor venous tubes; but an extensive and multiplied net-work of innumerable minute canals, connecting the two systems, and partaking the characters of neither. The question, therefore, comes to be, not what subverts the balance between the circulation of the arterial and the venous tubes, but what are the circumstances under which the capillaries of the part become inordinately filled and distended with blood? What are the agents, in short, by which these capillaries are thus injected? Whoever answers this question, resolves the problem. Whoever does not, must be held, we imagine, to fail, as all others have failed, in explaining the mode in which derangements of the capillary circulation of the substance of organs take place. Our inability we willingly admit.

It may be remarked, nevertheless, before we quit the subject entirely, that two points in the investigation may seem to merit some notice. One is, whether the blood moves more slowly than in the state of health, or, as some have imagined, entirely stagnates in some of the vessels? The other is, whether the large vessels and trunks contain their natural proportion of blood, or less than that; whether, in short, they are robbed to supply the distention of the capillaries? The first of these questions is no doubt connected with the circumstance of distention, and perhaps may prove to be either a cause or an effect of it. The second, however, is not necessarily connected with it, and may be safely thrown aside altogether, so far as we at present understand the inquiry. These questions this is not the place to prosecute further; and we proceed to the third general division, that of organic diseases.

Of these Dr. Abercrombie does not attempt any arrangement. They may be conveniently distinguished into organic changes, incident 1st, to the membranes, and 2d, to the brain.

The membranes may be simply thickened, and somewhat indurated. The occurrence of this, from deposition betwixt the laminæ of the *dura mater*, is the only example mentioned by the present author.

A whitish semipellucid substance, presenting the character of albumen, may occasionally be seen in undefined masses beneath the arachnoid. A substance not dissimilar, generally attached to the *dura mater*, or some of its productions, may perhaps be referred to the arachnoid covering which extends over the free surface of that membrane.

To the same head belongs the fungous tumour of the *dura mater*, the history of which has been investigated at great length, and with much accuracy, by M. Louis,\* and other

authors. Of this examples are common in collections. Patches of ossification are very frequent in the *dura mater*, and, when found in the brain, are generally connected with the membranes or its productions.

Of tubercles in the *dura mater*, a good example is given by Dr. Hooper in his sixth and seventh engravings.

Tubercles are found in the *pia mater*, and especially its cerebral prolongations.

That the cerebral substance is liable to induration as a consequence of chronic inflammation has been already noticed. Next to the cases of Morgagni, Meckel, and a few by Haslam, the best examples of this change are given in the *Annuaire Medico-Chirurgical*, by M. Hebreart, a late eminent physician of the Bicêtre.\* From these cases it appears that the portion of brain becomes firm, lardaceous, and of a yellowish tint, and loses entirely the usual appearance and peculiar atomic structure of cerebral substance.

Tubercular disease occurring in the brain may often be traced in its origin to the membranes. Thus in the 79th case, p. 173 of the present author, a large tubercular mass, imbedded in the substance of the right hemisphere, was found attached to the falx, in which it has doubtless originated. In the twelfth engraving of Dr. Hooper, in like manner, is given an example of white tubercle projecting into the fourth ventricle, which appears to be attached to the choroid plexus; and in the case described by Dr. Robert Evans in the present number, the tubercular disorganization of the right lobe appears to have commenced in the *pia mater*, while that of the left belongs more distinctly to the cerebral substance, without and above the ventricle. In the first four of the present author's cases, the tubercular disease was confined to the substance of the brain or cerebellum, without involving the membranes. From these cases, it appears in general to consist of a mass or masses of round or spheroidal bodies of various sizes, aggregated together, firm in general, like cheese, and of a white or yellow-white colour. The smallest, varying from a pin head to a pea, are generally connected with the membranes. When larger, that is from the size of a pea to that of a walnut, they are usually imbedded in the substance of the brain or cerebellum. The reader finds their principal characters, anatomical and pathological, well illustrated in the appendix to Part First.

A tumour of a reddish or flesh-colour, and resembling the substance of the kidney, is represented by Dr. Abercrombie as found in

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la Dure-Mere, par M. Louis. Cnez Mémoires de l'Académie Royale de Chirurgie, T. xiii. p. 1. Paris, 1774.

\* Observations sur quelques maladies du cervelet, du cerveau, et de leur membranes, recueillies à l'Hospice de Bicêtre, par M. Hebreart, Médecin Ordinaire des Aliénés, &c. Annuaire, p. 579.

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\* Mémoires sur les Tumeurs Fongueuses de

the brain. It is probable that he alludes to the blood-tumour, (*hæmatoma*,)\* as this is nowhere mentioned in the present classification. The steatomatous and adipose tumours are simply mentioned.

Bony matter, when found in the brain, is almost invariably the effect of osseous deposition taking place in the membranes. Of this manifold instances are given by Bonetus, Morgagni, Lieutaud, Sandifort, and others. The only authentic instance which seems to be one of bony matter found in the substance of the brain unconnected with the membranes is that delineated by Dr. Hooper in the seventh figure of his twelfth engraving. It is unfortunate that the history of this is not given at length; as in this manner only its connexion with the membranes can be determined or disproved.

Under the general name of hydatids, Dr. Abercrombie notices the hydatiform state of the choroid plexus, but properly remarks, that they are merely the cellular texture of the membrane elevated into vesicles by watery effusion. The real hydatocyst, or globular tumour containing watery fluid, he also notices, and illustrates its nature by reference to the case of Mr. Headington, formerly published in this Journal, and now in the appendix. For a good representation of these we refer the reader to the thirteenth and fourteenth engravings of Dr. Hooper.

In the fourth part the diseases of the spinal cord and its membranes are considered in the following order. 1. Acute inflammation of the membranes; (*meningitis spinalis*.) 2. Inflammation of the cord itself, (*myelitis*), terminating in pulpy destruction or suppuration. 3. Serous effusion in the spinal canal. 4. Extravasation of blood in the spinal canal or spinal apoplexy. 5. Fungoid disease and thickening of the membranes. 6. Induration of the cord. 7. Compression of the cord by new formations within the canal, as tubercles, albuminous deposition, hydatids, and ossification of the membranes. 8. Destruction of a part of the cord. 9. Concussion; and 10. Certain affections of the bones of the spine.

Though each of these forms of disease is well illustrated by apposite examples, it is unnecessary here to enter into minute examination of their character and history. Very analogous, if not entirely similar to the morbid processes already mentioned as taking place in the brain and its membranes, any lengthened examination would only lead to tedious if not profitless repetition. On one or two subjects only a few remarks may be offered.

The cases of pulpy destruction (*ramollissement*), of the cord, given by the present author, agree very closely in all essential characters with those published by Pinel Junior, the first accurate writer on this disease, and those

of Oliver and Velpeau, the most recent next to Dr. Abercrombie. The facts connected with the origin and progress of the disease show clearly that it is an effect of inflammation. Yet this inflammation does not appear to be necessarily fatal, at least when seated below the cervical portion of the cord; and when death ensues, it seems to do so indirectly and from another cause, or after it has destroyed a considerable extent of the cord.

The subject of hemorrhage into the spinal cord, or upon its surface, constituting spinal apoplexy, appears to have escaped notice, till the publication of the two interesting cases by M. Chevalier in the third volume of the Transactions of the Medico-Chirurgical Society. In the first of these which, so far as can be learnt from the report, was spontaneous, or without external violence, the hemorrhage was meningeal, since the blood was found between the membranes and the cord. In the second, which appears to have succeeded injury, the hemorrhage was both from the membranes and the substance of the cord, since the former was found inflamed, and the latter dissolved in blood. To these cases Dr. Abercrombie has added one from Morgagni, one from Du Hamel, one from Gaultier de Claubry, one from Olivier, and one from personal experience. The case from Howship, which was occasioned by injury, has been since more fully published in the work of Sir Astley Cooper, on dislocations and fractures, as an example of the effect of injury in producing inflammation and hemorrhage within the spinal canal. It was formerly noticed in our account of that work.\* The case of Du Hamel is the one which Sauvages in an unhappy hour, ranked under the head of *asphyxia spinalis*, and thereby allowed the influence of an ill-chosen name to mislead the physician on the true nature of the disease.†

To collect these scattered examples of this variety of hemorrhage is a service of the utmost importance, for two obvious reasons. In the first place, the features of these cases, by their mutual resemblance, throw a clear and steady light on the pathological characters of the disorder; and by placing them in the same rank with hemorrhagic maladies in general, and those of the brain in particular, enable the physician to understand much more distinctly the pathological relations of an affection which is obscure, indeed, when viewed in an isolated position. Secondly, this verification of the character of spinal hemorrhage leads directly to the appropriate treatment. To M. Chevalier, indeed, belongs the merit of showing, that the disorder, to be treated with the hope of success, should be treated promptly and decidedly as a hemorrhagic disease.

The fifth section, on fungoid disease of the cord and membranes, the sixth on induration,

\* Vide Howship's Observations in Morbid Anatomy, Case 27. Hooper, Plate X. Also the Morbid Anatomy of the Human Brain, by Alexander Monro, M.D.

\* Medical and Surgical Journal, Vol. xxi. p. 396.

† Nosologia Methodica, Classes vi. Ordo iv. gen. xxiv. 15, p. 820.

and the seventh on new formations compressing the cord, correspond entirely to the head of organic changes, and might with propriety have been arranged together. On the subject of concussion and injuries in general some interesting information is given; and in the section on *caries* and other unsound states of the bones, the reader will peruse with satisfaction various instructive examples of affection of the odontoid process of the second vertebra.

With all the elaborate research and generalization of the present author, nevertheless, it is highly perplexing to learn, that in several cases, in which all the usual symptoms of disorder of the cord or its membranes had appeared and continued to the last, neither in the brain nor in the cord could be found any change adequate to account for the symptoms. These anomalies and contradictions Dr. Abercrombie does not attempt to explain; and it is superfluous for us to say, that where he does not attempt we cannot hope to succeed.

The appendix to this part contains a short general sketch of morbid states incident to the nerves. This, of course is not offered for serious criticism; and it must be received by the reader rather as a guide to indicate the leading points of inquiry, than as a comprehensive source of information.

One of the most interesting parts of this inquiry is the knowledge of the history and effects of those minute tumours which are not unfrequently found in nervous trunks, the *subcutaneous tubercle* of Mr. Wood, and the *neuroma* of Chaussier. The excruciating pain which these growths produce is perhaps the least of their evils. Fully a hundred years ago (1720) Dr. Thomas Short of Sheffield showed that a minute tumour of this description, near the lower end of the gastrocnemii was adequate to cause epilepsy;\* and though this was not observed in the cases of Cheselden, Camper, Bisset, Pearson, Home, and Mr. Wood, the accuracy of the observation was recently verified by Mojon and Covercelli,† both of whom found these tumours connected with epileptic fits. It is not improbable, that, as in these cases the peculiar sensation called the *epileptic aura* was felt, the presence of these morbid growths was the material cause of the sensation. Though this is submitted as mere conjecture, we think it is not unseasonable to direct the attention of the practitioner to the fact, as it may enable him to cure a certain class of epileptic cases with more certainty than he could otherwise do.

These remarks, however, it is time to close. Though they have extended to a much greater length than was at first contemplated, we are perfectly aware that they are neither sufficiently lengthened nor minute to do the work of Dr. Abercrombie proper justice. Of the arrangement we have already expressed our

opinion; and this we do with less scruple, first, because in a work, the materials of which are good, defective arrangement appears much more conspicuously than in one of moderate merits; and, secondly, because we feel convinced, as the reasons must be admitted to be satisfactory, that the learned author has too much candour to be offended at our sincerity.

With this single exception, it gives us pleasure to say, that the merits of the work are so substantial as to render the ordinary terms of commendation quite superfluous. The diligence with which the author has laboured to illustrate the obscurities, and reconcile the contradictions with which the subject of diseases of the brain has been, and continues to be, surrounded, entitles him to the gratitude of every physician who wishes to understand one of the most important subjects of his profession. The examples of morbid action, and of its effects which he has recorded in the present volume, both from personal experience and the observation of his predecessors, possess an intrinsic value superior to the casualties of time, place, and opportunity. The importance of these the student of pathology will find little difficulty in recognising; and he will speedily discover, that no individual work in the English language contains a selection of information so useful and judicious upon a class of disorders, upon which the most experienced physicians have daily occasion to deplore the want of precise and accurate knowledge.

From the Medico-Chirurgical Review.

#### DR. MACCULLOCH ON THE PROPAGATION OF MALARIA.

In the leading article of our 15th Number, we gave a pretty full account of the first six chapters of Dr. M.'s very interesting work. We now propose to dedicate a short article to an important chapter on the propagation of malaria, by which we shall render to the public a much more comprehensive analysis of Dr. M.'s researches, than has yet been offered in any other periodical publication.

We observed, in our former article, that, as Dr. M. had not travelled much himself, he had evidently made good use of his library, in the study of medical topography. We have since learned that he is indebted to a gentleman, (who has made a comprehensive survey of the Mediterranean shores,) for some very valuable information respecting malaria, and those places where that poison most commonly prevails. This circumstance, instead of detracting from the worth of the volume, very much enhances its value in our eyes, as giving a greater authenticity to certain details, than if they had been gathered from books.

#### CHAP. VII.—*Propagation of Malaria.*

Next to a knowledge of those localities which give origin to malaria, is an acquaint-

\* Medical Essays and Observations, Vol. vi.

† Memorie della Societa di Genova, und Siebold Chiron, Band i. St. 3, where the Memoirs of Mojon and Covercelli are translated.

ance with the laws by which it is propagated. It is properly remarked, that whatever this miasma may be in its simple state, it is only as united with the atmosphere that we know it. It must, indeed, be considered as the very atmosphere itself, where it exists—and its propagation must, therefore, be primarily regulated by those laws which govern the motions of the air. Unfortunately, we do not know much of these laws. The union of malaria with air, may be more or less perfect, according to the varying conditions of the latter, as to moisture, and other physical qualities—and it is very clear that its propagation is greatly influenced—perhaps totally suspended, or mightily accelerated, by the ever varying atmospheric conditions. Though not so capable of becoming durably attached to bodies as the matter of contagion, yet there are not wanting sufficient proofs that malaria is attachable to certain solid substances, as vegetables, and, perhaps, to the soil.

The first fact which arrests our attention in the propagation of malaria, is *PROXIMITY*. While the atmosphere is quiescent, it may be taken as a general rule, that the place in which malaria is produced, or that which is nearest to it, suffers most. This was probably known to the Romans, from the earliest ages;—and hence it may be, that so many of their ancient towns were situated on hills. Some fatality led to the cite of Rome itself, in despite of this knowledge, if it did then exist. Dr. M. descants feelingly on the want of attention to this fundamental fact in the propagation of malaria, by men in all ages, while selecting places for towns, encampments, or single mansions.

“Rome perhaps became too gigantic during its period of ignorance, to be afterwards abandoned or transferred: but there was no apparent reason for perpetuating Calcutta, when, from the very hour almost, of its foundation by Charnock, its destructive situation had been demonstrated. That Holland should have persisted in inhabiting that Batavia which it had studied to render even more poisonous than nature had already done, by the model of its own pestiferous father-land, is a problem which Holland must be allowed to explain as it best can.”

Innumerable other examples might be pointed out, of the ignorance or obstinacy of our forefathers, in selecting places, that actually courted the grim king of terrors to visit them more frequently than he otherwise would have done. It is chiefly in military service that the ravages of malaria have been conspicuous, and that the want of knowledge, or the want of inclination to listen to the dictates of experience, has entombed thousands in a premature grave! The writings of Lancisi, Zimmerman, Pringle, Lind, Blane, Jackson, and many others, take away all excuse from some of our modern commanders, for the rash manner of proceeding which they have often adopted, and by which armies have been annihilated, expeditions frustrated, and the best laid political projects counteracted.

In Walcheren we lost 10,000 men, and the Antwerp fleet to boot—when the French army attempted Naples, in 1528, they were reduced from 28,000 to 4000 men, in a few days, by choosing an injudicious encampment at Baix! Ignorance, however, might have been better pleaded 300 years ago than now.

“It was a fortunate discovery in fortification, that a dry ditch was more defensible than a wet one; since the safety and efficiency of a garrison seem never to have entered the minds, even of the Vaubans, the Coehoorns, and the Cormontaignes; though far more intimate, it must be supposed, with malaria than ourselves.”

A whole regiment (says Dr. M.) was incapacitated at Malta, and many of our men destroyed, by persisting to occupy a village which the natives had abandoned. A party of 30 men were successively destroyed, by obstinate attempts to occupy, as a telegraph station, a rocky point in Sicily, between Rasaculmo and Spadafora, although we were warned by the natives of the deadly malaria there prevailing.

“Thus also was our hospital at Port Mahon fixed on the precise spot where it received the whole malaria of that pernicious valley, pestiferous during four months of the year; while by choosing the elevation of Cape Mola, at its north-eastern margin, these bad effects would have been entirely avoided.”

The next subject which we are to notice, and which is intimately connected with proximity, is *CONDENSATION* of malaria. It is natural to suppose, that the gradual production of this poison must occasion a gradual accumulation, unless decomposed or dispersed. Thus, we might anticipate that a marsh, confined within the walls of a forest, as in the pine swamps of America, or the marshy ground of a jungle, or even in our own moist woods, would accumulate malaria, and thereby render it unusually virulent. This is confirmed by experience. So, also, a marsh, enclosed between high hills, or otherwise deprived of ventilation, is generally very insalubrious.

“How nearly this general rule may be applied to our own country residences, where uniting stagnant or still waters to the confinement of a woody lawn, it is quite superfluous to say. Those who cannot profit by general principles, but who must, at every minute, have the application made for them, are not of a capacity to profit by any thing.”

And now for the *MIGRATION* or *DISPERSION* of malaria. If currents of air always move horizontally, as weather-cocks or the sails of ships would indicate, the matter might be more easily investigated. But the fact is, that atmospheric currents are irregular and intricate in the highest degree—nay further, they scarcely, in any instance, obey the common law of rarefaction, or unequal density, by which they are supposed to be regulated. If, therefore, we cannot explain how a current of malaria may be directed or limited, so, no movement can occur in this poison, however

unexpected, that may not find its solution in the capricious currents of the atmosphere. If these currents move vertically upwards, so may malaria—if downwards, the malaria may descend. Indeed, both these facts are ascertained. If there be curvilinear courses of malaria, there are curvilinear winds enough to justify such courses. But there are phenomena attendant on the propagation of malaria, which cannot, we fear, be explained, even by the capricious and intricate currents of the atmosphere. One of these is the fact, that a marshy spot of ground will produce disease at some distance, while the inhabitants of the marsh itself will be little affected, or escape altogether.

"In Italy, it has been ascertained that the poisonous exhalations of the Lake Agnano reach as far as the convent of Camaldoli, situated on a high hill at the distance of three miles: this instance farther proving that, thus far at least, malaria can be conveyed by the winds. In France, at Neuville les Dames, above Chatillon on the Indre, and at St. Paul near Villars, both situated on high grounds, there are found as many or more fevers than in the marshes beneath, where the malaria is produced, and the same is generally true all through Bresse in the Lyonnais. Thus also the plain of Trappes near Versailles is affected by the marshes of St. Cyr, though considerably elevated above them.

"I am also informed that a case of this nature occurs in Malta, of a very marked nature; the malaria which is produced on the beach beneath a cliff, producing no effect on the spot itself, while it affects, even to occasional abandonment, the village situated above. Many more similar instances might be collected; but I must be content with adding a few from our own country coming under my own observation, and sufficiently well known to be easily verified.

"At Weymouth, where the back-water, as it is called, produces intermittents, and also autumnal fevers, commonly mistaken for typhus, these diseases scarcely affect the immediate inhabitants of its vicinity, but are found to range along the high grounds above; and the same, in Cornwall, is true of the vicinity of St. Austle, receiving its malaria from the marshes of St. Blaisey. If I am not misinformed, it is equally true of the marsh of Marazion in the same county.

"The marshes about Erith in Kent, also, are less injurious to the inhabitants of the lower grounds near them than might be expected; while their effect on the houses which are situated high on the hill above, is such as, at different times, to have been very severely felt by the inhabitants. The same is true of Northfleet, if my information is correct; or, the fact as stated is, that at some distance, on the high ridge so well known, agues are more prevalent than below and near the point of the production of the malaria. If this is not to be explained by the flow of a current, so directed as to escape the low grounds beneath these cliffs and declivities, while it

ranges across the hills in contact, I have no solution to offer."

Here our author introduces a statement from Captain Smyth's valuable statistical table of Sicily, which appears to generalize the whole of these facts—and lead to the conclusion that, in nearly an equal number of cases, the higher grounds suffer as much as the lower—the locally healthy as much as those which are the very seats of the malaria!

"In this document, out of seventy-six unhealthy towns and villages enumerated, there are thirty-five situated on hills or declivities; while, from his personal information, I may add that many of them are at considerable distances from the tracts which produce the disease. And I may add one remark as to the theory of this propagation, derived from a writer on the climate of Italy. It is, that the southern winds in that country, propagate along the hills, upwards, that malaria which the northern or mountain ones do not; such winds, independently of their superior power in producing the pernicious exhalations, tending, from their temperature, to ascend the acclivities, while the other winds, as is easily understood, have the opposite inclination."

Dr. M. makes many interesting remarks on the propagation of malaria along the valleys, and on the opposite effects produced, in different places, by the same process—the cutting down or the planting of trees. Thus the cutting down of trees, in some places, would let in a stream of malaria—in others, it would keep it out. Hence the topography of places must be well studied, before we venture on any preventive or corrective measures. Thus a convent at St. Stephano, became unhealthy in consequence of cutting down some trees—and the extirpation of a wood brought on severe fevers at Velletri, during a space of three years, as also happened at Campo Salino, in the Pontine Marshes.

We had occasion, in our first article, to advert to the opinion entertained by many, that Rome was, in ancient times, protected from malaria, by groves that were held sacred. Lancisi remarks that, in later times, there was extirpated near Rome, a forest to the southward, reaching from the heights of Fracasti and Albano, to the Tiber, protecting it from malaria so abundantly generated in that quarter by marshes. Thus, says he, was destruction first let in upon the Campagna. Since that date, if Dr. M.'s information be correct, a similar proceeding seems to have opened Rome itself in another quarter, to the malaria of that pernicious land. Dr. M. observes that there was formerly, in a situation interposed between the Campagna and the Porta del Popolo, a wood, cutting off the communication through the north-east winds—and it is, since the destruction of this wood, that the new progress of this pest has been noticed. If this be a fact, it will prove a valuable one, as the Papal government will thus acquire a remedy, as far as this point is concerned, which it has long sought for in vain by drainage.

In whatever way the malaria is generated,

or makes its way into the everlasting city, as progress seems to be determinate, if slow:—spreading, as it were, from a fixed point, and making, in every year, a further step, so as gradually to drive the inhabitants before it—as far, at least, as they are opulent, and able to quit their unhealthy habitations. The progress and various windings of this malaria through the streets of Rome, are traced for Dr. M. by some hand, who was evidently well acquainted with the medical topography of that interesting city. Dr. M. speculates on the fate of Rome, in mournful accents; but the malaria may take another direction, or some revolution in the bowels of the earth may procrastinate the fall of this mistress of the world. Dr. M. thinks, and with great probability, that the decreasing population itself, is one cause of the accelerated march of the malaria, since nothing checks the generation of this poison so much as dense population and high cultivation. Among the mysterious circumstances connected with the generation and propagation of malaria is its partiality for one side of a street, and its repugnance to the other side.

“In Rome, it is pointed out, in more places than one, that the malaria, which must there be transported, not generated, will occupy, even with some permanence, and in some instances also, perennially, one side of a garden or street, while the opposite one remains exempt. If, in some cases, this is connected with that singular propagation just described, it is an explanation that will not solve every case of the difficulty. They who know Rome, and its tales on that subject, will remember the opposed churches where the porter or janitor on the one side, long and invariably suffering from fever, was cured by the mere transference of his office to the opposite side of the same street; and where, at the same time, the duty had been always as safe as it was invariably dangerous or destructive on the other. This is a circumstance indeed of very frequent occurrence in various parts of Italy, but I will only quote one more instance from that country, out of many, because it is well known to many officers then serving with our army in Sicily. The village, the name of which has escaped me, unless that be Faro, was situated above the Faro of Messina; and while one side of the street was in the highest degree pestiferous, producing mortal fevers among the troops, the opposite one was entirely exempt.”

Dr. M. endeavours to throw some light on this apparent mystery, but with very doubtful success. It is probable, he observes, that the matter of malaria is often connected with vapour or mist—nay, that it is conducted and confined by this its vehicle. We find dews, mists, and hoar frosts often occupying a certain extent, both as to height and depth, reaching, for example, a particular hedge in some valley, and then ceasing by a most definite and sudden line; while also terminating at a particular altitude on the trunks and branches of trees, as if suddenly cut off. This

being the case, we may conceive how a malaria, thus united with a mist, may be as defined and local as it actually is found to be, in these singular cases. The following is a remarkably domestic instance of the transportation of this peculiar poison, for the accuracy of which our author pledges himself.

“This is the high road between Chatham and Feversham, involving an extent of about twenty miles; and it is here remarked by the inhabitants, that in every village and town, including also the detached houses, and comprising, from Chatham, Raynham, Newington, Sittingbourne, Bapchild, and Boughton, the ague occurs, on the left hand side of the road, generally, and is unknown on the right side; though the breadth of the road itself forms the only line of separation. If I were to repeat, in addition, some special facts, believed and related by the inhabitants of some of these places, and at Sittingbourne among others, this separation is even more wonderfully and mysteriously precise than the general fact as thus stated would prove it to be. I need only add, that the lands producing this malaria are situated generally at about a mile distant, on the left hand, being as well known as the road itself.”

The above fact rests on such unquestionable authority, and on so many eye-witnesses, that it cannot be doubted. Indeed, it is not much more remarkable than the confinement of malaria to one side of a street. The late terrible epidemic cholera of India, presented innumerable instances still more inexplicable than the above. The cholera would travel for days and weeks along one bank of a small river, or even of a nullah, leaving the opposite bank free—and then deserting its favourite track, it would suddenly migrate to the bank that had so long escaped its ravages. For our own parts, we would be more inclined to account for the phenomenon, on the principle of malaria *generated* on the spot, than *transported* to it from a distance. The cause of the epidemic cholera at length reached Cape Comorin, and, from thence, after a certain lapse of time, it travelled to the Mauritius, over a great tract of ocean, and without the aid of a favouring monsoon—nay, against the atmospheric currents. How could this be transportation? It must have been generation.

The caprices of malaria, in respect to level, are often very difficult of solution. In Italy, where it was first remarked, as a general rule, that the malaria lay near the ground, and was transmitted in the direction of a stratum near its level, in preference to a higher one, the solution was sufficiently obvious. Thus, it was found safe to sleep in the second or upper story of a house, while the fever seized on those below—hence certain popular practices relative to the closing and opening of windows. This fact was also well ascertained in other and distant regions, both in the eastern and western hemisphere.

“The solution here seems easy, and perhaps it is also the true one. It is, that the malaria is especially united with that transferable sub-

stance which forms the foggy stratum; or that the lowest portion of the atmosphere in the act of depositing water, is its vehicle and its residence."

It is not a little curious that, in some places on the coast of Norfolk, where malaria prevails, the second or upper story of a house becomes its favourite point of attack, while the ground floor-invariably escapes. It is probable that, were the circumstances narrowly investigated, there would be found an explanation in the direction of some prevailing winds, or other local incidents, that have hitherto escaped observation.

The poisonous gas constituting malaria, or the vehicle in which it resides, is capable of lodgment and retention in places where it has not been produced. Valetta offers a striking example, in the cases of the Floriana Guard, which suffered so severely, while other portions of the garrison escaped. Here the ditch was very deep and narrow, but so perfectly dry, that it could not be suspected of producing the malaria to which the effects in question were owing. Nor could this be explained, except by supposing that this ditch lodged and protected from dissipation, a current of noxious air, produced from the salt marsh, which seems to be the source of the malaria in Valetta, and which the sea-breeze directed on this spot.

"Nor is this explanation improbable, either for this case or other similar ones, when we know that carbonic acid, as well as watery vapour, or a moist atmosphere, can thus remain at rest on the ground, or in any other place where it is protected from the general circulation of the atmosphere, for a great length of time."

The idea of the attachment of malaria to solid substances is strongly countenanced by many facts. Thus, in the Campagna of Rome, it is remarked that, if the labourers cut down certain plants, (a bushy thistle,) a fever, that otherwise would not have occurred, is the consequence. The malaria is supposed to be entangled within it, and to be let loose by this disturbance. Farther it is observed that in many parts of Italy, the labourers are safe, so long as they keep to the erect posture; but if they sit or lie down, they are in danger.

"In such cases as this, from the far inferior virulence of the poison with us, the result might be a very slight fever, or at most an ordinary one; while, as such an event would most frequently occur during the time of harvest, it would naturally be attributed to heat or fatigue, or to the influence of the sun; and might thus, under peculiar symptoms, as it most unquestionably often has, be even considered a phrenitis."

In respect to the propagation of malaria, as dependent on certain chemical conditions of the atmosphere, our author has little to offer except analogy, and some detached facts. If not rigidly a gaseous matter, Dr. M. thinks it must be such, or nearly such, in its union with the air. If odoriferous substances be allowed to be analogous, the malarious gas should be

most easily united with, and diffused through a moist atmosphere—as seems to be tolerably well proved to be the case with matters of contagion generally. A moist atmosphere, indeed, may not only give facilities to the propagation of malaria, but contribute to the production of those diseases which are of a malarious character. But, in how far a moist air is favourable to the primary formation or development of the poison itself, is a question which cannot be solved in the present state of our knowledge. The *propagation* of it by air impregnated with moisture, is, he thinks, pretty well proved, not so much by definite facts, as by a great number and variety of probable circumstances. Thus, the popular opinion goes not only to the belief that malaria is conducted by common fogs, but that these fogs themselves are the poison, or, at least, the cause of the diseases. This is the opinion in Holland, in America, and even in this country. The intermixture of malaria appears to be the real cause of the pernicious nature of fogs, making allowance for the effects of cold and moisture at the same time.

"If it were not so, the same diseases which the pernicious fogs of fenny countries produce, should occur in elevated or mountainous situations subject to be involved in clouds, since the cloud is, in every other respect, a fog. If it were not so, the fogs of dry countries should produce the same diseases as those of moist ones, which they do not; and if it were not so, the westerly fogs that so often arrive in our island from the Atlantic, should generate the diseases of malaria, like the easterly ones arriving from Holland or formed on our own fenny coasts, which they are never known to do. And to confirm this, it is remarked, that while, in Flanders, (in Artois,) it is the south-westerly and southerly winds which bring and spread disease, in consequence, obviously, of the lands which they traverse, as well as of their own conducting qualities, it disappears as soon as the sea wind from the northern quarters sets in, although this is accompanied by dense and durable fogs. And the same rule will be found to hold good in many parts of the Mediterranean, as well as in France, in numerous cases."

The next fact is analogous, though somewhat different. It is equally a matter of popular belief and medical evidence. It is the pernicious nature of the morning and evening mists formed on low grounds. In the hotter climates, the effects of such mists in generating fever are very notorious—and this fact certainly strengthens the doctrine that the watery or moist atmosphere is the the active conductor or repository of malaria—and that when the former is dissipated, as by the sun in the day, the latter is checked in its progress—possibly in its production. The poisonous qualities that have been attributed to dews, in hot climates, are doubtless owing to their holding miasmata in solution.

"Thus also is it especially remarked, that if a hot day is succeeded by a cold and damp night, the effects of malaria are much aug-

mented; and the same analogy holds to similar changes in seasons, or as to incidental ones occurring in any manner. Hence if cold and wet weather should unexpectedly take place in the midst of a hot summer, an augmentation of severity, or a state of disease before not in existence, will occur; and hence also severe epidemics occur particularly, if, to such a hot summer there should succeed a cold and rainy autumn; the production, of the poison, as I formerly remarked, being apparently augmented in this manner, while the atmosphere is also rendered a better conductor."

The effects of rains in hot climates are accounted for in the same way by our author, who disagrees with Park and Lind, as to the power of rain or rainy seasons to produce intermittents by themselves. He here mentions a curious popular belief, grounded on popular experience, in Italy—namely, that there is no danger from malaria, however plentiful it may be, after nine o'clock at night—in other words, that its influence belongs to evening rather than to night.

"It is conceived, of course, here, that as it is entangled in the morning vapour, becoming dissipated or destroyed as the sun approaches the meridian, so when the condensation of the evening mists has been completed, it is precipitated and rendered inert or null."

This is the explanation which Dr. Johnson, gave nearly 20 years ago, with the addition that, as the earth, in hot climates, continued to be hotter than the air for some hours after sun-set, so there continued to be an extrication of miasmata from the earth, for some time after sun-set, thus meeting with, and augmenting the miasmata falling with the dews. In this country, indeed, the night air is considered unhealthy—but this circumstance is generally attributed to the cold of the night. Dr. M. believes this explanation to be quite erroneous, and the following are his sentiments.

"It is thought unwholesome because it is cold, or because it is damp: these are the reasons assigned; but the philosophy is false or confused, and thus the rule of avoidance becomes an inconvenience without being a precaution; while as an inconvenience, it is for ever broken. It is broken also when this air is not damp and not cold, because the philosophy is erroneous: and hence danger and disease which real knowledge would have prevented. No one fears a summer evening, even a mild summer night, unless indeed he shall find or fear a dew. Yet here lies the very danger; in a land of meadows, and parks, and ponds, and rivers, and woods, a thousand times more hazardous than all the nights of all the winters that ever were. This is the real night air to be feared, even though the gray mist should not arise, as it is called, or the dew not fall. To take a pleasant evening walk by the banks of the river or the lake, to watch the trout rise from the fish-pond or the canal at the evening flies, to attend the milking of the cows in the green meadow, to saunter among wet groves till the moon rises, listen-

ing to the nightingale, these, and more of such rural amusements and delights, are the true night air, the malaria, and the fever."

The prevention of such malarious influences in the night season, hinges principally on exercise, and invigorating food and drink. How far smoking and stimulant liquors may be preventive of the operation of miasmata, it is difficult to say, though the general opinion is that they are salutary. To sleep in a miasmatic situation, exposed to the night air, is, of course, most dangerous. In this place our author dwells a good deal on *food*, as rendering the body more liable to the impressions of malaria, especially in hot countries, when taken in immoderate quantities, at improper times, and of too animalized nature. It is a fact, that the natives of hot climates almost always breakfast before sun-rise, and dine after sun-set. This is the case, at least, with the great tribes of the Asiatics. They eat but little in the heat of the day, and that chiefly vegetable food. They drink cooling beverages then, and repose as much as possible in the shade. The Europeans too often exercise, eat meat, and drink stimulating potations, throughout the fiery heat of the day, and we all know the rate of mortality among them. It is highly probable, that much of the sickness among Europeans is caused in this way.

It is a curious fact, in the history of malaria, and contrasts strongly with the properties of contagion, that the former is less readily propagated through dense population and dirty streets, than along the most spacious terraces, or through the thinly inhabited suburbs. Thus the Judaicum at Rome, (the Saffron Hill or St. Giles's, of London) escapes the malaria in a remarkable manner:—and so a great number of examples might be cited of the same kind. Perhaps the following explanation may be as good as any.

"The malaria must be a chemical compound, and therefore decomposable: it is experimentally, decomposed by fire and smoke, and it is therefore probable, that, amid the unknown mixture which forms the atmosphere of crowded streets or habitations, it is actually destroyed."

The last topic which we shall touch upon is, the real or supposed defence against malaria by means of a veil or canopeum surrounding the head. In Malta, and some other places the belief in this measure is universal—hence the popular practice of covering the mouth and nose with a handkerchief, in the morning going out, or in other suspicious circumstances. If dependence can be placed in popular belief or assertions, there is some foundation for this practice.

Our limits have been outstripped, and we must bring our account of Dr. Macculloch's first volume to a close. We are now prepared for the promised volumes on the diseases arising from malaria—and these must afford ample materials for interesting discussions. We shall not fail to give our readers due information of all practical matter brought forward by this industrious author. We are sorry to see

some of our cotemporaries endeavouring to turn into ridicule all investigation of the nature, laws, and effects of malaria. They would better exercise their "TALENTS," of which they have unfortunately TOO MUCH, upon the trickeries, the delinquencies, and the *abuses* of the profession. These are the legitimate objects of satire and criticism—not the patient labours of those who are toiling to draw the veil from some of those obscurities that are perpetually entangling the medical practitioner in the labyrinths of error.

From the London Medical Gazette.

**FATAL ATTEMPT TO CURE AN ARTIFICIAL ANUS THROUGH THE VAGINA,** *by cutting into the Cavity of the Abdomen, and Uniting two portions of Intestine by Suture.*

[La Charité.]

A washerwoman, 25 years of age, of good constitution, was confined about the end of January, after a very severe labour. The head of the child was retained a long time, and it was obliged to be extracted with the forceps. During some days afterwards, the woman had no evacuations by stool, but she did not experience any symptoms consequent upon their retention. Ten days afterwards, fecal matter, in a semi-fluid form, was passed by the vagina, and from this moment the patient continued to pass the feces by this channel exclusively. In this state she was admitted into La Charité on the 29th January, twenty days after her delivery. This disgusting malady embittered her life to such a degree, that she continued to implore to be relieved by an operation, however dangerous and painful that might be. The finger, introduced as high as the neck of the uterus, discovered behind, and towards the left side, an opening into which it could be insinuated. This corresponded with the point of union of the vagina with the neck of the uterus, the posterior lip of which appeared to be destroyed. An examination by means of the speculum, only confirmed the diagnostic established by means of the touch; the nature of the substance passed off by this passage: the height at which the orifice was situated, led to the belief that this was not a recto-vaginal fistula, but that it was a portion of the termination of the ileum that opened at this point. Several enemata were administered, and all were again returned by the anus, without a single drop flowing from the vagina.

An attempt was made to effect a cure by abstinence. For a long time, the patient's whole diet consisted only of two portions of rice broth in 24 hours. The feculent matters were then furnished in smaller quantity, but still passed entirely as before; so that nothing was to be expected from the efforts of nature. The entreaties of the woman became every day more pressing, and M. Roux, anxious to

relieve her if possible, first planned the following operation, namely, to seek for the fold of intestine which was perforated; to cut through it both above and below its adhesions; to draw out the two extremities; to fix them outwardly, and thus to substitute an artificial anus exteriorly, which he was afterwards to attempt to cure. But considering that this plan exposed the patient to the risk of a dangerous operation, with but little prospective benefit, M. Roux abandoned this idea, and formed the design of bringing the two cut extremities of the gut into contact, and maintaining them there by sutures. This project being fixed upon, M. Roux with the concurrence of his colleague, proceeded to perform the operation on Saturday the 1st of March, at nine o'clock in the morning, in the presence of a great number of spectators, attracted by the novelty of the operation. The appearance of the patient was favourable; her long abstinence had but little diminished her *embon point*; her constitution appeared sound; and she preserved her strength and the freshness of her complexion unchanged. She was placed on a bed; the surgeon stood upon her right side, and commenced his incision (three inches and a half long) on the median line, about two inches and a half below the umbilicus, extending to the same distance from the symphysis of the pubes; the skin and the linea alba were carefully divided down to the peritoneum; some small arterial branches were immediately secured. The greatest precaution now became necessary, to prevent any injury to the contents of the abdomen. The epiploon pushed forward by the intestines, was so immediately contiguous to the peritoneum, that the operator at first thought that they adhered: an attentive examination, however, proved that this was not the case. He raised the peritoneum up gently, and opened it with great precaution; he then passed in a grooved sound between this membrane and the viscera, and laid open the peritoneum to the extent of the external wound.

The cavity of the abdomen being thus opened, M. Roux passed two of his fingers deeply into the pelvis, to seek for the fold of intestine which formed the preternatural anus; he reached it: the index finger of his right hand placed in the vagina, met the corresponding finger of the left hand, which was in the abdomen, and touched it through a thin partition. A portion of the intestine, continuous with that which opened in the vagina, was laid hold of close to this point; a ligature was passed across its mesentery, by means of a curved needle, in order more readily to draw it forth, and to retain it. The portion of the intestine which was supposed to be another part of the fold included in the disease was easily drawn out, without the assistance of a ligature: this portion of intestine was cut across by means of a curved, button-pointed bistoury, cutting on its convex side. The portion of intestine supported by the ligature was then divided in the same manner. The intestine first cut contained some hard and ra-

ther solid faecal matter, which induced the belief that it was the lower end of the gut: the two ends of intestine which were intended to be put together, having been brought outwards, the mesentery was separated from them for an extent of some lines: some small vessels were cut in making this division of the mesentery, and one affording a jet of blood was secured by a ligature. The two portions of intestine were then placed together, and fixed by three points of suture, which were applied to the two ends, at a few lines distance from the point of division, making the ligature penetrate and pass out on the peritoneal side in such a manner, that the ligature being drawn tight, the two extremities of the gut were placed against each other by their serous surfaces. Of these three points of suture, two were applied upon the sides of the insertion of the mesentery, with the intestine; and the third at the point opposite to that insertion: the two ends of each ligature were cut close to the knot. The sutures being accomplished, the two ends of the gut were pushed back into the cavity, so that they only touched by their serous surfaces: the parts were then placed again in the abdomen, and the incision in its parietes closed by the twisted suture; straps of adhesive plaster, some pledgets, compresses, and bandage round the body, formed the whole of the dressing: the operation lasted one hour and a quarter, including the dressing.

A few hours afterwards, acute pains were felt in the abdomen: the sutures were obliged to be loosened: a bleeding from the arm was had recourse to, and ice was placed upon the belly. The pains continued: the slightest pressure was insupportable, and in the evening there were some mucous and bilious vomitings: (fomentations to the abdomen.) The day after the operation, the abdomen was very painful: the slightest pressure augmented the patient's sufferings: the thirst was extreme: the pulse very frequent and hard: (fifty leeches to the abdomen.) In the course of the day the pains augmented; and she continued to get worse till 11 o'clock at night, when she expired, 38 hours after the operation.

*Autopsy.*—The peritoneum was scarcely more vascular than natural; at the lower part there was a little yellowish fluid. Some portions of the small intestines were covered with albuminous exudations; they, together with the stomach, were distended with gas: an attentive examination discovered the following particulars. The ileum, at six inches from the cæcal extremity, adhered to the point of junction of the posterior part of the uterus with the vagina; these adhesions were well organized, but not so solid as to resist rather strong efforts: it was this adhering portion of the intestine that formed the preternatural anus: the whole caliber of the intestine was not included in this destruction; it had only been pinched, as it were, in one half of its circumference: the continuity of the intestinal fold, was not interrupted at its concave or mesenteric edge: the inferior end was pushed

into the preternatural anus, and formed a conical projection in the vagina, about an inch in elevation.

The small intestine had been incised about two inches above its adhesion; but the operator was mistaken in supposing that he had cut it between the point of adhesion and the cæcum. This portion of intestinal canal was untouched; but the iliac portion of the colon had been cut, not far from the rectum, and the ileum had been fixed to the superior end of the sigmoid flexure of the colon. Two portions of intestine opened into the cavity of the abdomen; the rectum, or the inferior portion of the colon on one part, and on the other the ileum, in the place where it had been cut, at a small distance from the preternatural anus. A mass of faecal matter about the size of a small nut, was interposed between the extremities of the intestine that were brought together. The yellow liquids found in the depending part of the abdomen, came, without doubt, partly from the superior end. The two ends of the gut which had been brought together, had already contracted some partial adhesions.

Much discussion has arisen in Paris, as to the propriety of the above operation: some have praised it, but the great majority have condemned it, regarding it as unjustifiable to expose a patient to the chances of so formidable an operation, to remedy merely an infirmity. If ever such an operation should be again attempted, it will be proper to guard against the mistake into which M. Roux fell. If the patient had not died from the immediate effects of the operation, if the two ends of the intestinal canal placed together had united; the fæces must have undergone a retrograde movement; they must have traversed the colon inversely to their ordinary course; stopped by the ilio-cæcal valve, they must have flown back again, and if they had overcome that barrier, they must have passed out at the ilio-vaginal opening.

It is probable that the patient would soon have perished in consequence of this operation, even if the immediate object of effecting a perfect union between the portions of intestine fixed together by suture had been accomplished. We record the case not as one to be imitated, but shunned; and as a most unwarrantable attempt on the part of the surgeon, who appears to have set the desperate chance—the bare possibility of success, and consequent renown to himself, against the almost incalculable hazard to his patient.

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From the *Journal de Physiologie*, &c.

#### CHRONIC ULCERATIONS OF THE TONGUE AND PHARYNX CURED BY THE HYDRIODATE OF POTASH. By M. MAGENDIE.

A few years only have elapsed, since iodine has assumed a place among medicines, and it

may already be regarded as one of the most valuable articles, which, for a long time, has been added to the *Materia Medica*.

Placed, by my duty, in the midst of patients deemed incurable, and whose condition is no less revolting, than worthy of commiseration, I never enter this horrible abode, without seeking what method I shall adopt to combat the disgusting diseases, which overwhelm the unfortunate females confided to my care.

Those afflicted with cancerous affections, who are admitted into the hospital, after having undergone, in Paris or elsewhere, all kinds of surgical operations, internal treatment, empirical remedies, &c. have particularly awakened my interest. Happy would it be for humanity, if chance should lead to the discovery of a remedy, for a disease the most hideous, and the most dreadful to which humanity is exposed!

At the present moment, I place some reliance on the hydriodate of potash, exhibited in large doses; nothing that has hitherto fallen under my observation, has convinced me that I ought to relinquish the employment of this substance.

The result of my experience, will ere long be laid before the public; in the meanwhile the following cases of chronic ulceration, reported incurable, and placed as such in my department, may not be deemed uninteresting; they were cured with a promptitude equally productive of surprise and pleasure.

*Hôpital de la Salpêtrière. Salee St. Nicolas,  
M. Berard, élève interne.*

The first, was a woman, of a lymphatic constitution, who had enjoyed good health up to her thirtieth year. At this period, the catamenia appeared less regularly, and each return was accompanied with serious symptoms. Some epileptic paroxysms announced their approach; they were very abundant, and the body was covered with livid spots, during the latter part of their continuance. This state of things continued increasing for some time, and it was then, according to the patient, that symptoms of a more formidable character supervened. Extensive ulcerations formed in various parts of the body, on the thighs, legs, above the breast, on the forehead, face, head, &c. for which she was unable to assign any cause. The periosteum of the tibia, and radius of the right side, subsequently inflamed, and fragments of bone, were extracted from the resulting abscesses.

The appearance of these large ulcers, the tumefaction of the periosteum, and the extensive ulcerations of the mucous membrane of the tongue and pharynx, induced the physician in attendance to suspect a constitutional venereal taint as the cause of all the symptoms. It is however important to remark, that notwithstanding the hope of a certain cure, which was held out to the patient, provided she gave a correct account of the origin of her disease, she still adhered to the same statement, and declared that she had never had the venereal. After a denial thus explicit, we may be per-

mitted to doubt the syphilitic character of the complaint.

She was subjected notwithstanding, to a complete mercurial course, combined with the usual diaphoretics. The ulcers of the inferior extremities and those of the breast healed, but the others continued intractable, and new ones formed. Those on the tongue increased in size, and the whole of the right margin, and half of the upper surface of the same side were covered with one large ulceration. By degrees the voice was lost, a symptom which supervened without having been preceded by acute pain of the larynx, though it was supposed that ulcerations similar to those in other parts of the body, had destroyed the chordæ vocales. Such was the situation of the patient, when M. Magendie assumed the charge of the hospital of incurables, where she then was. Nearly a year had elapsed since she had been subjected to the preceding treatment, which had produced such ambiguous effects,—the cicatrization of some ulcers, and the appearance of a greater number of new ones.

Complaining one day of a greater degree of dyspnœa than usual, M. Magendie ordered a julep containing twenty-four drops of a solution of the hydriodate of potash, with a view to accelerate the catamenial discharge, which was then expected; but anticipating still further advantages from its use, he directed me to continue its administration and to augment the dose gradually, which was done, to the extent of thirty-six drops at the expiration of six days. The happy effects of the remedy were quickly perceptible; the surface of the ulcers soon became clean, and after the lapse of fifteen days, that, situated upon the tongue was completely cicatrized. Those on the other parts of the body, by reason of their greater extent, required a longer time to heal; however, on the thirtieth day, there remained only a fistula near the right wrist, through which the radius was felt denuded.

The patient continued to improve for about three weeks, when she was attacked at the same time, with violent ophthalmia, and great difficulty of respiration. The cicatrization of these old ulcers had induced a fear that unpleasant consequences might follow, and in order to obviate them as far as possible, an issue had been established some time before. But as the difficulty of respiration appeared to be only an increased degree of a symptom to which she had been long subject, M. Magendie directed the application of leeches to the anus, and revulsives to the extremities.

A more careful examination on the morrow developed the cause of the disorder, and although the patient did not complain of pain in the larynx, M. Magendie readily perceived that so much dyspnœa must arise from some obstruction in the aperture of the glottis. The disease throughout its progress had been marked by the greater number of the symptoms attributed to œdema of this part, and was especially accompanied by that singular modification of the voice, which has been sup-

posed characteristic of some disease, but which so often accompanies a diminished diameter of the laryngo-tracheal canal in whatever manner produced.

Recourse was had to the antiphlogistic system, notwithstanding such a plan did not appear very strongly indicated. Tracheotomy was not performed, though the symptoms would seem to have justified this operation. The patient died of asphyxia on the eighth day.

On dissection, the lungs were found livid, and engorged with blood; the bronchiæ filled with mucus even in their most minute ramifications. The mucous membrane of this part was red, as likewise that of the larynx; but this redness terminated abruptly in the larynx. In the interior of the ventricle, upon the chordæ vocales, and in all the space comprised between the glottis and epiglottis, the mucous membrane was covered with indurated and whitish vegetations; the glottis itself was so much contracted as scarcely to admit the introduction of a quill.

*Second Case.*—Madelaine Petibon, aged forty-one, was admitted four years ago, into the Hôpital Saint Louis, for extensive ulcers upon the legs, which she had for a long time. She was scarcely discharged cured from the hospital, when she was attacked with dyspnoea, paroxysms of suffocation, and acute pain in the region of the larynx; the tone of her voice changed, and eventually she could speak only in a whisper; at the same time, extensive and deep ulcerations made their appearance upon the face and neck, and others invaded the apex and upper surface of the tongue.

After having undergone various modes of treatment, this patient was admitted into the infirmary 29th March, 1827, three years subsequently to the appearance of the ulcers on the face and neck. Those on the latter part were entirely cicatrized; the nose, destroyed in great measure, presented a very deformed cicatrix. Five or six thick, yellowish encrustations, from six to eight lines in diameter, covered as many soft and fungous excrescences scattered over different parts of the face. An unequal and gray coloured ulcer, with hard and elevated margin, occupied the whole palatine surface of the tongue, and gave origin to several fungous excrescences; deglutition was extremely difficult, great dyspnoea, and articulation almost impossible. On the 27th June, she was put upon the use of the hydriodate of potash, two drachms in a julep. A few days of this treatment effected a great melioration; the ulcers cleaned, assumed a better aspect, and by the 21st July, they had entirely cicatrized, though the incrustations on the face still remained undetached.

1st Feb. 1828. Up to this period, there has not been the slightest appearance of a relapse; the incrustations and ulcerations still continue, but are evidently improving.

March 1. The crusts have now become detached, and the cure is complete. The dose of the hydriodate never exceeded eight grains per day.

From the Medico-Chirurgical Review.

*Researches into the Causes, Nature and Treatment of the most prevalent Diseases of India, and of Warm Climates generally. Illustrated with Cases, post-mortem Examinations, and numerous coloured Engravings of Morbid Structure.* By JAMES ANNESLEY, Esq. of the Madras Medical Establishment, &c. &c. Imperial Quarto, pp. 700, with 21 coloured Plates. Vol. the First. Longman and Co. 1828.

This magnificent work will transmit Mr. Annesley's name to posterity, in conjunction with the medical history of our extensive empire in the East. We know not which to admire most—the indefatigable labour, and the unconquerable zeal of the author in the collection of his facts, or the beauty and fidelity of the plates, which portray the ravages of disease as it appears in the Torrid Zone, with the most scrupulous accuracy.\* We hope and trust that the East India Company will do an act of justice in rewarding Mr. Annesley for the toil of mind and body which he must have undergone in the construction of this immense undertaking—leaving the tremendous expenses out of the question. If they can so cheerfully vote away their thousands in the annual pension of those who make war—they surely might well expend a few hundreds in the encouragement of those whose labours will mitigate the miseries of warfare and the deleterious influence of climate, long after their bones are mouldered into dust.

We do not deem it necessary now to apologise for dwelling on those diseases which scourge our country beneath a foreign and burning sky. Some of the greatest improvements in medicine have resulted from researches made in hot climates—and there is not a single fact observed, or a single disease investigated on the banks of the Ganges or the Mississippi, that does not bring its quota of utility to the practice of medicine in our own country.

After stating the excellent means which he possessed, for a great many years, in India, of acquiring and registering the most authentic information, Mr. Annesley remarks as follows:—

“In India, the medical practitioner has every possible opportunity of investigating disease by *post mortem* examinations, and of connecting the symptoms and treatment with those morbid changes which take place in its course. To this subject the author has always paid especial attention: but the great difficulty of describing morbid structures, and

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\* It will doubtless be said that some of the plates are too highly coloured. This may be true—and it is an objection to most coloured plates. But it should be remembered, that diseases run a rapid course in high temperatures, and that dissections are necessarily made there, in a few hours after death.

the impossibility of preserving the natural appearance in the way morbid preparations are usually made, led him to cause Drawings to be executed of the more interesting and remarkable changes produced upon the internal organs by the diseases he was called upon to treat. Circumstances placed in his power the means of accomplishing this object, and he fully availed himself of them. *Post mortem* examinations necessarily take place in warm climates soon after death, and before the capillary circulation in the internal organs has undergone that change which is experienced after a few hours, or before the blood has returned from the minute arteries into the venous trunks. Thus, the warmth of the climate has indirectly enabled him, it may be presumed, to give a more correct delineation of the appearance of diseased structure than could otherwise have been obtained. The knowledge unfolded by this circumstance induced him to follow up the indications to which it pointed; and as an early examination of the subject of disease after death appeared necessary to accurate ideas as to the more minute changes and finer shades of disorder, impressed upon the different internal viscera during life, it was never neglected when it could be practised with propriety."

This is a very important consideration, and tends to enhance the value of the work under review. We must pass over the preliminary discourse of our author, in which he dwells, with allowable earnestness, on the advantages which a long residence in India has conferred on himself—and points out, in no very measured language, the source of error which may have operated, where the residence has been short, the scene of observation limited, or the constitutions of the patients of a peculiar description. All these things we are ready to grant to our experienced author; but we would just hint, from some 30 years close observation of men in all climates, that it is a comparative rare occurrence to find any discovery or improvement in medicine result from *mere length of experience*. We refer Mr. A. to the history of our art generally—and to the medical history of tropical climates particularly, for the proofs of this position. Mr. A. regrets that few or none of the old and experienced practitioners of India have left us any records of their practice. If we look to the West, we shall see the same thing. Have any of the old residents of the Antilles left us any works to compare with those of Jackson and others, who were only a few years in that unhealthy climate? In short, unless observations are made in the vigour of life, by medical men in hot climates, they will never be made at all! After a certain number of years, in hot as well as in cold climates, the current of zeal, in the minds of medical practitioners, is too often dried up or frozen up—and it is replaced either by a sharp look out for the "main chance"—or a settled resolution to take things easy—to enjoy the short span of existence with as little encumbrance as possible—and to leave to others the trouble of

observing for themselves, as they themselves were obliged to do! Mr. Annesley, it is true, forms an exception to the general rule. He is a "rara avis in terris"—but he may take our word for it, his call on the old practitioners will meet with few responses. Let us, therefore, be thankful for the contributions of our younger brethren, both here and elsewhere—and let us be doubly thankful to such men as Mr. Annesley, who have devoted a long series of years to practical and pathological researches beneath the enervating influence of a vertical sun, and surrounded by so many temptations to indolence and luxury!

But, leaving these considerations on one side, we come to the work itself. We must pass over the whole of the first chapter of the work, occupying 45 folio pages, which treats of the physiology of digestion, and the functions of the liver, spleen, &c. Considering what a size and price Mr. Annesley's book necessarily amounted to, we cannot but think a chapter on the mere elementary matter of digestion, to be found in the school-books already in the possession of even pupils, was somewhat impolitic. We next come to the second chapter, exhibiting "a general view of the causes chiefly productive of diseases in warm climates, particularly in India."

#### MALARIA.

This subject has been treated in so masterly a manner by Dr. Macculloch, that little or nothing new or interesting can be expected from Mr. Annesley. The copious analysis which we have given of Dr. M.'s works will be a sufficient excuse for passing over the section in question from the pen of Mr. A. We shall notice one particular only. The writers on marsh miasmata have generally insisted, especially since Dr. Bancroft's Essay appeared, that animal matters have nothing to do with the poison of terrestrial exhalations. We have always been of a different opinion, and the following passage shows that we have Mr. Annesley on our side.

"A most important circumstance, which goes far to account for the much greater unhealthiness of moist and marshy situations in warm countries, is the quantity of animal matter, in a state of decomposition, which they present. The same circumstances which render vegetation quick and luxuriant, tend also to generate immense swarms of reptiles and insects; the exuviae and dead bodies of which, mingling with vegetable matter in a state of decay, and combining with moisture, give rise to miasms of a much more noxious description than those resulting from vegetable decomposition and moisture alone. In the course of our experience in warm climates, we always have considered the number of insects and reptiles with which a place abounds, as more indicative of its unhealthiness than any other circumstance; for in it there is a most powerful cause of disease in its worst forms superadded to those already in existence; and, as the one cause is extensive and powerful, so, generally, is the other. The great unhealthi-

ness of low, moist, and marshy places in temperate climates, during warm seasons, particularly in the months of July, August, September, and October, is as much owing to the immense swarms of insects which then abound, and which die during these months. Italy furnishes numerous proofs of this; and every warm country in the globe will verify the axiom, that a place is unhealthy in proportion as it furnishes, with the various causes of disease depending upon locality and temperature, animal remains and animal substances in a state of decomposition, mingled with the products resulting from the decay of vegetable matter."

In a subsection, Mr. Annesley introduces some remarks on the nature, properties and effects of miasmata—and on the manner in which they invade the system. In respect to the *first* point it is hardly necessary to say that we know nothing. We are as ignorant of the nature or essence of malaria as we are of the inhabitants of the Georgium Sidus. Of the properties and laws of vegeto-animal effluvia, we have taken ample notice in our review of Dr. Macculloch's Essay—and Mr. A. appears to have drawn pretty freely on that and all preceding works on malaria. We shall proceed at once to "the effects of malaria on the human constitution." Intermittent and remittent fevers, of course, are the acknowledged products of the invisible poison—"even yellow fever, in its worst forms, seems to be the consequence of these causes operating in a state of greater activity or concentration upon highly disposed subjects." Mr. A. believes the plague of Egypt to be the product of malaria. Next in importance, to fever, is dysentery, which, in its epidemic forms, Mr. A. ascribes (and probably with justice) to malaria. He does not deny, however, that sporadic cases of this disease are the products of vicissitudes of temperature, errors in diet, intoxication, and other causes. He thinks there can be no doubt that the scorbutic dysentery, so well described by the late Mr. Bampffield, is produced by terrestrial exhalations acting on constitutions badly nourished by improper food. He says the scorbutic dysentery which prevailed at Rangoon, and the endemic at the Milbank Penitentiary, are recent examples of this. Even hepatitis he is disposed to attribute to malaria conjoined with tropical heat. "There is seldom seen, within the tropics, a case of disease in which, upon dissection, the liver and spleen are both sound." In fevers from marsh effluvia, indeed, whether within or without the tropics, there seems to be a strong tendency to derangements of the liver and spleen. The exposure, therefore, to malaria, even when no fever or dysentery is produced, seems to affect the hepatic system, as we see in all marshy countries. The malaria of India has an infinitely greater effect on the European than on the native population. If the children of Europeans are not sent home young, their constitutions are liable to be ruined, and the range of their existence abridged.

"In addition to the diseases we have enu-

merated as being produced among Europeans by malaria, and in addition to its blighting effects in warm climates upon a native white population, even when it fails of inducing active and specific disorder, we should particularize its influence in occasioning ulcers of the lower extremities, and foul sores, and even sphacelation and gangrene. Every military surgeon has numerous opportunities of observing, in the East, the relation which subsists between unwholesome situations and these disorders, both among Europeans and natives. Indeed, it seems to be a general and necessary effect of malaria to diminish the powers of life throughout the whole body; and the phenomena accompanying and indicating this effect are various, according to numerous concurrent circumstances, to predisposing causes, and to concomitant influences. Of these we shall have occasion to speak when the diseases preceding chiefly from this grand agent come specifically before us."

In respect to the *modus operandi* of malaria, or even the channel through which the poison is conveyed, Mr. A. is unable to furnish us with any positive information.

"But observation has supplied data, which, when calmly considered, seem to show that terrestrial emanations, and all those causes of disease which float in the atmosphere, make an impression on those surfaces with which the air comes in contact: and this impression, when sufficiently strong, or frequently made, is productive of disease, either of the system generally, as in fever, or of some important viscus, as the liver or spleen. It is, therefore, chiefly to the internal surfaces of the lungs and air passages that we are to look as the channels through which malaria makes its hurtful impression upon the animal frame. But whether it acts by deranging the healthy condition of the nervous system of the organ, which derangement produces farther disorder until specific disease is fully formed; or whether the exhalations floating in the air are actually absorbed from the surfaces of the air passages and cells into the blood, vitiating this fluid, and, by its presence there, deranging the whole system, or some important viscus, it is impossible to decide. Both sides of the question have found supporters who have adduced arguments in behalf of their opinion, in the absence of positive proofs."

Mr. Annesley seems to conclude that both may be combined. The miasm may offend the nervous system, and, entering the circulation, may vitiate the fluids at the same time.

After taking a survey of those circumstances which favour the operation of malaria, as irregularities of all kind—and more especially the depressing passions, indolence, &c. our author devotes a subsection to the consideration of the means of preventing malaria, and counteracting its effects on the human body. These subjects have been amply discussed—or will be so, in our reviews of Dr. Macculloch's volumes. We must pass over another great portion of the work before us, consisting of medical topography—embracing both

hemispheres, as well as the Mediterranean. From a section on the diet of Europeans in India, we shall select the following passage, exhibiting the "diary of a day," if we can use such an expression. It is a matter of curiosity, in more than one respect.

"The military officer goes to parade at six o'clock, A. M. and breakfast between eight and nine upon tea, coffee, or cocoa, with fish, meat, eggs, rice, and whatever may be most agreeable to him. From breakfast till one o'clock he generally applies to study or amusement, or to paying visits. The heat of the weather, and perhaps a hearty breakfast, and the nature of the articles taken at it, produce thirst, which renders the necessity of gratifying it urgent, and occasional draughts of wine and water, beer and water, or brandy and water, are therefore necessarily taken; and although this is by no means a habit, nor is indulged in beyond what seems a matter of necessity, yet it must, in a certain degree, be injurious. At one o'clock he eats a hearty tiffin, consisting of roast and boiled meat, fish, mullagatawny or other soups, various wines, bottled beer, &c. He afterwards occasionally rides out in the sun, and either lounges on a sofa, or amuses himself with cricket or fives till evening parade. Dinner is next disposed of, at seven o'clock, or half-past seven, or eight. This meal is, properly speaking, the supper, that which is taken at one o'clock being the dinner. The seven o'clock meal is generally profuse, consisting of soups, fish, rich and hot curries, roasted and boiled meats, and other richly made dishes, with various wines, and bottled beer. To all this succeeds coffee or tea; and upon the repleted stomach and excited system he retires to bed at eleven or twelve, when the feverish collapse induces the sound sleep indicating plethora, or the restless slumbers attendant upon prolonged excitement."

The above is a faithful diary of a tropical sojourner in the military service, and we have reason to believe that it equally applies to the civil servant of the Company. This being the case, we cannot wonder that our countrymen return from the East more frequently with enlarged livers than lacks of rupees. It is seen that animal food is partaken of largely, at least twice, but generally thrice in the day, together with an abundant supply of various other stimuli and provocatives—all this, too, in a climate where adequate corporeal exercise cannot be taken, without immediate risk of life. That such a system of repletion must keep up a constant over-excitement in the digestive organs, including the liver and spleen, cannot be doubted. It would do so beneath the gloomy skies of England, with all the exercises of the field, and the bracing air of a British winter! Mr. Annesley protests against this system of full living, and eloquently supports the arguments of those who have gone before him in this line of investigation. We cannot so fully concur with our talented experienced author in his ideas respecting exercise in tropical climates. Mr. A. strenuously

recommends corporeal exercise "so as to promote a full and copious perspiration and regular circulation in the cutaneous surface." We are advocates for exercise, in all proper times and places; but we have been led, from personal feeling as well as observation, to regard corporeal exertion as much less adapted to tropical than hyperborean regions. The vessels of the surface are too much excited by the heat of an Indian atmosphere, even when a person is at rest, and therefore, we believe that quietude in the middle of the day is best, while we recommend exercise before sunrise and in the evenings, in moderation. This, indeed, after all, is the rule to which Mr. A. comes in the sequel.

In a chapter on the premonitory symptoms of diseases, we find many judicious observations. Every one knows that there is an interval between the application of a morbid cause—say the cause of fever, and the development of its effects. This may be termed the period of incubation. The phenomena which take place in this interval generally pass unnoticed, or unattended to by the patient; but a careful observer will see that the seeds of disease are sown, and that a storm is impending.

"In fuller illustration of this subject, we shall instance a very frequent case, and one that will be recognised, not only by those in India, but by those who have ever been there;—we allude to snipe shooting and hunting parties. These are generally arranged late in the evening, after dinner, and are entered upon early in the morning. It is impossible, therefore, that the individuals engaged in them can have the repose necessary to recruit the system from the exertions of the preceding day. After riding eight or ten miles, they commence snipe shooting in the marshes and rice-fields, where they are up to their knees in water; and thus, in a state of fatigue, they are at once brought within the influence of those marshy exhalations which are the most frequent exciting cause of fever in warm climates. The exposure to this cause taking place during a period of predisposition to its invasion, and at a time of the day when the cause itself is in considerable concentration, that impression is made upon the system which is productive of fever, and its future subject returns from his excursion with the seeds of it sown in his frame. For a day or two he complains of little or nothing excepting a weight in his back, loins, and limbs, some loss of appetite, and a disinclination to exercise or employment of any kind. To these he attaches no importance, imputes them to fatigue from his excursion, and he does not resort to any means for removing them. They, however, continue, and even increase; and in a short time a slight headache, with confusion of ideas, comes on, especially towards evening, and is attended with disturbed repose and unpleasant dreams. His appetite now becomes further diminished, his countenance is pale, sallow, and a somewhat darker tinge is remarked beneath his eyes, which are at the

same time muddy, and deficient of their usual expression and liveliness. These symptoms continue for several days: they are insufficient to confine him, or even to excite ideas of his being actually ill; but he feels out of health, and every kind of occupation is a burden to him. At last, after a period widely varying in its duration, generally enduring from two or three days to a fortnight,—during which time these symptoms continue gradually to increase,—nausea often supervenes, the bowels become irregular, the tongue white and loaded, the countenance sunk and muddy, the surface, cold, dry, and harsh; and at last irregular chills, formication, and even complete rigours, supervene, with sinking and a sense of anxiety at the pit of the stomach and præcordia, and increase of the pain in the head, loins, and limbs. This is that precise stage of the disease at which the patient generally becomes alarmed, and when he is first unable to keep about.”

When we carefully consider the foregoing phenomena, which are correctly recorded, we will be constrained to admit, that they afford full as much support to the humoral, as to the nervous pathology of diseases. The sallow countenance, the dark tinge round the eyes, the white and loaded tongue, together with the deranged state of the secretions and excretions, which the author has not sufficiently noticed—all these are as indicative of vitiated fluids, as of disordered nerves in the body. But, on this subject, we have already dilated in another part of our Journal. Mr. A. goes on to state some causes of disease in warm climates, which are of considerable importance. The mode of living before described, combined with want of exercise, tends to plethora, and this is aggravated by a constipated state of the bowels, and a vitiated condition of the secretions. These circumstances, our author observes, “tend very rapidly to vitiate the constitution of the blood itself.” After making many judicious observations on the state of various functions, as indicative of incipient disease, Mr. A. comes to the second book of his work, in which he takes up the subject of disorders of the stomach, as they appear in tropical climates.

#### STOMACH DISORDERS.

Our author remarks, that this class of complaints is comparatively rare in hot climates, at least in “a pure and uncomplicated form.” They are seldom much noticed, till they become connected with, or give rise to, more serious disease, as of the liver or intestines. Mr. Annesley does not profess to go very deeply into the investigation of stomach disorders, and we are forced to confess, that the short dissertation contained in the work evinces neither novelty nor originality. Our author is disinclined to view, as some have done, the stomach affection as the *cause* of the biliary derangement. The causes of both these complaints, he properly observes, are simultaneously acting on the two organs, and although the first symptoms are generally noticed in

the process of digestion, yet it does not follow that disordered biliary secretion is behind hand in its part of the morbid process that is going forward. We are unable to glean any thing from this chapter, and shall, therefore, pass on to that “on inflammation and organic lesion of the stomach.” Mr. A. remarks, that simple and uncomplicated inflammation of this organ is an extremely rare disease in tropical and in cold climates. But the mucous coat is very frequently inflamed secondarily, between the tropics, in consequence of the extension of disease from the liver or other contiguous viscera. Mr. Annesley appears to place faith in the doctrine of Dr. Philip, that the advanced stage of indigestion is that of inflammation. The mode in which he conveys his belief is rather equivocal. “It (inflammation) *supervenes*, we are most thoroughly convinced to a greater or less extent, in the advanced stages of dyspepsia.” This may be the case, but this does not prove that inflammation is the *cause* of the disease. The following short case, which we shall extract verbatim, from page 253 of the work before us, will show how far the treatment of stomach affections needs reformation in the East.

“CASE.—*Inflammation of the Mucous Coat of the Stomach supervening to Dyspepsia, and terminating in Ulceration.—Dissection.*

“WILLIAM SPARKS, admitted 19th of June, 1815.—Returned from field service much emaciated, and extremely languid. Has been complaining for some time of dyspeptic symptoms, with occasional attacks of fever. Countenance sallow; tongue foul; some purging, without pain; no fulness in the hypochondria; pulse 70; skin warm and moist. Has used wine with tonics.—*R.* Mist. amaræ, (infusi gentian. comp.) ℥.; tinct. cinchonæ, ℥ij.

“22d.—Bad taste of the mouth, with nausea and general sickness; no desire for food; bowels rather open.—*Capiat* pulv. ipecacuan. ℥j. pro emetico. Take at 11 o'clock, A. M. Mist. amaræ, ℥jss.; tinct. ferri mur. ℥xij.

“*Evening.*—Threw up some dark, grumous fluid after the emetic; several stools.—*R.* Aq. menth. pip. ℥jss.; tinct. calumbæ, ℥jss.

“23d.—Feels pretty easy this morning; pulse is slow and languid.—*R.* Infus. gentianæ comp. ℥jss.; tinct. ferri, ℥x. twice a day. Continue the wine.

“24th.—Repeat the draughts twice a day, and wine.

“26th.—Takes his draughts and wine; appears to be declining; debility increases; bowels loose. Continue draughts and wine.

“*Evening.*—Seems much worse; some hiccup, and the pulse at the wrist nearly gone; debility extreme. Continue the wine.—*R.* Mist. camphorat. ℥jss.; spirit. æther. nitros. ℥ij.; aquæ, ℥vj. M. statim. Blisters to the insides of the legs.

“27th.—Died this morning.

“*Dissection.*—The coats of the stomach were much thinner than natural; the villous coat was found covered with small and numerous superficial ulcerations, which were still more

numerous near the cardiac orifice; they discharged a thin brownish fluid, which was very fœtid; the liver and spleen were sound. The small and large intestines appeared healthy."

Mr. Annesley condemns the practice in this case—and we have no hesitation in affirming, that "the inflammation supervening to dyspepsia," in this case, was induced by the treatment, and had no necessary dependence on the natural progress of the disease itself. But we cannot dwell on the stomach any longer, leaving the disorders of this organ to Abernethy and his followers; while the pathology will be best studied among Continental writers, who pay more attention to dissection than those of our own country.

#### DISEASES OF THE LIVER AND BILIARY APPARATUS.

It has often been remarked, that hepatitis is much more frequent in the Eastern, than in the Western Tropics. The returns of regimental sick show that it is at least treble in the former to what it is in the latter. In India, the average annual per centage of liver complaints, in the different divisions of the army, was estimated at 13 per cent. in the effective strength!

The increased secretion of bile observable in Europeans on their removal to a tropical climate, has been ascribed, by Dr. Johnson, to a sympathy between the skin and the internal organs. The sympathy is admitted by Mr. Annesley, but the doctrine is superseded by one first discovered by Crawford, Lavoisier, &c. and confirmed (as is said) by Dr. Copland, on the coast of Africa—namely, "that the quantity of carbonic acid gas, formed by respiration in a given time, is much diminished in high temperature, and under circumstances which lower the powers of life." This being established, they say, (for it is presumable that Dr. Copland speaks, together with the author) "it becomes a basis on which much *important speculation* respecting the origin of several intertropical diseases may be founded." Thus the diminished formation of carbonic acid gas in the lungs, during a high temperature, must increase the secretion of bile, and so on. We do not deem it necessary to discuss this chemical physiology. There is one little fact past over by the ingenious authors, which, in our humble opinion, proves fatal to this gaseous hypothesis. It is this. After a certain residence in hot climates, the torpid state of the liver is just as evident as the increased function of that organ was in the early residence. Yet the temperature of the tropics does not, we believe, alter, so as to accommodate the theory of one or other party. This fact does not militate against, but rather supports, the doctrine of sympathy between the skin and liver. In the long residents, when the biliary organ falls below par in function, the skin is in an analogous state—dry and constricted. Our authors have wasted a great many royal quarto pages in the pursuit of these theoretical phantoms, instead

of filling them with practical facts, that might turn to better use.

A number of common cases of increased secretion of bile, among newly arrived Europeans, are detailed with much minuteness, and then the treatment of "increased secretion of bile" is entered upon. With every disposition to be pleased with the work before us, we cannot help being grieved to see the systematic author so much predominate over the practical writer. By this procedure, the work has been rather injured, than benefited, for general circulation and use. We could substantiate this position by many extracts. The following quotation, in which the *dilution* of facts by words is really less than in almost any part which we could possibly pitch upon, will suffice.

"During our practice in India, we have had numerous opportunities of observing, in the *post mortem* inspection of those who had died of diseases either immediately seated in the liver, or affecting other organs, the gall-bladder distended with a thick, viscid, and acrid bile, and the ducts running from the secreting granula of the liver through its substance to their principal trunk, completely gorged with bile of nearly similar characters. In different cases, indeed, this secretion presented different appearances, as regards colour and consistence; but the engorgement of the ducts and gall-bladder was generally remarkable, without any apparent organic change sufficient to account for the circumstance. In the majority of instances, the outlet of the ducts in the duodenum was quite free, and their channels unobstructed, unless the viscosity of the secretion may be viewed as an impediment,\*—an inference that seems by no means irrational. Where any obstacle existed, such as narrowing of the ducts, the impaction of calculi in them, or the existence of spasm,—the cause was then evident; but in the absence of all these, the only conclusion we could form as to the cause of this very frequent appearance, was, that the secreting functions of the liver may be so modified in a warm climate, that, in addition to an increase of the biliary secretion, this fluid itself may be retained and accumulated in those parts of the apparatus which admit of the retention. Attentive observation of the phenomena, marking the origin and progress of the diseases of the liver and bowels, and of the various types of fever, has further confirmed our opinion as to this particular point, and con-

\* "We have frequently seen, upon the examination of bodies which had died of different diseases, the gall-bladder loaded with bile of a dark green colour, and so thick and viscid, that it could scarcely be forced through the ducts by squeezing the gall-bladder, although a blow-pipe or probe would pass readily along them, showing that the obstruction was then owing to viscosity alone. Doubtless, spasm, or other more permanent obstruction, will frequently arise, as we shall have occasion to show in the sequel."

vinced us that this state of function actually obtains at the commencement and during the progress of these disorders, more frequently than is supposed, and is actually oftener present at these periods of ailment, than in the last or fatal stage of disease; and that it is not only met with as a symptom or concurrent phenomenon in these disorders, but as an ailment *sui generis*, the disturbance observed in the system being the result of this cause, or arising from the irruption of the long retained bile into the alimentary canal.

"During an increased secretion of bile, if any momentary impediment come in the way of the flow of this fluid, either in the course of the common duct, or at its outlet, a copious regurgitation of it into the gall-bladder, and accumulation of it in the biliary ducts, must be the consequence; and if the obstacle placed in the way be either partial or complete, or of short or long duration, the accumulation will be in proportion to its extent and duration, and the copiousness of secretion. If the secretion be going forward abundantly, an obstacle, partial in its operation and of short continuance, will give rise to a great accumulation in the gall-bladder, and in the liver itself. If the secretion be natural, or even less than natural, a more complete or long continued impediment opposing its discharge into the duodenum will have a similar effect. Thus, in recruits and other strangers to the climate, on their arrival in India, when the biliary secretion is much increased, *the temporary obstruction produced by exposure to currents of cool air, to wet, and by eating indigestible and hurtful substances, &c. often occasion the most formidable symptoms of disease, and when the obstruction is overcome, an immense quantity of vitiated bile is passed.* On the other hand, temperate persons, of regular habits and good conduct, are not so liable to these kinds of derangements, and suffer less severely from them when they occur. It is also reasonable to suppose, if the gall-bladder and ducts be over-distended with the accumulation of bile within them, that their vital contractility may be weakened, and that they will be the less able to react upon the distending power; and thus the evil will be increased, until that degree of constitutional disturbance be excited by the morbid distention, or until some internal or external cause supervene, which shall enable the organ to throw off the load which oppresses it, and discharge its morbid secretions."

To say nothing of the vague reasonings in the foregoing quotation, what, we ask, becomes of the carbonic theory about the *increased secretion* of bile from atmospheric heat? Here we have currents of *cool air, wet, &c.* not checking the secretion, as it ought to do—no, it has not the least effect of that kind—it only checks, by some theory unexplained, the *exit* of the bile from the liver and gall-bladder. Now, the plain matter of fact appears to be, that the exposure to wet and cold checks both the perspiration and biliary secretion, and when reaction takes place—in other words,

when these secretions are restored, there is a redundancy of both fluids, as a necessary consequence. We think it would be difficult to adduce a passage from any work, so pregnant with gratuitous assumptions as the following:

"The obstructions which generally *occasion accumulation of bile* in the apparatus concerned in its secretion and discharge, seem to be whatever suddenly *diminishes the vital influence of the organ* or the system generally; as exposure to terrestrial and morbid exhalations, sudden chills, the depressing passions, the use of cold fluids and ices when the skin is perspiring, &c. Spasms of the common ducts may arise from these and other causes, and produce more completely the same effect. *A weakened state of the digestive organs, particularly of the duodenum and stomach, may also be productive of accumulation of bile, by furnishing a copious supply of ill-digested chyle, abounding with the elements whence bile is formed;* while, at the same time, the debility which these viscera experience extends itself to the gall-ducts and bladder; and the emulgent operation, usually produced by a healthy and active function of the duodenum, no longer takes place, or, if at all, in a lesser degree. *The accumulation of mucus on the internal surface of the duodenum may also obstruct the mouth of the common duct,* and prevent the flow of bile into the alimentary canal, until this obstruction be either overcome or removed."

But enough of hypothesis. The signs of accumulation in the gall-bladder or biliary ducts, cannot always be depended on, especially when viewed separately; but, taken in connexion, Mr. A. thinks they may be duly estimated by an experienced practitioner.

"The earliest symptoms of which the patient generally complains, when he attends to his sensations and state of health, are, clamminess and foulness of the mouth, fauces, and tongue, with a bitter taste, particularly in the morning; a sense of distention and weight at the epigastric region and at the præcordia, frequently with a sense of coldness and sinking in the same situations; slight anxiety; acid and acrid eructations about three or four hours after a full meal, with painful fulness at the epigastrium, and difficult digestion. The patient often complains of headach, pain in the back or loins, uneasiness under the shoulder blades, fulness and pain in the region of the liver, particularly when pressure is made at the time of his taking a full inspiration; and of aching in his knees, shoulders, and limbs; his countenance being pale, sallow, or muddy, and the conjunctiva more or less tinged of a yellowish hue. The state of the pulse is different in different cases. It is often slow and full, and sometimes it is irregular in frequency and strength; occasionally it intermits, and not unfrequently becomes quick, but oppressed upon the least motion or exertion. The urine is generally high coloured, and depositing a brownish sediment. The stools are often costive, sometimes light or clay-coloured, and frequently tenacious. When the accumulated bile is discharged into the alimentary canal,

much constitutional disturbance then generally arises, according to the qualities which this fluid may have acquired from its retention. The pulse now becomes quick, and often irregular; vomiting and purging, with griping, pain, and anxiety, often supervene, sometimes with spasms. Thirst becomes urgent, and the tongue, which was before foul, is now excited, often dry, and its papillæ large, distinct, and erect."

The ultimate effects of these accumulations and subsequent overflowings of vitiated bile, will be various in different individuals. In one it will produce simple bilious diarrhœa—in another, sporadic cholera—in a third, simple dysentery, or inflammation of the bowels, or even the stomach, when the bile regurgitates into that organ. In some instances, though not very frequently, the inflammation will be found confined to the duodenum, as was verified by the following case and dissection:

"A female, leading an irregular life, came into the hospital complaining of all the symptoms of bilious accumulations of a morbid character, with much debility, a broken-down constitution, quick, feeble, and fluttering pulse, nausea, and vomiting of dark green bilious matters, slight purging of dark bilious and fluid motions, coldness of the surface, sunken countenance, and pain and anxiety at the pit of the stomach and right side. Blisters were applied to the epigastrium; laxatives with ammonia were given internally, and enemata of an aperient and cordial kind thrown up. She died soon after admission, and the body was inspected within twelve hours after death. Upon examining the alimentary canal from the œsophagus to the rectum, and exposing its internal surface throughout, the duodenum was found highly inflamed from the pylorus to the jejunum, the upper portion of which latter was also inflamed. A part of the duodenum, a little below the entrance of the ducts, was sphacelated. A few red points were observed in the stomach and other parts of the alimentary canal; but these were not more numerous or extensive than what are often remarked in cases of death from diseases in which the functions of the alimentary canal were unaffected. The portal veins were turgid; the liver somewhat enlarged. There was no other morbid appearance."

The presence of vitiated bile in the duodenum sometimes occasions an alarming state of depression and prostration of the vital energies—especially in nervous and melancholic temperaments. Mr. A. thinks that, in those cases where the natural functions of the bowels have been impeded by accumulations of viscid bile, "the irruption of morbid bile is productive of much less constitutional disturbance, and is even beneficial, inasmuch as it detaches this matter from the mucous surface, and leaves it free and unencumbered in the performance of its functions." Cases in illustration of biliary accumulations are detailed, and the author next proceeds to a new section.

#### CONGESTION OF BLOOD IN THE LIVER.

This Mr. A. supposes to be a much more frequent occurrence in this as well as in tropical climates than is imagined. He conceives that it is present in the early stage of the majority of febrile diseases—particularly those which are idiopathic—and that it is not generally overcome until after the stage of excitement has been fully formed. The rationale of this state of hepatic congestion is substantially the same as was given by Dr. Johnson many years ago—namely, the peculiarity of the portal circulation, with reference to the general circulation. Mr. Annesley conceives that this hepatic congestion not only plays an important part in fevers and many other inter-tropical diseases, but leads to hepatic inflammation, the great scourge of Europeans in India.

"We have already alluded to the existence of congestion of the liver, during the progress and decline of other diseases. This is particularly remarkable in the history of the dysenteries of India, and in the remittents, intermittents, and continued fevers of that country, and indeed of other inter-tropical regions. Even in the dissection of those cases which terminate fatally, whether from fever of whatever type, from dysentery, from cholera, either simple or epidemic, or from disorders of the other abdominal viscera, and even in those more particularly affecting the head or chest, great congestion of the vessels of the liver is not unfrequently observed. Nor can the appearance be considered more the consequence of death, or of the changes immediately preceding dissolution, than previously existing disorder; for the attentive observer may often remark the signs usually characterising congestion of the liver, during the life of the patient, or may trace an obvious connexion between this condition of the viscus and the disorder of which the patient died."

The anatomical characters of this congestion of liver are well illustrated by plates, which, from the size and price of the work, are unfortunately beyond the reach of the profession generally. We may shortly state that the viscus is usually much increased in size, particularly the right lobe, and in a direction upwards into that side of the thorax, forming a large segment of a circle. The colour of the organ is generally changed by the congested state of its vessels, and seems to depend on the particular sets of vessels which are the seat of this plethora, and also on the absence or co-existence of accumulations of bile in the ramifications of the hepatic ducts.

"In some cases, the surface of the liver is of a darker brown than natural, almost amounting to black, greenish black, or bottle green, and this deep colour in some instances passes very abruptly into a reddish or light brown tinge. Sometimes the surface of the congested liver is variously mottled, or marbled, and occasionally it is streaked and clouded, of a yellowish brown, greenish black, or yellowish green hue. These shades of colour are gener-

ally more remarkable upon its upper or convex surface, but they are often observed upon the concave surface, and are quite independent of any effects which may have been produced by the bile contained in the gall-bladder. Sometimes the surface of the liver is very dark; and yet, upon cutting into its substance, the subjacent texture is of its usual colour.

“When cut into, the substance of the liver is, however, generally darker than usual, and gives out a large quantity of dark fluid blood: but in regard to fluidity, there is much difference, according to the period which has elapsed from the time of death to that of inspection. In India, where the *inspectio cadaveris* is usually made a few hours after death, the blood is observed, in cases presenting congestion of the liver, of a fluid or semi-fluid, or thick consistence, and of a very dark colour. The portal vessels and the hepatic veins are the seats of congestion, and it is often difficult to say which of the two sets of vessels presents this appearance to the greater extent, or more frequently; but we believe that the hepatic vein is more generally congested in the greater degree. In many cases, the congestion of the blood-vessels and accumulations of bile in the biliary ducts, although existing to a great extent, are insufficient to account for the very great increase of the size and weight of the liver, showing that these appearances are often connected with augmented size of the viscus, independently of the extent to which they could have increased its bulk, and of any organic disease. On some occasions, congestion and accumulation of bile have been considerable, without any very marked augmentation of size; but more generally, congestion of the blood-vessels, particularly when associated with accumulations of bile in the biliary ducts and gall-bladder, gives rise to increased size of the liver; and such increase is often in relation to the extent to which congestion of the blood-vessels and biliary ducts obtains.”

The appearances of the bile are various. Sometimes it is pale, deepening, in different subjects, from a straw colour to an orange—and from that down to yellowish green—green—dark bottle green, &c. In the lighter shades the bile is generally most fluid, and *vice versa*. Upon making slices of the congested liver, the divided mouths of the distended ducts appear round or oval, according to the direction of the incision—and, in some instances, small granular or miliary calculi are found in the ducts. In cases presenting the greatest degrees of congestion and biliary turgescence, the viscosity of the bile appeared to our author to have given origin to the formation of these small calculi in the substance of the liver. The cystic bile, in these states of congestion, is generally of a green colour of various shades and consistences.

The above morbid appearances are often seen accompanying organic diseases of this viscus.

The *symptoms* which denote, in the living body, these congested states of the biliary vessels, cannot be individually depended on. They must be viewed in connexion.

“When, however, the countenance is pale, anxious, inexpressive, sallow, of a dark or muddy hue; when the tongue is covered with whitish or yellowish-white fur, or otherwise loaded; when the bowels are costive, or when the stools are morbid, dark, and watery, with griping and tenesmus; when the digestion is difficult, attended with nausea, or when the appetite is diminished, and the patient complains of pain and oppression at the scrobiculus cordis, particularly after a meal, with flatulence, borborygmi, and oppressed breathing, and a difficulty of filling the lungs to their utmost; when the skin is cool, clammy, and foul, or of a dark muddy tinge, with irregular chills, sometimes approaching to rigours; when pain, fulness, weight, and oppression, are experienced in the region of the liver, and at the epigastrium, or across the shoulder-blades, or beneath the scapula, and have supervened suddenly; when the uneasiness in those situations is increased upon a full pressure and full inspiration; when the pulse is full, slow, and irregular, or when it is quick, but oppressed; when there is headach, restlessness, disturbed sleep, with unpleasant dreams; and when the urine is turbid, or presenting a muddy sediment,—we may infer that congestion of the vessels of the liver is actually present.”

It must be remembered that all, or even the majority of these symptoms are not to be expected in the same individual, although many of them may be recognised in different grades. The state of the pulse is very variable, and not to be depended on. Mr. Annesley thinks that, although pain, oppression, weight about the epigastrium, or under the scapulæ characterise, in general, inflammation of the substance of the liver, yet that these are often marks of congestion also—especially when they supervene suddenly, and are attended with many of the symptoms already described. Inflammation does not arise or reach its acmé in a few hours, but congestion may. Neither can pain, he thinks, be always considered indicative of inflammation, since the membranes of the liver are put on the stretch by congestion. The causes of this congestive condition of the biliary organ are those which have been already portrayed—high atmospheric temperature—too much animal food—too highly seasoned dishes—“indolence and insufficient exercise in the open air”—inordinate use of spirituous liquors.

#### TORPOR OF THE LIVER.

We were rather surprised to find this section rise in view, after the brilliant doctrine of increased secretion of bile on carbonic acid principles. The torpid condition of this apparatus, however, could hardly have escaped the notice of Mr. Annesley, though both he and his hypothesis manufacturer appears sadly at a loss to account for the phenomenon. After a great deal of physiological and pathological speculation, the meaning of which is far beyond our comprehension, we come to the pith of the business, in the following short passage.

"Torpor of the liver, then, may arise simply from a diminished or exhausted energy of the secreting functions of the organ; and, from this state, complicated with accumulations of bile in the biliary ducts and gall-bladder, and with congestion in the blood-vessels of the organ; the former state of disorder gradually superinducing, and becoming complicated with, the latter derangements."

Torpor of the liver, Mr. A. observes, is generally complicated with dyspepsia—"and not unfrequently originates in that disorder." As the increase of the secretion was accounted for by the diminished production of carbonic acid gas in the lungs, we wonder that Mr. A. and Dr. Copland did not try to connect the diseased secretion with some modification of the same doctrine. Instead of the carbonic theory, the following explanation is given, which indicates that the doctrine of sympathy was not entirely annihilated in their minds.

"Over-excitement, also, of the perspiratory functions, from long-continued marches, fatiguing exercises, and too warm clothing, is not infrequently productive of considerable exhaustion of the secreting actions of the liver, and often disposes it to torpor, venous congestion, and accumulations of bile in the biliary ducts, upon the slightest exposure to cold, to moisture, to the impression of malaria, and when the depressing passions are brought into operation, or when hurtful or indigestible matters are taken into the stomach."

Whether those exposures to cold, moisture, malaria, &c. do not influence the cutaneous secretion, we leave to our readers to determine. The symptoms indicative of this torpid state of the biliary organ are next delineated. It is acknowledged that these symptoms are not always so unequivocal as could be wished.

"If, however, we find the patient to complain of want of appetite, drowsiness, with pain over the eyebrows, lowness of spirits and hypochondriacal feelings, dark and high-coloured urine, a costive state of the bowels, and pale or clayey motions, a dark or sallow countenance, wasting of the flesh, slow and painful digestion, with the symptoms noticed in a previous section as constituting diminished function of the stomach, flatulency, particularly of the bowels, without any evident fullness or enlargement in the region of the liver, but with a bitter or disagreeable taste of the mouth, and a loaded state of the tongue, particularly in the morning,—we may reasonably infer that the functions of the liver are inadequately performed; but it is by no means so easily to be determined whether or no such torpor is the result merely of diminished function, or of change of the structure of the organ, unless we are acquainted with the patient's habits and the nature of his former ailments. When the foregoing symptoms occur in one addicted to the use of spirituous liquors, or in one who has resided long in a warm climate, and suffered former attacks of hepatic disease, then the latter alternative may be more reasonably inferred."

After detailing a sufficient number of cases

illustrative of this torpid state of liver, Mr. A. proceeds to the treatment of these functional disorders of the biliary apparatus. This may be summed up in a very concise manner. Where plethora exists, and the patients have been living too free, blood-letting is recommended, as the first step—and local depletion afterwards, if necessary. The antiphlogistic system should be strictly adopted, and the bowels to be kept well cleared by purgatives. Where sickness and bilious vomitings obtain, warm water is ordered, and afterwards a brisk dose of calomel. Even when all the morbid secretions are cleared away, Mr. A. recommends a full dose of mercurial at night, with aperient draughts in the morning, with the view of changing the secretions of the liver, and effecting a healthy flow of bile. If the mouth becomes affected, under this treatment, "a healthy state of function of the liver is the more likely to supervene speedily." It is not the object, however, of Mr. A. to affect the mouth by mercury. In many cases, where the rush of vitiated bile into the duodenum occasions distressing symptoms, cordials will be necessary before the purgative plan is put in force. In respect to that form of disorder, which has been termed torpor of the liver, Mr. A. found a full dose of calomel at bedtime, followed by a bitter aperient medicine in the morning, the most beneficial practice, with blisters over the epigastric or hypochondriac regions. After a few days of this treatment, the pilula hydrargyri combined with the pil. alois. cum myrrha, is prescribed at night, with the bitter aperient in the morning. The practitioner is warned against the exhibition of tonics and stimulants for the apparent debility which accompanies these biliary derangements.

This carries us through full half of this immense volume, and to the end of functional disorders of the biliary apparatus. In another article we hope to afford our readers a general view of the remaining chapters dedicated to inflammation and to various organic diseases of the liver. We shall withhold any general observations on the work till the close of our analysis, hoping by that time to enable every reader to judge for himself. We cannot help, however, again expressing our regret, that the letter-press of this volume should have been so very much expanded by disquisitions that might have been spared, and by didactic precepts far too much spun out. It is probable, indeed, that the talented author may think it much better to be needlessly minute than unsatisfactorily brief. On this point he may be right and we may be wrong.

From the Edinburgh Medical and Surgical Journal.

**CASE OF AMPUTATION OF THE THIGH**  
*during the progress of Gangrenous Inflammation.* By GEORGE MACDERMOTT, Esq.  
Surgeon of the 4th (king's own) Regiment.

Private John Stokes, 4th (the king's own) regiment, aged 22 years, tall and slender, of a

florid complexion, was a young man of sober habits, and generally healthy. While the regiment was quartered at Lima, in Portugal, he fell on the 16th May, 1827, at eleven A. M., from the window of a convent upwards of thirty feet high, dislocated his right shoulder, and fractured both bones of the right leg, about four inches above the ankle joint. The fracture was compound; but the wound in the integuments was very small, and just over the spine of the tibia.

The man was immediately brought to the hospital, where I saw him a few minutes after the accident. He was then pale and exhausted from the combined effects of fright and injury. I reduced the dislocation with the greatest care, the muscles not having then recovered tone sufficient to present any obstacle. The leg was much swelled, and a considerable effusion of blood had taken place underneath the integuments, which were detached about two inches from the bone. After the reduction of the dislocation the man was laid on a bed, and the limb placed in a straight position. There was no tendency to retraction, and therefore no difficulty in preserving its due length. Old linen wet with the saturnine lotion was then spread over the fracture. From the facility with which the bones were kept in apposition, I concluded that the fracture must be transverse. On the day after the accident the leg was still more swelled and painful; but the temperature was not increased, and there was no symptomatic fever, nor any decided marks of inflammation. Pulse 84; skin soft and natural. He had an opiate the night before but with very little benefit; but this night I increased the dose. It procured him some refreshing sleep, and in the morning every thing seemed to be going on well. The constitution remained undisturbed, the swelling of the leg was diminished, and the effused blood seemed to be in a regular course of absorption.

From this period until the fifth day (20th) no very unfavourable symptoms, either locally or constitutionally, had taken place. But on that day the leg became much swelled, œdematous, and of a deadly pale colour; and he complained of a burning heat in the instep. Still he was young and strong, and therefore might yet do well; but on that night the most rapid and disastrous change took place. On the morning of the 21st I found the fore-part of the leg, from the knee to the instep, in a state of gangrene; and close to the fracture the integuments, cellular membrane, and superficial muscles, were completely sphacelated. The disease was advancing rapidly, and had already reached half way up the thigh, and his constitution was fast sinking under it. His stomach was irritable, his countenance sunk and alarmed in its expression, and his pulse low, frequent, and weak.

Under those circumstances the vigour of his constitution seemed to afford the only chance of saving his life, which must otherwise have inevitably terminated in the course of a

few hours. I determined on giving him that chance; and, with the aid of my assistant, Mr. Parry, proceeded without delay to perform the operation. I took off the limb about three inches below the *trochanter major*, which was as low down as it could be done, the integuments even there being discoloured and œdematous, showing the near approach of the gangrenous inflammation. He bore the operation with considerable patience and fortitude, but sunk extremely low after it. Three arteries required ligatures; and after they were tied the stump was dressed with sticking plaster, and lint spread with simple ointment laid over it.

After he was put to bed his stomach rejected some wine which he had taken during the operation. The skin was then covered with cold sweat; the pulse low and feeble; the eyes had a fixed vacant stare; and altogether his countenance exhibited such distress and anxiety as led me to apprehend that serious, perhaps sudden and fatal consequences, would follow the operation. Small quantities of brandy, with a few drops of laudanum, were given to him frequently, and after a few hours he recovered a little. His stomach, however, continued occasionally to reject whatever he took throughout the day; but his pulse, though low, was more distinct and firm. At night he took two grains of opium in a pill. Though it did not procure much sleep, it rendered him composed and tranquil; and in the morning he had evidently rallied considerably. His pulse was more firm and not so quick or small; the skin was soft and moist; and his countenance had assumed its natural expression. During this day his stomach became retentive, and in the evening he was altogether much improved. For several days after this the suppuration was so considerable, and the sloughing of the cellular membrane so extensive, as to require the free administration of wine, tonics, and nourishing food. I apprehended for some time that exfoliation of the point of the bone would take place; but this fortunately has not occurred; and the stump is now (3d July) perfectly healed, and the young man in the enjoyment of excellent health.

On examining the limb after amputation we found that a splinter of bone three inches long was completely detached from the tibia, (longitudinally,) and forced down between the lower portion of that bone and the fibula. The fracture was transverse, as we supposed it to be.

I have been induced to send the above case for publication, as it seems to me important, from involving a question of great consequence in practical surgery; and its fortunate termination is a strong proof of the propriety of amputating without waiting for a line of *demarkation* to be formed, when the progress of mortification, as in this instance, threatened not only the loss of limb, but of life also.

*Lisbon, July 25th, 1827*

From the Edinburgh Medical and Surgical Journal.

*Ueber die Punction des Chronischen inneren Wasserkopfs, &c.*

*On the Operation of Puncture in Chronic Hydrocephalus.* By DR. FREDERICK WILLIAM OPPENHEIM, Physician and Surgeon, Hamburg. (*Rust's Magazin für die gesammte Heilkunde*, B. xxiv. 1827.)

In publishing an additional instance of an attempt to cure chronic hydrocephalus by the operation of puncture, Dr. Oppenheim has laboriously collected and analyzed all the cases of the kind previously made public, and by collating them, and comparing their results with the opinions of the best modern authors, has endeavoured to discover how far this mode of treatment deserves to be countenanced. He has in consequence arrived at the conclusion, that the particulars of the cases are by no means discouraging; that the objections stated by the writers who have discountenanced the operation are none of them very weighty; and that, if resorted to at an earlier stage of the disease, and under less desperate circumstances than in the instances in which it has hitherto been tried, there is reason to expect that it will not so often disappoint the hopes of the physician. The importance of Dr. Oppenheim's opinion, and the want in our own language of an analysis such as he has drawn up, will render the following short review of his paper not unacceptable to the English reader.

We shall commence with an account of the author's own case, and an abstract of the analysis he has given of the rest, fifteen in number, which he has collected from various quarters.

The subject of Dr. Oppenheim's case was a female infant 27 weeks old, who enjoyed good health for the first five months, but then began to exhibit symptoms of hydrocephalus, with enlargement of the head. At the date of the operation the head measured 18 inches at its greatest circumference,  $11\frac{1}{4}$  from the root of the nose to the occipital protuberance,  $7\frac{3}{4}$  from one parietal protuberance to the other,  $13\frac{1}{4}$  from the *foramen magnum* to the *alæ nasi*. The sutures were separated considerably, with the exception of the frontal; the eyes were so much turned downwards that the pupils could not be seen. Before the puncture was made, the head was surrounded horizontally by a circular bandage, having attached to it at right angles four others, of which two were carried round the arm-pits forward and tied on the back, while the others were carried round the arm-pits behind and tied over the breast. The purpose of this arrangement was to afford points of resistance to eight linen straps which were attached to the upper edge of the circular bandage, and which, going through eight holes in a round piece of strong linen laid on the vertex, could be tightened at will by being pulled downwards and fastened on the circular bandage. This very convenient and

effectual apparatus for maintaining a steady and general pressure being properly secured, the first puncture was made on the left side of the anterior fontanelle with a trocar and canula half a line in diameter; it was made to penetrate an inch obliquely; and an ounce and a half of a clear serum were withdrawn. The canula was then closed with wax, and secured in its place by plaster, and the bandages were properly tightened. The infant did not seem to suffer any pain, slept well in the course of the afternoon, and sucked vigorously. On the *second* day it was found that a good deal of fluid had trickled out, in consequence of the mother moving the child too much. Nevertheless two ounces more were withdrawn, and the bandages tightened. On the *third* day the child had several attacks of vomiting after taking powders of calomel and foxglove, which had been prescribed the evening before; but the head was diminished in size, the pupils were visible, and they acted readily. Serum had oozed from the wound. Three ounces were withdrawn by the canula, and the bandages again tightened. On the *fourth* day the canula was expelled during a fit of vomiting, the child was feverish, and the puncture had suppurated. The puncture was therefore closed with plaster, and cold fomentations were applied to the head. On the *fifth* day the fever having disappeared, a puncture was made on the right side of the fontanelle, fresh bandages were applied, and two ounces and a half of fluid were withdrawn. The cold fomentations were continued. On the *sixth* day the child was pale and motionless, hardly breathed, and refused the breast. The canula was therefore removed. On the *seventh* day it expired.

On dissection the greatest circumference was found to have been diminished two inches; the dura mater was very little thickened; the lateral ventricles greatly distended with fluid, the fourth ventricle natural; the brain soft and pulpy, its cortical not distinguishable from its medullary part; the cerebellum very soft and pulpy; the vessels at the base of the brain natural; the two punctures in the dura mater open, not inflamed, and the trocar had not penetrated deeper (?); above a pound of fluid was contained in the cavity of the skull.

Such is the substance of our author's narrative. The annals of surgery do not contain a history more unfavourable to the operation.

The cases which he has collected from other writers are fifteen in number. Two, however, are mere notices, and may be left out of the account. In four of the remaining thirteen the death of the patient was evidently more or less connected with the operation; for in three of them it took place within ten days after it, and in a fourth the child appeared to be exhausted by hemorrhage. In the other nine cases there was in general marked relief and abatement of the symptoms after each tapping, and the patients lived from 31 to 172 days after the first; in none was death distinctly connected with the opera-

tion; in several there was, in the opinion of the respective practitioners, a fair promise of ultimate success; and, according to Dr. Oppenheim, one recovered; but this statement we shall presently find not to be correct.

The four unfavourable cases were the following. 1. Lecat operated on a child three months and a-half old, affected with hydrocephalus from the tenth week after birth. The sutures were considerably separated, and the eyes squinted. He took away five ounces thrice in three days, a canula being kept all the while in the opening; and the child died on the fifth day after the first puncture. The ventricles of the brain were much expanded, and the pineal gland nearly all destroyed.—(*Phil. Trans.* xlvii.)

2. Dr. Whitmore of New-York, in similar circumstances, punctured a child six months old, in whom the disease began in the sixth week; and in eight days drew away, at fifteen different periods, from one opening, without a canula, 116 ounces. The child died two days after the last discharge of fluid; and the membranes were found inflamed.—(*American Med. Recorder*, July 1821.)

3. Dr. Hood of Liverpool punctured a child nine months old, who had been affected since the third week after birth. Six ounces were withdrawn by a trocar, more oozed from time to time out of the wound, and death took place on the third day. The brain was softened, and the *tubercula quadrigemina* had passed into suppuration.—(*Edin. Med. Surg. Journ.* October 1821.)

4. Mr. Brown of Preston operated on an infant five months old, and at first drew off nine ounces and a half. For 28 days afterwards the mother would not allow the puncture to be repeated. In the course of the eight succeeding days three new punctures were made, and from two of them fluid was withdrawn twice. On these five occasions 25 ounces were evacuated. Two of the punctures caused hemorrhage, one of them to a very great amount, and the child seemed to die of it namely, 36 days after the first operation.—(*Lond. Med. Phys. Journ.* li.)

The nine more favourable cases may be arranged according to the length of time they survived the first puncture.

5. Mr. Gray operated on a child nine months old, whom at the age of one month he had cured of *spina bifida* by compression, but who was attacked with hydrocephalus not long afterwards. At three tappings, with an interval of ten days between them, he drew off 45 ounces. The child died 31 days after the first. The symptoms of pressure were mitigated considerably after each.—(*Lond. Med. Phys. Journ.* liv.)

6. Dr. Frekelton of Liverpool operated on a child eight months old, who had the disease for three months. He employed successive punctures with a trocar, drawing away on the 1st day eight ounces, on the 2d seven, on the 7th two, on the 14th and 21st seven, on the 56th sixteen. Death took place on the 59th, and the brain was found expanded with 48

ounces of fluid.—(*Edin. Med. and Surg. Journal.*)

7. Mr. Callaway operated on a child five weeks old, and drew away on the 1st day four ounces, on the 14th, 21st, 28th, and 35th, about as much; no bad symptom followed any of the punctures, the head evidently decreased in volume, and the hearing and appearance of the countenance improved. Nevertheless the child died of gradual exhaustion and marasmus about the 70th day.—(*American Med. Recorder*, July 1821.)

8. Mr. Remmett operated on a congenital case two months after birth. In the course of the first six days he punctured the head thrice, and drew away 36 ounces in all; four weeks afterwards he drew away 12 ounces, and four weeks after that 32 ounces. Till three weeks after the last puncture the child did well, but it then fell into general ill health, and died in ten days, consequently 93 days after the first operation. The punctures always healed two days after they were made.—(*Edin. Med. Comm.* vi.)

9. Mr. Money operated also on a congenital case ten months after birth. On the 1st day he withdrew four ounces, on the 4th two drachms, on the 7th five ounces, on the 13th three, on the 21st three and a-half, on the 27th three, on the 46th four, on the 55th three and a-half, on the 66th four, and on the 74th two ounces. The child died on the 84th day, and the brain was found rather soft and dilated into a sac which held 35 ounces. Till the fifth tapping, the operation was followed each time by alarming faintness.—(*Lond. Med. Phys. Journ.* lii.)

10. Mr. Sym of Kilmarnock operated on an infant eleven weeks old, that had been five weeks ill, and in whom the integuments were so tense that sloughing was dreaded. In the course of 90 days he punctured the head five times, drawing away seven ounces at an average each time. Temporary relief was always obtained, and after the last puncture the water ceased to accumulate. Fourteen days afterwards, or 104 days after the first puncture, the child died; and the membranes were found thickened, and two pounds and a-half of fluid within the head.—(*Edin. Med. Surg. Journ.* xxiv.)

11. Dr. Glover of the United States operated in a case, apparently congenital, nine months after birth. In four months 156 ounces were withdrawn at eight tappings. Much temporary and considerable permanent amendment was procured; nay, after the last tapping the patient had strength to pass prosperously through an attack of cholera; but a continued fever afterwards made its appearance, and death took place on the 120th day. The dura mater, though thick, was not inflamed.—(*New-York Med. Repos.* iv.)

12. Mr. Lizars operated on an infant seventeen weeks old, affected with the disease since its sixth week. In the course of 90 days he punctured the head sixteen times, at intervals of three, four, or seven days, and drew away each time from three to seven ounces; once

only he withdrew ten ounces. After the last puncture on the 90th day, the convulsions with which the patient had been occasionally attacked ceased to return, and altogether matters promised well. About this time the irritation of teething seemed to be the cause of the prospect changing. On the 171st day, fluctuation having returned, three ounces and a half were withdrawn with temporary relief; but the convulsions returned, and death ensued next day. The brain formed a sac containing three pounds of fluid.—(*Edin. Med. Surg. Journ. April 1821.*)

13. Dr. Vose of Liverpool operated on a case, apparently congenital, seven weeks after birth. At the first tapping he withdrew three ounces and a-half, on the 16th day five ounces, on the 38th eight, on the 47th twelve. After that the head did not increase, the sutures ossified, and a watery diarrhoea of four days' continuance appeared to carry off the remains of the disease. Three months later, when the author's account was drawn up, the patient continued well; and accordingly this case has been set down by Dr. Oppenheim as an instance of recovery. We are informed, however, by Dr. Monro (*Morbid Anatomy of the Brain*, p. 146,) that symptoms of pressure manifested themselves after the sutures were ossified, and the child ultimately died,—under what precise circumstances we have not been able to learn.—(*Lond. Med. Chir. Trans.* ix.)

We have now to consider the commentary and conclusions which Dr. Oppenheim has formed on reviewing these cases. We shall not follow him through his enumeration of the opinions of authors on the subject. It matters little what were the sentiments of any writer, except in recent times; because till lately they were founded solely on the results of cases in which the whole fluid which would issue was withdrawn at once,—cases that appear to have uniformly proved fatal very soon after the operation. As to recent authors, by far the greater number either pointedly forbid it, or represent it as holding out the faintest possible prospect of success. Such, however, is not the opinion of Dr. Oppenheim.

The objections which have been urged against the practice, and which he has taken under consideration, are the following. 1. That the mechanical injury done to the brain and its membranes is apt to induce inflammation; 2. That the entrance of air into the inside of the skull may have the same effect; 3. That much immediate danger may flow from the collapse of the brain; 4. That the operation has never been successful, or at least that the cases of that description are not more numerous than those of spontaneous cure. The last proposition is not very distinctly laid down by him, but he discusses it in the course of his reply to the objections.

In this reply he insists, 1. That the brain and its membranes have been repeatedly injured in a state of health without any material bad consequence; and in particular that none of the cases of puncture for hydrocephalus proved fatal by inflammation; 2. That with

ordinary precaution the air may easily be prevented from entering by the wound, and that, although it does enter, it is not so injurious as has been alleged; for in Mr. Lizars's case, in which it did effect an entrance, no material ill effect ensued; 3. That the danger arising from sudden collapse of the brain, certainly a real one, may without difficulty be obviated by withdrawing the fluid gradually, and making corresponding pressure by a proper bandage; and, 4. That spontaneous cures are so rare that his countryman Zang, who opposes the operation on the ground that such cures are as common as cures by puncture, could not say he had ever seen any; and Dr. Baillie of London only knew of one case, and that an equivocal one.

He then goes on to observe, that, according to the results of the cases he has collected, the operation does not in the least disturb the vital functions, and in particular does not injure the brain or its membranes; that the brain, even in its internal parts, is tolerably insensible; that the size and form of the brain do not contribute to its functions; that at least one case, that of Vose, is on record, in which a cure was effected; and that the prosperous result in that instance, and the obvious amelioration produced in many others, although in all the disease had arrived at an advanced and desperate stage, entitles the practitioner to expect a more fortunate result from the operation at an earlier period. In fine, he maintains that, the disease being otherwise inevitably fatal, it is the duty of the physician not to exhaust the patient's strength in unavailing trials with internal remedies, but to resort to puncture of the head *as soon as he has satisfied himself that effusion has taken place.*

It does not appear to us that the author's statement of objections is altogether fair, or his arguments against them sound, or the facts on which he rests in forming his conclusions correct. We shall close this analysis therefore by laying down what we conceive to be the leading obstacles to the adoption of the proposed method of cure.

The objections drawn from the risk of air entering the cavity of the skull, or of the brain collapsing, or rather, we should say, of the brain being deprived of the pressure necessary to the right exercise of its functions, are certainly immaterial: They may both be avoided by simple precautions.

It is also certainly true, as Dr. Oppenheim argues, that the brain and its membranes have, while in their natural state, sustained injury without any particular disturbance of the health. But he cannot be ignorant that the reverse is oftener the fact. Nor can his assertion, that puncture of a hydrocephalic head has never excited inflammation in the reported cases, be admitted as altogether accurate. Whitmore's case (No. 2) proved fatal two days after the operation, and the membranes were found inflamed; Hood's case (3) evidently ended in *remolissement* and suppuration of the brain; it is probable that Lecat's case (1) was nearly of the same

nature; and we cannot help thinking that Dr. Oppenheim's own case likewise terminated in inflammation and softening of the cerebral and cerebellar tissue. Nevertheless, it is remarkable how frequently the brain and its membranes were pierced in the remaining cases without inflammation being caused; and it will presently be seen, that, in the four cases in which this result did ensue, the irritation to which the brain was subjected was unnecessarily great. On the whole, under proper management inflammation need not be much dreaded, much less than in many other surgical operations.

The fourth objection of the opponents of the operation, that it has never been successful, we maintain must stand uncontradicted. It has already been mentioned that the child, supposed by Dr. Oppenheim to have been cured by Dr. Vose, did really die at last of the disease; a short notice of a successful case extracted by him from *Frorieps's Notizen*, cannot receive from us more credit than is attached to it by our author, who thinks the circumstances too imperfectly related to justify him in adopting it; and as little can any sound inference be drawn from another short notice of a successful case by Rossi of Turin, which has been quoted by Dr. Monro, but at the same time condemned by him as not sufficiently authenticated. (*Morbid Anatomy of the Brain*, p. 146.) There is then not a single well established instance of success on record. With regard to the amelioration produced in nine of the fourteen cases, it must undoubtedly be admitted, that the immediate effect of the operation was in every instance to relieve for the time the symptoms caused by the pressure of the fluid on the brain. But it may justly be doubted whether any permanent advantage was derived from it in more than five of them, (Nos. 5, 7, 11, 12, 13.) Nay, the reality of a permanent amendment even in them admits of doubt; for it is not satisfactorily proved with regard to any of them, by a relative measurement of the head, and of the progressive portions of fluid withdrawn, that there was a gradual decrease in the secretion of the fluid. After the punctures had been repeatedly performed, indeed, there happened in four of them (7, 10, 12, 13,) a sudden temporary arrestment of its augmentation; but this is no more than occurs in the ordinary course of hydrocephalus, and is not what would be expected to happen under the influence of a remedy, which is not merely palliative and subsidiary, but, as our author supposes, the principal and efficient means of cure. Did the operation strike at the root of the disease, as Dr. Oppenheim seems to imagine it will, if resorted to at an early period, it ought to be seen acting gradually on what is, if not the cause of the fatal termination, at least the sure sign of its approach,—the increasing effusion. It ought to produce a gradual reduction of that increase,—an effect, which has not yet been clearly proved to occur in any of the published cases.

This observation leads to the remark, that Dr. Oppenheim has omitted the most material objection to puncture as a remedy for hydrocephalus,—namely, that it is a mere palliative, that at best it puts off the fatal termination of the disease, by relieving from time to time urgent pressure on the brain, and that it does not in the least affect the derangement of function or structure, whatever that may be which gives rise to the effusion and other symptoms. This is apparent in all the cases to which it has been hitherto applied; nor can one easily see how it could be otherwise, although the operation were performed at the early period which our author insists upon. The operation, therefore, must be subordinate to the action of internal remedies, or the efforts of nature, in removing the tendency to increased exhalation. It is unnecessary to call to remembrance the little trust which can be put in art or nature for accomplishing this end, when unaided by the operation of puncture; it has been shown in the foregoing pages that the concurrence of the operation gives but little reason for increased confidence in the advanced stage of the disease; and it remains to be seen whether the physician will find himself better circumstanced by resorting to it at the beginning of the disease.

Although we entertain great doubts whether Dr. Oppenheim is borne out in his sanguine hopes, we would by no means insist on the operation being abandoned. It may be well to conclude, therefore, with a few words of caution, as to the method of performing it, derived from the particulars of the cases which have been collected by the author.

The best method of performing the operation seems to be by means of a small trocar and canula, but the canula certainly ought not to be left in the wound, as was practised by Dr. Oppenheim; there is evidently much less risk in repeated punctures; and it appears that all the cases, in which the canula was left in the wound, proved rapidly fatal. The punctures should not be frequent; when made once in four or seven days, they have always healed easily, without causing any irritation: On the contrary, when the fluid has been withdrawn frequently, either with or without fresh punctures, death has speedily ensued; when the secretion therefore is so rapid as to require very frequent punctures, the operation may be abandoned. The quantity of fluid withdrawn each time should not be great, not more, if possible, than from four to seven ounces. A single large tapping, as practised before Lecat's time, in general proves rapidly fatal, probably by the irremediable removal of that degree of pressure which Magendie has proved to be necessary for the exercise of the cerebral functions. Finally, great care must be taken to keep up, by means of such a bandage as that of Dr. Oppenheim, a steady gentle pressure of the skull, according as it diminishes in volume by the diminution of the fluid.

From the London Medical Gazette.

## VACCINATION.

NO. II.

*To the Editor of the London Medical Gazette.*

Sir,

Whatever difficulty there may be in determining the exact proportion in which vaccination fails to impart that "charmed life" which was its early attribute, one thing is clear, that the cases of failure have been sufficiently numerous to attract forcibly the attention of the public. Incompetent to reason correctly concerning these occurrences, and wanting the means of tracing them to their true, but obscure causes, that public has, nevertheless, made efforts to improve their condition, in respect to security from the small pox, and three different proposals have been suggested with this view. Each of them is occasionally practised—each of them becomes, in its turn, the topic of familiar conversation. They will, therefore, require separate consideration. A fourth proposal to improve the public protection originated with a member of our own profession, and will require some notice, in order to complete that sketch of *remedial measures* which it is my object, in this letter, to lay before your readers.

1. The first plan proposed is the resumption of small-pox inoculation. It is no less curious than instructive, to observe how differently this proposal has been met in the metropolis, and in the provinces. Here, small-pox inoculation is practised to an extent so trifling, as scarcely to merit notice or animadversion. In the western parts of London there are only three or four persons who have the character of being inoculators. Their practice, in that respect, is very limited; and, from all I can learn, directions with regard to seclusion are properly given by them, and faithfully executed. In one instance only, for many months past, has any patient in the Small-Pox Hospital traced his complaint to the practice of inoculation; and I am inclined to think that the public is in no material degree injured by the few cases of inoculation which an immense population, like that of London, must always be expected to afford. In the provinces, however, a very different notion prevails. Applications for small-pox virus, for the purposes of inoculation, have frequently been made to me by respectable practitioners; and in too many instances the spread of small-pox in a village has been distinctly traced to inoculation, practised by persons in a lower rank of life. It is impossible, I think, to separate this fact from the consideration of the greater facility of obtaining good and fresh vaccine lymph in the metropolis, to that which the country affords. Vaccination enjoys a higher reputation in London than in the country, because it is found, by experience, to be more effectual. It is more effectual, because the supplies of lymph are here more copious, more regular, more accessible, and, I believe,

more perfect. I shall, hereafter, have occasion to revert to this topic; but, in the meantime, may join in expressing my earnest hope that the practice of small-pox inoculation will be as much discouraged throughout the country, as it has long been in London; and that practitioners will never lose sight of the often-repeated fact, that inoculation, by keeping up the supply of contagion, destroyed two for every one that it preserved.

2. The favourite system in London, at the present moment, is revaccination. This is, by many, considered as the panacea for all vaccine imperfections; and the practice is rapidly extending. It was originally grounded on the theory of the limitation of vaccine influence. The period for which vaccination ensures the constitution has been differently stated—at seven, ten, and fourteen years. I have not been able to trace this idea to any medical author; but though it began with the public, it is not on that account to be discarded from medical reasonings. Dr. Jenner held, that "when once the human frame had felt the *full* influence of the genuine cow-pox, it was never afterwards assailed by the small-pox." Admitting the correctness of this, as a general doctrine, we may reasonably inquire what is the law, when, owing to some peculiarity, the human body receives only a *portion* of that influence which the cow-pox is capable of imparting? In other words, when the arm of a vaccinated child exhibits a small vesicle, with an imperfectly formed areola, the whole process being completed, and the scab falling off within fourteen days, leaving an indistinct cicatrix, what opinion is to be given? What is the law which regulates the subsequent susceptibility of small-pox, under such circumstances? These cases are far from being uncommon, and the answer requires deliberation. From the result of my own observations, I am induced always to state to the parent that such a process will give a *temporary* security to the child; that, for a certain number of years, such a child will not be susceptible of the small-pox; but that, at some future period, revaccination will be necessary, in order to complete that saturation of the system with the vaccine influence, which circumstances at present preclude it from receiving. It will thus be seen that I am disposed to accede to the doctrine of a limitation in the period of vaccine protection, under certain restrictions. I believe it to be an essential feature in the theory of vaccination, but I have no grounds whatever for believing that it applies to those far more numerous cases, in which the primary process is complete in all its stages;—that is to say, when every insertion takes effect; when the vesicles are large, pearly, and elevated; when a full areola is at its height on the tenth day; when the constitution, at that time, gives some evidences of internal derangement; when some of the scabs remain adherent to the twenty-first day; and, lastly, when the resulting cicatrix is *permanent* in after life.

It may be asked, on what grounds I aver

that the notion of a limitation to the period of vaccine protection is applicable only to the cases of *imperfect* vaccination? My reply is, that it corresponds with the phenomena daily passing before my eyes, and more especially with those of revaccination. It is only within the last twelve months that I have witnessed these upon an extensive scale, and I believe there are few practitioners hitherto well acquainted with them. This will, I hope, plead my excuse for submitting to the notice of your readers those few facts which have lately come to my knowledge regarding revaccination. I may previously mention, that a most unexpected and admirable opportunity lately occurred, at the Deaf and Dumb Institution, of prosecuting this investigation. The results have not hitherto reached me, but I have reason to hope that Dr. B. Babington will, ere long, communicate them to the profession.

I have noticed four different effects resulting from the operation of revaccination. In some cases, the skin appears completely insensible to the virus. The incision heals as though the unarmed lancet had alone been employed. More commonly, however, the poison irritates locally. In three (or at furthest four) days from insertion, an irregular areola appears, surrounding a minute acuminated and angry vesicle. Frequently the axillary glands swell; and in particular habits of body, especially in adult females, irritative fever, to a considerable extent, is superinduced. In a third set of cases, a pimple forms more gradually without any of this local or constitutional irritation. A slight degree of surrounding inflammation succeeds, and the vesicle contains, on the ninth day, a considerable quantity of a thin lymph; but this lymph will be found, on trial, *incapable* of propagating the disease. In a fourth set of cases, the revaccination runs a regular course. A true areola appears on the usual day, and the lymph will be found to propagate a good and genuine cow-pox. It may be called jumping at a conclusion, but I cannot forbear entertaining the idea, that those who exhibit the first set of appearances now described, would have effectually resisted small-pox; that those who were under the circumstances last mentioned, might have undergone the disease in some of its ordinary forms; and that the others might, under exposure to small-pox contagion, and in circumstances calculated to favour its development, (such as change of air,) have exhibited that class of symptoms called *varicella*, chicken-pox, mild or modified small-pox.

From the remarks now offered, it may be presumed that in my own practice I am induced to recommend revaccination in special cases only; that is to say, whenever any reasonable doubt exists as to the perfection of the primary process. But I as invariably discountenance it, when the proofs of a prior successful vaccination are clear, from the conviction that it is unnecessary, and from the feeling so strongly expressed by Mr. Edmonston, and so properly echoed by yourself, that the general adoption of such a principle,

merely to gratify curiosity, or to indulge the caprices of a parent, or to increase the chances of success, is wantonly, nay, cruelly, to disturb the public mind.

3. A third suggestion which has been thrown out with the view of insuring the safety of an individual from the chance of casual small-pox, is testing with variolous matter at some considerable distance of time from the original vaccination. This has been more talked of than practised. Those who have confidence in vaccination of course object to it; and those who may have imbibed doubts on the question of its unlimited efficacy, yet acknowledge its modifying influence, and feel that the inoculated disease cannot well be milder than the casual disorder, as it occurs after vaccination. The surgeon inoculated for small-pox merely because it was a disease of such extraordinary malignity. Remove that malignity, and the necessity for inoculation ceases. The cases, therefore, that warrant the practice of testing with variolous matter are very few indeed. It is perhaps applicable to the case of young men intended for the medical profession, who must necessarily be much exposed to small-pox contagion, and who may chance to take the disease at a very inconvenient moment. Under this impression, I have tested two or three gentlemen; but hitherto, in every instance, they have resisted the contagion. I am far from recommending this practice; yet it might be defended, with some show of reason, on the following ground. The most favourable age for receiving small-pox, (as far as my own observations extend) is from eight to fourteen. The comparative feebleness of the human constitution in infancy, offers less resistance to the inroads of this severe disorder; and, consequently, any measure calculated to throw the burden of the disease upon a period of life better able to oppose it, would benefit mankind, even though it enjoyed no pretensions whatever to the character of a *preventive*.

4. The last topic to which I shall advert, is the proposal of Dr. Ferguson to inoculate with the vaccine and variolous virus at the same time, or within such short periods that the two influences may coexist; the object being to produce artificially that mild small-pox which we now so often meet with casually, at long periods after vaccination. Such a proposal appears, at first sight, to be a philosophical application of the facts and principles now under discussion; but the difficulties which oppose its introduction into practice are insurmountable, as Mr. Edmonston, in the valuable Essay already referred to,\* has clearly shown. It proceeds upon the principle of keeping alive pure small-pox and pure cow-pox. Its application, therefore, could be only on a very limited scale; for, were the practice to become general, pure cow-pox would either become extinct, or continual recourse

\* Edmonston's Observations on Cow-Pox, page 113.

must be had to the cow. This involves a question which has frequently been agitated; but having already exceeded the just limits of a communication, I must defer it, with some other questions of a speculative nature, to a future opportunity.

I have the honour to be,

Sir,

Your very obedient servant,

GEORGE GREGORY.

From the Medico-Chirurgical Review.

## PECULIAR AFFECTIONS OF THE CRANIAL BONES.

Dr. Abercrombie has dedicated a section of his late work to certain affections of the bones of the cranium, to the investigation of which he was led by the following case.

A female, 48 years of age, fell down stairs, and received several contusions, one of which, on the head, confined her for some days. From this time, her health was bad—she had fixed pain in the head, and disordered stomach and bowels. Still she attended to her domestic concerns till within three weeks of her death, when she was seized with fever and high delirium. These subsided after venesection, and next day erysipelas appeared on the face, which also went off. She recruited a little, and was able to leave her bed, but complained of fixed and deep-seated pain in the right side of the head, a little above the ear, from which there was some discharge. Three days before death, (one year from the date of accident,) she became comatose, with partial paralysis of the left side.

*Dissection.*—The bones of the cranium were very soft. The inner surface of the skull-cap, when raised, exhibited a singular state of disease. The inner table seemed to be wanting through its whole extent, and there appeared the rough, irregular, and cancellated structure of the central part of the bone. Between this surface and the dura mater, there was a deposition of soft adventitious membrane of a yellowish colour, about the tenth of an inch in thickness. On raising the skull-cap, this membrane was found to adhere to the dura mater, leaving exposed the irregular cancellated structure of the bone. In other places it adhered to the bone, exposing the dura mater of its natural appearance. The greatest degree of erosion was in the parietal bones, where several portions were thin and transparent—in a few parts perforated. The external surface of the cranium was of a natural appearance. In the lower part of the right hemisphere of the brain there was an extensive abscess. On the petrous portion of the right temporal bone, the dura mater was of a dark colour, and detached from the bone, which last was sound.

There was no reason to suppose any syphilitic taint in this woman's constitution; the cranial affection must, therefore, have been the result of slow inflammatory action follow-

ing the blow, and gradually destroying the bone by caries. Dr. A. has not been able to find any case on record, precisely similar to the foregoing. Desault mentions a case, where death followed a blow on the head. The bone appeared sound externally, but the internal table was blackened throughout the whole extent of one of the parietal bones. The dura mater adhered to the parietal bone as firmly as to the other parts of the cranium, and there was suppuration on the surface of the brain. Some other cases, analogous to this last, are collected from surgical writers by Dr. A. who considers them as examples of an uncommon modification of disease of the cranial bones, confined principally to the inner table. The more common modification, however, is that which occurs in the outer table, or which affects the whole depth of the bone, with the history of which disease some remarkable phenomena are connected. It seems to be a low inflammation, which may arise from injuries extremely slight—or without any obvious cause. Its progress is very slow, and it may terminate by exfoliation of a part of the outer table, or it may affect the whole bone, and, extending to the brain, prove fatal. Mr. O'Halloran mentions the case of a man who was seized, without any injury, with pain in the upper part of the os frontis, which increased in violence, and unfitted him for his avocations. In the course of four months an abscess formed and burst—the bone was found carious and perforated by an opening, through which the dura mater was seen covered with pus. The piece of bone became loose, and separated in ten days. The patient recovered. Dr. Abercrombie does not think that the trephine promises much success in these cases. We think differently, and shall show cause in our next fasciculus.

The following case recently occurred at St. George's Hospital.

Caroline Hollyoake, æt. 20, was admitted Nov. 7th, with extensive disease of the right parietal bone. The outer table was completely exposed for a space about the diameter of a crown piece, but longer, and the edges of the scalp around were curiously tucked in. The bone was rough and blackened, and around it a line of separation had been commenced, but was ineffectual in checking the disease, for the probe passed beneath the scalp, and discovered dead bone for some distance around. The coronal suture crossed the exposed bone in front, and between its serrated edges, which were more apart than they should be, pus was seen undulating in correspondence with the pulsations of the brain. The menses had always been irregular, and for the last three years had ceased entirely. About a year previously she began to be affected with pain in the head, and hysteria, and she first noticed matter amongst the hair, nine months prior to admission. An abscess appears to have formed, and the part of the scalp to have been destroyed by ulceration. Besides this disease of the cranium, there was

likewise dead bone in the lower jaw. There was no reason whatever to suspect a syphilitic taint—she had never received any blow upon the part—nor could she account for the affection in any way. On the 13th, Mr. Rose removed a portion of the blackened outer table by an elevator, and on the 29th he divided the scalp, and removed a still larger portion by means of Hey's saw. Large flabby granulations were exposed beneath, which pulsated strongly; but whether they arose merely from the diploë, from the dura mater, or partly from one and partly from the other, it would be difficult to say. The patient was going on pretty well—the pulsation had distinctly lessened, and her health was certainly not suffering much, when she was persuaded by her stupid friends to leave the hospital, lest she should be made to undergo any more operations.

At this time the probe passed for some distance across the os frontis, and there was altogether a very large amount of diseased bone on the right side of the head. What was the cause of all this it would be difficult to say, though it must be owned that the girl was of a puny scrofulous habit of body. It is curious how little disturbance these affections cause to the sensorium. In the first case, the patient was scarcely aware of the existence of the tumour; and, in the second, there was no paralysis, no affection of the mind whatever. Mr. O'Halloran objects to the use of the trephine, but upon what grounds we know not. If there is a mass of diseased bone in the skull, particularly if the outer table only is affected, it surely must be an object to get rid, as quickly as may be, of this dead and irritating body. If, indeed, there be symptoms of irritation to the brain, as convulsions, paralysis, epilepsy, &c., then we conceive that the surgeon, *cæteris paribus*, has no choice in the matter, but is imperatively called upon to remove the affected bone.

From the Edinburgh Medical and Surgical Journal.

**CASE OF PERTUSSIS, with the Dissection of the Body, in which was found, besides the Morbid Appearances, a remarkable transposition of the Thoracic and Abdominal Viscera.** By WILLIAM MONCRIEFF, M.D. F.S.S.A. Fellow of the Royal College of Physicians, Edinburgh, &c. (Read to the Medico-Chirurgical Society of Edinburgh on the 5th March, 1828.)

E. C. a girl æt. 4½, became my patient on the 19th January last, affected with pertussis, under which disease she had laboured from the commencement of November, 1827. The fits of coughing were very frequent and severe; expectoration partly purulent, and reported to have been tinged with blood for about a month, previous to her coming under my care. Breathing quick and oppressed, apparently easier when lying on the left side; skin warm; tongue white but moist; abdomen tumid; bowels reported to be in general

costive; faces slimy and fetid, usually of a dark colour, sometimes greenish. Pulse about 120; complexion pale; and she was considerably emaciated. She was nursed by Mrs. —, with whom she had lived from the evening of the day of her birth. Mrs. — informed me that she was a fat flabby child till she was weaned, when about nine months old, and continued in tolerably good health till about two years ago, when she was affected with severe cough and difficult breathing. The medical gentlemen who attended her said that the disease was inflammation of the lungs. She was ill for three months, during which time leeches and blisters were applied repeatedly to her chest, &c. After she recovered from the above attack, she continued apparently in good health till she was seized with pertussis in the beginning of November last, as previously mentioned.

The usual treatment in cases of whooping-cough was adopted. The fits of coughing became less frequent, but the breathing continued much oppressed, and the expectoration purulent, but not mixed with blood. She became gradually weaker. The pulse rose to 130 and 140 in the minute; and for the few last days of her life her feet were œdematous; her urine in small quantity, with great thirst; her breathing was more and more oppressed, with considerable restlessness; and she died quite exhausted on Wednesday, the 6th February, at twelve noon.\*

The examination of the body took place on the following day, at 1 P. M. and was conducted by my friend Mr. Moodie.

*Thorax.*—On opening the thorax, there were strong adhesions found between the pleura pulmonalis and costalis on the right side; none on the left side. The left lung had *three lobes*, the right *two*. The lungs on both sides were very much diseased, but principally on the right side, which contained a great many small tubercles, some with purulent matter in the centre, and others assuming the appearance of small abscesses. The upper part of the left lung was in the same state as the right; the lower half more healthy. A considerable quantity of purulent matter exuded from the branches of the bronchiæ when cut. The bronchial glands were very much enlarged; and some of them consisted entirely of scrofulous cheesy matter. The pericardium was very much distended with serum. The heart was on the left side, but apparently nearer the centre of the body than usual. It was removed rather speedily, along with part of the blood-vessels attached, to be examined more minutely afterwards, so that the relative situation of the parts cannot be precisely stated. However, the part of the thoracic aorta, which was left in its situation, lay on the *right* side of the spine, but gradually reached the mesial line as it descended. On examination of

\* It may not be altogether irrelevant to notice, that the father and mother of this child, previous to their union, were reported to have been in a very close degree of relationship.

the heart afterwards, nothing preternatural could be detected in its structure.

*Abdomen.*—The œsophagus passed through the diaphragm on the *right* side. The cardiac extremity of the stomach and spleen were also on the *right* side. The great lobe of the liver was on the *left* side; the smaller lobe towards the *right*. The commencement of the duodenum and the pylorus were on the *left* side, as likewise the cœcum, appendix vermiformis, &c. The sigmoid flexure of the colon occupied the usual place of the cœcum and ascending colon. It terminated as usual in the rectum, which made its way through the pelvis in the usual manner, excepting that it commenced on the *right* side.

The viscera of the abdomen appeared otherwise healthy. The kidneys, urinary bladder, and uterus, with its appendages, were in their natural situation, and without any morbid appearance.

*The Brain.*—The veins on the surface of the brain were very turgid; the substance of natural appearance. The ventricles contained a considerable quantity of limpid serum.

My friend Mr. Moodie, who is one of Dr. Knox's pupils, communicated to that gentleman a report of the dissection; and Dr. Knox being desirous to have an opportunity of inspecting the body, requested me to obtain permission to have it re-opened, which was very readily granted; and accordingly Dr. Knox along with some of his pupils, Dr. James Simpson, and myself, were present at the re-opening of the body the following day, the 8th February, at twelve noon. Dr. Knox wrote a short account of the appearances which he observed, and requested my permission to send it to the editor of the London Medical Gazette for insertion in that periodical publication, which I with pleasure acceded to. The account of the abdominal viscera which I have read to the Society is almost verbatim in the words of Dr. Knox, whose accuracy and indefatigable zeal in anatomical pursuits are well known to the members of this Society; and it affords me great satisfaction, that the deviation from the natural situation of the viscera which occurred in this case, was witnessed by so able an anatomist.

The following is a list of some of the works in which similar cases of transposition of the viscera are to be found, viz:

Bonetus, Sepulchret. sive Anat. Pract. Tom. iii. lib. iv. sect. xi. observ. 7th, p. 549. ("Cadaver sicarii cujusdam, dicti Richardi Francœur, qui supplicio rotæ fuit addictus anno 1630, dissectum fuit," &c. A complete case of transposition of the viscera.

Schenkus, Observ. Med. Rar. lib. iii. obs. ix. p. 389. ("Hepar in sinistris, et lien in dextris."

Bartholinus, Hist. Anat. Rar. Tom. i. cent. ii. hist. xxix. p. 219. ("Lien in dextro, je-cur in sinistro hypochond." &c. Dissection at Paris in 1650, by Mag. Pet. Reguier.

London Philosophical Transact. No. cvii. 1674. (Account by Dr. Sampson of the dissection of the body of J. D. a minister in

Yorkshire, whose bowels were completely transposed.)

Hist. de l'Acad. Roy. des Sciences, 1688. ("M. Mery a fait rapport à l'Academie d'une dissection faite par M. Morand à l'Hotel Royal des Invalides du corps d'un Soldat mort à l'âge de 72 ans. Il y trouva un déplacement général de toutes les parties contenues dans la poitrine et dans le ventre tant des viscères que des vaisseaux," &c.

Haller, Element. Physiologiæ, Tom. vi. pp. 118, 391, 460. (General observations on transposition of viscera.)

London Philosophical Transact. Vol. lxxviii. for 1788, part 2d, p. 350. (Account by Dr. Matthew Baillie, of a remarkable transposition of the viscera, observed in the dissection of the body of a man near 40 years of age.)

Dr. Duncan's Med. Comment. Vol. xiii. 1778, p. 428. (Short abstract of the above dissection.)

London Medical Journ. 1789, p. 178. (Account by Dr. M. Baillie of the same dissection, with some alterations and additions.)

London Medical and Physical Journ. Vol. xxxvii. No. 218, for April 1817, p. 346. (General transposition of the viscera, observed by M. Beclard, in a woman about 50 years of age, who died of a pulmonary affection.)

Edin. Med. and Surg. Journ. Vol. xix. for 1823, p. 652. ("Cases of three soldiers, reported by M. Scoutetten in *Journ. Univers.* 1823, who had passed their twentieth year, of good constitutions, and who had enjoyed excellent health, till they were cut off suddenly by gastro-enteritic inflammation. The anatomical details present nothing remarkable, except the extreme precision with which the viscera of the opposite sides occupied the place of one another.")

Nouvelle Bibliotheque Medicale pour Decembre 1827, p. 478. ("M. Serres a fait voir le 24 Novembre dernier, aux élèves qui diséquaient à la Pitié, un cas de transposition des viscères," &c.) This is also a complete case of transposition. M. Serres intends to inject the subject, and to preserve it.\*

Several other works are mentioned by Ploucquet and Voigtel, in which transposition of the viscera was found more or less complete. Vide Ploucquet, *Literatura Medica Digesta*, under *Viscus dislocatio*, *Intestinum dislocatio*, *Hepar dislocatio*, *Splen dislocatio*, &c. and *Handbuch der Pathologischen Anatomie* von Dr. F. G. Voigtel. Halle, 1804. Zweiter Band, p. 314.

From the London Medical Gazette.

## ON THE TIC DOULOUREUX.

By SIR HENRY HALFORD, Bart.

Sir Henry Halford having politely given us permission to lay before the readers of the Ga-

\* Vide Journal of Foreign Medicine Vol. i. p. 379.

zette an account of his interesting observations on this subject, we subjoin a pretty full analysis of the paper read at the College of Physicians on Monday last, in which, strong evidence will be found in support of the position which it was the object of the learned President to establish—namely, that tic douloureux is frequently connected with, and dependent upon, an affection of the bone.

He observed, that the severest form of the disease was that which occurred in the 5th pair of nerves, and that it might be distinguished by its intensity from the milder species affecting the nerves of the extremities, or other parts, and which often depends upon impaired digestion. The latter, for the most part, yields to general remedies; the former seldom does so. The fact of the division of the nerve, and cutting off its communication with the brain, so frequently failing to cure the disease, was mentioned as sufficiently proving that the seat of the pain is not always the seat of the disease; and the unsatisfactory nature of all the pathological explanations hitherto proposed, was briefly alluded to.

“May I venture (said Sir Henry) to throw out an opinion, founded on the observations with which my experience has furnished me, that the disease is connected with some preternatural growth of bone, or a deposition of bone in a part of the animal economy where it is not usually found, in a sound and healthy condition of it, or with a diseased bone?”

“The following cases have occurred to me, and seem to give a degree of probability to this surmise; and I throw it out for the consideration of the profession, in order that a number of facts may be collected, from which a safe inference at length can be drawn.

“A lady, 40 years of age, suffered under the violent form of tic douloureux, at Brighton, notwithstanding the careful attention and skill of a very judicious physician there. On returning to town, it was observed that the rending spasms, by which the disease is marked, were frequently preceded by an uneasiness in one particular tooth, which exhibited, however, no signs of unsoundness; but the constancy of this symptom was enough to justify the extraction of the tooth in this instance, (though the failure of this expedient to afford relief in general does not encourage recourse to the operation,) and on its being drawn, a large exostosis was observed at the root of the tooth, and the lady never suffered more than very slight attacks, and those very seldom, afterwards.

“The D. of G. was attended by Dr. Baillie and myself, for six weeks, under this disease, in its most marked and painful form, without deriving benefit from our prescriptions. At length we thought it best to advise him to repair to the sea-coast, in hopes of renovating his shattered system by taking bark there. After he had sojourned a month by the sea-side, a portion of bone exfoliated from the antrum highmorianum, and the D. recovered immediately, and has never suffered the disease since. The bone had been

hurt, probably, by a fall from his horse, which the D. had met with some months before.

“The late Earl of C. underwent martyrdom by this disease, and excited the warmest sympathy of his friends by the agonies he sustained for many years. He submitted to the operation for the division of several branches of the 5th pair of nerves repeatedly, by Sir Everard Home and Mr. Charles Bell, without obtaining more than mere temporary relief. At length he was seized by apoplexy, and lay insensible for some days, and in great peril from the attack, but finally recovered. After the apoplexy, the paroxysms of the tic douloureux became less frequent and less severe, and were administered to satisfactorily by an ingenious physician, who wrote his inaugural exercise on the disease. For the last year or two of his life, his lordship had ceased to suffer from the tic, and died at an advanced age, without any marked malady. His head was not examined after death, and therefore we are left to conjecture only what might have been the immediate cause of his former sufferings. Whilst I attended him, he underwent repeated exfoliations of the alveolar processes of the teeth, which I thought occasioned his torment; and to account for the cessation of the complaint, I supposed that these efforts to throw off diseased portions of bone might have ceased, or that the apoplexy had disqualified the nerves for suffering so exquisitely; but there might have been besides, as some later instances have made probable, disease in the bones of the head.

The late Dr. P. fell a sacrifice to this dreadful disease, after sustaining its tortures for some years, with a constancy which attracted all our pity and esteem, and died at last under apoplexy. No assistance which the experience of any of us could afford him, gave him relief, or controlled the violence of his attacks. On examining his head after death, there was found an unusual thickness of the os frontis, where it had been sawn through above the frontal sinuses, and at its juncture with the parietal bones. There was discovered also on the falciform process of the dura mater, at a little distance from the crista galli, a small osseous substance about  $\frac{3}{8}$  of an inch in length, rather less in breadth, and about a line in thickness. The vessels of the pia mater were turgid with blood, and about an ounce of fluid occupied the ventricles. I lamented that the frontal sinuses had not been examined, for I remember he replied to a question which I once put to him, as to his ever having experienced any suppuration within any bony cavity, that he had twice suffered suppuration in the frontal sinuses. Dr. P. had submitted, with great patience, to a division of several branches of the 5th pair of nerves, under the judicious operation of Sir A. Cooper, who on my mentioning to him the notion I entertained of the cause of tic douloureux, was so obliging as to show me the skull of a person who had died of this disease in the country. The internal surface of the frontal bone is a perfect rock work.”

All the preceding cases had fallen under

Sir Henry's own observation, and he added one communicated to him by a physician of high character, in which a lady suffered from this complaint for nearly ten years, and at length died of apoplexy. An enormous thickening of the frontal, ethmoidal, and spincinal bones was found; there was also general thickness of the cranium, but not to so great an extent.

In these instances, there certainly is every reason to believe that the preternatural condition of the bones had proved the exciting cause of the disease: but, as the learned President observed, there are other cases in which no such immediate cause of irritation can be discovered, so that it is probable that the nerves, in these, are affected by sympathy with various parts. Several examples were mentioned in illustration; among others, that of a boy, 11 years of age, in whom a dose of rhubarb was followed, three different times, by an epileptic fit; and that of a lady, in whom the same medicine produced severe strangury, which she stated to be the constant effect of that medicine on several of the members of her family. Sir Henry also alluded to the disturbance sometimes produced by an issue, mentioning that Dr. Darwin was once called to a young lady labouring under epilepsy; finding that she had an issue on the arm, "without one word of remark, he filiped the pea from its place, and the young lady never experienced an epileptic attack afterwards."

From the Edinburgh Medical and Surgical Journal.

*A Corn. Celsi Medicinæ libri octo ex recensione Leonardi Targæ. Quibus accedunt, &c. &c. Concinnavit EDVARDUS MILLIGAN, M.D. S. A. S. S. &c. &c. &c. Edinburgh, 1826.*

It is rare, indeed, that we can venture to take notice of new editions of works which have been already long before the public. The great number of new works on various subjects, which necessarily claim more or less attention, renders this a matter of no easy performance.

Various circumstances, however, induce us to deviate from our usual custom in regard to the work, the title of which is prefixed to this notice. In the first place, we understand it to be generally admitted, that the work of the Roman physician abounds with so much excellent matter, shows so much accuracy of observation, and exhibits such a fine application of the general principles of sound reasoning and good common sense to medicine, all expressed in the most elegant and select language, that antiquated as some pretend to rate it, it is a work with which physicians of reflection and taste have at all times been proud to avow their acquaintance.

The necessity of presenting the text of an author so distinguished in as pure a state as possible is obvious. This is not the place to consider the merits of the different editions of Celsus. It is sufficient to say, that to the one which was published in 1769, by Leonard Targa, a learned physician of Padua, after an

elaborate collation of the most creditable manuscripts, is generally assigned the palm of superior accuracy. Whoever reads the Epistles of Morgagni on this subject is aware how corrupt the text was previous to the time of this able scholar, and must know what a benefit was conferred on the literary world by the classical labours of Targa.\*

The edition of this editor, which has thus been regarded as the model of perfection, has been repeatedly reprinted. As occasionally happens, however, with such reprints, every successive edition was less correct than the one which preceded it; and, had the system been continued, it is truly difficult to say to what extent this species of corruption might have proceeded.

With the laudable intention of obviating these evils, Dr. Milligan of this city, a gentleman distinguished not only for his professional attainments, but for much accurate classical knowledge, has presented to the lover of medical literature the present edition of Celsus, according to the most correct text of Targa. However humble this labour may be regarded by many who neither know nor value the attainments by which only it can be accomplished, it would be difficult to mention a more disinterested example of the application of medical knowledge and classical learning, or one for which the student ought to be more grateful. The text is throughout correct to an extraordinary degree. We speak from actual trial of a large proportion of the volume, when we say that we have not been able to detect a single error either in typography or punctuation.

Besides presenting the text in this highly accurate form, Dr. Milligan has improved the present edition by many useful services. Thus he has given the synonyms of the diseases described by Celsus in the nomenclature of Sauvages and Cullen. He has attempted to ascertain the synonyms of various substances in the ancient *Materia Medica*, and given them in the nomenclature of Linnæus, Thomson, Werner, and the Edinburgh Pharmacopœia. And he has added throughout marginal notes on the subjects treated by the author,—a method of much use to the student in confining his attention to the subject. Dr. Milligan has also given tables of the weights and measures of the ancients according to Pliny and Celsus; a synoptical view of the *materia medica*; and an explanation in figures of the ancient baths, and of certain instruments used in the treatment of diseases.

To conclude, this edition of Celsus is the best which has been published since the original one of Targa; and it may be justly recommended to the attention of the student.

\* Jo. Baptistæ Morgagni in Patavino Gymnasio Primarii Anatomæ Professoris in Aur. Corn. Celsum et Q. Ser. Samonicum Epistolæ; in Quibus de Utriusque auctoris variis editionibus, libris quoque Manuscriptis et Commentatoribus, disseritur. Hagæ Comitum, 1724.

## Medical and Philosophical Intelligence.

*Communication between the Vessels of the Uterus and Placenta.*—M. Biancini has instituted the following series of experiments, to prove the existence of a direct and immediate circulation between the mother and fœtus. He injected the vascular system of a woman who died in childbed; the uterus had not contracted, and the placenta was still adherent. The matter of the injection was found in the vessels of the chorion and amnios, and having examined the flexuous arteries of the uterus, it was observed that they penetrated the tissue of the placenta, ramified on the membrane, and deposited the injected fluid in the cells described by Hunter and Meckel. In a young woman who died eight days after parturition, a portion of the placenta still adhering to the uterus; an injection into the arteries of the latter not only penetrated into the adherent fragment, but was extravasated also into the cavity of the uterus and the vagina, by the lacerated extremities of vessels, called by the author, *utero-placental arteries*. On dissection, the vessels of the uterus and placenta were found filled with the injection. In a woman who died of uterine hæmorrhage subsequently to delivery, an injection through the aorta, above its bifurcation, demonstrated the continuity of the flexuous arteries with those of the uterus, the open extremities of the latter permitting the injection to transude on the internal surface of that organ. It is through these vessels, according to M. Biancini, that the injection passed in the first experiments, from the uterus to the placenta. A cat was killed thirty days after conception, and an injection of size coloured with vermilion, thrown into the vessels; it was found to have penetrated through the substance of the placenta into the arteries, ramifying upon its fœtal surface, which, filled with the injection, were seen converging towards the insertion of the umbilical vein, beyond which point it could not be traced. A similar result was obtained when gravid rabbits were the subjects of the experiment. And in another cat, the injection was found not only in the umbilical vein of six of its eight fœtus, but in the two others, it had also penetrated into the vena cava superior and its most minute ramifications. Having thus demonstrated the direct passage of the blood from the mother to the embryo, M. Biancini continued his researches, to determine whether the communication was equally direct in the reverse direction. Having, with this view, injected mercury into the umbilical veins of a calf, he observed that the metal after permeating in flexuous lines, the placenta and the fœtal coverings, passed immediately by nine short cylindrical branches, to which he has given the appellation of *placento-uterine veins*, into the veins of the uterus. The same thing took place in a rabbit, and in another animal of this species, the injection not only passed from the umbilical veins of the fœtus into the uterus, but was

found also in the iliac and hypogastric veins, and the ramifications of the vena cava inferior of the mother.—*Jour. des Progrès, &c.*

The *Endermic Method*, consists in applying active medicines to the surface of the cutis deprived of its cuticle, by the action of vesicating substances; boiling water should be excluded from their number, as too uncertain and dangerous in its operation. The pain occasioned by vesicatories, may be in great measure obviated, by surrounding them with emollient cataplasms; the vesicle may be punctured, to evacuate the serum, and the remedy to be used, introduced beneath the detached cuticle, in which case the absorption is more active. During the dressings, it is necessary carefully to remove the flocculi, which form upon the suppurating surface; and for this purpose, a solution of the chloruret of lime may be advantageously employed. The medicines used, should be reduced, if possible, to an impalpable powder; and if their local action be too irritating, they may be incorporated in gelatine or axunge; the dose being increased in proportion to the vehicle. Liquids should be poured guttatim and very gradually upon the denuded surface, and resinous substances employed in the form of plasters. In the administration of the medicines, an increasing, stationary, and decreasing course should be pursued, and their action will be found more energetic, in proportion to the proximity of the vesicated surfaces to the nervous centres. More advantage will be derived from applying the remedy to a great number of small vesications, than upon one continuous one, which should equal them all in extent. Any unpleasant consequences which may result, may be presently obviated by the immediate detersion of the blistered surface, by compression, by the application of a cupping glass, or lastly, by a substance possessing opposite properties; thus tetanus occasioned by two grains of strychnine, has yielded to the substitution upon the same surface, of the same quantity of the acetate of morphine. Two well marked effects speedily follow the application of remedies in this manner, one of which is local, and the other, the consequence of its absorption. The first, consists most commonly in a pruritus, or a sense of heat, accompanied by redness, and injection of the vessels. The second varies with the substance employed, and manifests itself from ten minutes, to two or three hours afterwards. It makes its appearance, in general, by a sensation of heat, which diffuses itself from the denuded surface towards the nearest splanchnic cavity, and thence following the course of the principal vessels and nerves, is propagated throughout the system. Absorption appears more active upon the internal than external part of the

extremities, on the anterior of the trunk, than posteriorly, and in persons of soft and delicate skin, than in those in whom that membrane is dry, brown, dense, scaly, and covered with hair. It appears to be more vigorous in the evening or night, after a bath, and when the digestive organs are at rest; in moist than in dry weather, and in warm than in cold seasons.—*Essai sur la Methode Endermique. Journal des Progrès, &c.*

*Chronic Bronchitis cured by the External Application of the Acetate of Morphine.*—Georgette Richemont, aged 55 years, of a lymphatic-nervous temperament, and very delicate constitution, had been repeatedly attacked with bronchitis upon the approach of winter, which eventually assumed a chronic form, and became so much exasperated towards its usual periods of accession, that she was obliged to enter into an hospital. The usual pectoral remedies had been tried with only slight melioration. Fourteen years had now elapsed since the commencement of the disease, and its progressive advancement had occasioned a great alteration in the external habitude of the patient; the cough was incessant, with exacerbations during the night, and thick, flocculent and grayish expectoration; continued pruritus in the course of the trachea; oppression, sleeplessness, chills and sweats, occurring every evening. It was ascertained by means of percussion and auscultation, that the air permeated freely every part of the lungs. Expectorant remedies were employed, and a blister directed to the breast; no benefit resulting from these means, and the patient having been constipated for the last eight days, five grains of aloes were sprinkled upon the vesicatory, which, ten hours afterwards, produced several stools, with some degree of griping. Encouraged by this circumstance, it was resolved to attempt the introduction of some sedative medicine through the same channel, and half a grain of the acetate of morphine, reduced to powder, was applied to the blistered surface on the 16th March. It occasioned a pricking sensation, which soon subsided; in a quarter of an hour afterwards, the tracheal pruritus had moderated, and the cough and oppression were lessened. The quantity was raised to a grain the next day, with the effect of inducing sleep, to which the patient had long been a stranger. The dose was gradually increased to two grains by the 22d, and the improvement had been so rapid, that the cough had almost entirely disappeared. Desirous to ascertain whether this change was due to the medicine alone, it was not applied on the following day, and the consequence was a recurrence of all the symptoms with increased severity. Recourse was again had to the acetate on the 24th, in quantity of three grains, and so great was the melioration, that by the 5th of April she had already recovered, in some measure, her *embonpoint*, and the cough and expectoration had entirely left her. The medicine was seve-

ral times discontinued, and always with the same result as in the instance just mentioned; it was ascertained that the imagination of the patient was in nowise connected with this alternation. The quantity was ultimately increased to four grains, the improvement was progressive, and toward the end of May, she was discharged entirely cured.—*Journal des Progrès, &c.*

*Traumatic Tetanus cured by the external application of the Acetate of Morphine.*—Marie Ursin, aged 21 years, of a scrofulous constitution, was admitted into la Salpêtrière, for an ulcer, situated on the malleolus externus, which had exposed the tendon of the peroneus longus. On the 10th of June, 1824, the dresser pulling at the exposed tendon, gave rise to acute pain, which continued about ten minutes, accompanied by vomiting and extreme *malaise*. An hour afterwards, there was a pricking sensation in the leg, and the patient fell to the ground without consciousness. The jaws were firmly closed, the muscles of the neck were in a state of great rigidity, abdomen hard and tense, the legs convulsively flexed, eyes fixed, pulse hard and accelerated, &c. She was immediately bled copiously, and thirty leeches directed to the anus; two hours later, the same state continuing, she was immersed in a warm bath for a long time, with as little success. Recourse was then had with similar result, to mercurial frictions, a blister to the neck, &c. It was now determined to make trial of the endermic method of M. Lembert; the fourth of a grain of the acetate of morphine, was mixed with a very small quantity of cerate, and applied to the blistered surface at 10 A. M. of the second day of the disease. The trismus ceased entirely, but the rigidity of the neck continuing, the quantity of the acetate was doubled at 8 P. M. Three hours later, every symptom had disappeared; the night was passed calmly, and the next day only a slight degree of lassitude remained.

A case of spontaneous tetanus is also detailed, in which the above remedy was employed with similar success. The patient, a woman of a nervous temperament and delicate constitution, had been five years in the apartment of incurables of la Salpêtrière, affected with darts situated on the internal surface of the superior and inferior extremities. On the 23d July, she was so much affected by the spectacle of a person who fell suddenly by her side, in an epileptic paroxysm, that she fainted; and a short time after, while narrating the circumstance, she was suddenly interrupted by convulsions. Her face was retracted, jaws closed and immoveable, forearms strongly flexed, the neck drawn firmly backwards, and the whole body in a state of general rigidity. A small blister was applied to the back of the neck about 11 A. M. and being removed after a lapse of four hours, it was sprinkled with a quarter of a grain of the acetate. By six

o'clock, the trismus had disappeared, but the other symptoms still continued; the same quantity of the acetate was again applied; and at ten, the contraction of the muscles of the forearm ceased; the muscles of the neck, face and eyes resumed their natural condition somewhat later, and on the 25th the patient was able to resume her ordinary avocations.—*Journal des Progrés, &c.*

*Muriate of Ammonia successfully employed in Stricture of Œsophagus.*—The patient, aged thirty-seven years, began to experience, about the 1st December, 1825, some difficulty in deglutition. He swallowed his food in the first instance sufficiently easy, but after a short interval (from ten to twenty seconds) it appeared to rise from the stomach into the pharynx, and could not be swallowed a second time, without much pain and difficulty, which continually augmented. This species of rumination was not observed in relation to liquids, but a disagreeable sensation of heat towards the lower part of the œsophagus, accompanied their deglutition. The patient was pale and melancholy, and his countenance indicative of much suffering. After an attentive examination, Dr. Pagenstecher discovered that the aliment did not reach the stomach in the first deglutition, as was supposed by the patient, but that it remained some time in the œsophagus near the cardia, occasioning acute pain in that part. When the bolus was small and well masticated, these symptoms were observed in a much less degree. A slight sense of tension only, was felt when no attempt at deglutition was made. The patient stated that the pain appeared to rise from an ulcer at the part, over which, the passage of the aliment, occasioned a pricking and tearing sensation. The disease was supposed to be inflammation of the lower portion of the œsophagus, accompanied with stricture; and there was reason to apprehend a connexion with some affection of the lungs, with which the patient had been formerly afflicted—a dry cough still remaining, and at each return, occasioning much pain. Blood was detracted by means of twelve cups applied to the epigastrium, demulcents, a liquid diet, &c. No amendment being produced by these means, and the disease continuing to increase, it was resolved to make trial of the muriate of ammonia, so highly extolled in these cases by M. Fischer of Dresden, and the patient was directed to take every two hours, half a spoonful of a mixture, consisting of two drachms of the salt, and two ounces each, of the sirup of elder and mallows. The next day he expressed himself considerably better; the medicine had occasioned a slight degree of burning at the diseased part, but its sensibility was already lessened, and fluids could be swallowed without difficulty. From this period he became rapidly better; on the 14th he could take a small quantity of solid aliment without pain. The cough, which was probably sympathetic, entirely left him, and in ten days the cure was

complete, but the medicine was directed to be continued some time longer.

The same physician relates a similar case, occurring in a woman 46 years of age, of feeble constitution, long subject to cough and cephalaria, to which symptoms were ultimately added, indigestion, pains in the stomach and extremities, constipation, &c. A short time after taking a glass of some stimulating fluid, she experienced pain in the vicinity of the cardia, with difficulty of deglutition, &c. as in the preceding case. She was successively treated by two physicians, but finding no relief from their prescriptions, and the dysphagia continually augmenting, Dr. P. was sent for. At this period, the patient was suffering greatly; the tongue was covered with a yellowish coat, constipation, flatulence, a sense of oppression in the epigastrium, and a continual and pungent pain above the cardia, and along the vertebral column. Her nourishment consisted in a small quantity of coffee; she had already adopted the idea that her complaint was incurable, and could with difficulty be induced to take the sal ammoniac. Its effects were equally prompt as in the case just related. At the expiration of three days the burning sensation, caused by the medicine in its passage over the inflamed part, ceased altogether, and after the lapse of a week she could swallow both solids and liquids without difficulty.—*Jour. des Progrés, &c. from Hufeland's Journal.*

*Fracture of the Femur, and extensive deposition of osseous matter, cured by the Seton;* by Professor Weinhold.—The patient, aged 18 years, had fractured the thigh about its middle, and was treated in the ordinary manner by a neighbouring surgeon, till the end of the fourth week, when yielding to the pressing solicitations of his patient, he permitted him to resume his labours in the country. After the expiration of ten weeks of continual exercise, the limb was observed to be shortened two inches, and the callus had so much augmented, as to equal in volume the head of a new born infant. At this time, the patient, unable to continue his ordinary avocations, was obliged to relinquish his employment. When seen by Dr. Weinhold, the callus was 18½ inches in circumference; there was great extravasation of lymph in the cellular tissue above and below the tumour, and in several places, portions less indurated than others, which eventually suppurated and formed fistulæ. The disease was deemed beyond the resources of art, but the limb, notwithstanding, was placed in a suitable apparatus, and extension by means of pulleys, was continued during eight days, without producing any perceptible effect upon the callus, which after a lapse of three months, had acquired too much solidity to yield to any mechanical means. In this state of things, Dr. Weinhold determined to introduce through the callus, a seton overspread with some stimulating substance, with a view to excite inflammation and suppuration of the

bone, bring about the softening and absorption of the callus, and restore the limb to its original length by means of extension, gradually and regularly applied. The operation was accordingly performed about an inch on the outside of the femoral artery; the external shell of bone having been perforated, the seton was passed through a cavity, four inches in diameter, and brought out on the opposite side. During the three first days, cold applications were made to the part; subsequently the seton was spread with some stimulating substance, and drawn through the wound twice daily. An abundant suppuration took place in the indurated cellular membrane, and the matter found an exit by two fistulæ, situated one above and the other below the tumour; these were healed by means of compression, and shortly after, the induration above mentioned had disappeared. Towards the sixth week, the callus had become very painful, and the temperature of the part was much augmented;—these symptoms yielded to cold applications, continued for the space of forty-eight hours. The suppuration of the callus so long desired, was established in the seventh week, and by pressure with the finger, a large opening could be felt, indicating that the proper time for making extension had arrived; it was applied therefore, and with such beneficial results, that at the expiration of the tenth week, the difference between the limbs was not more than two lines. The seton was continued two weeks longer, after which the wound was permitted to cicatrize. Some time afterwards, the tumour had considerably diminished, the patient could walk without crutches, and the affected limb had nearly acquired the size and appearance of that on the opposite side.

A case of spina ventosa, situated about the middle of the femur, and for which amputation had been proposed, was successfully treated in the same manner. Dr. Weinhold supposes the inefficacy of the ordinary seton, in the cure of artificial joint, arises from its exciting too slight a degree of inflammation, and permitting the access of air to the extremities of the bones, which are thereby extremely disposed to become carious; to obviate which, he adopts the practice of making a funnel-shaped wound, and introducing a cuneiform seton. Two cases, cured in this manner, are related, in one of which the tibia, and in the other, the femur was the seat of the disease; the latter had existed ten years, and was complicated with caries and fistulæ. In another instance, in which the neck of the femur had been fractured, and the acetabulum injured, the seton was introduced at the urgent solicitation of the patient, three months after the accident, the existence of an artificial having been ascertained. Suppuration, caries and hectic fever followed, and proved fatal at the expiration of six weeks.—*Bibliothek der Practischen Heilkunde.*

*Deafness cured by Electricity.*—A young girl, aged eight years, who had lost the faculty

of speech and hearing four years previously, was subjected some twenty times during a period of four months, to the action of an electrical machine. The fluid was employed in a variety of ways, sometimes through the medium of electrical baths, and on other occasions by means of sparks and shocks directed as nearly as possible to the affected organs. At the expiration of the period above mentioned, a great improvement was perceptible; the patient, who previously could not hear the report of a pistol, discharged by her side, is now able to understand conversation carried on in a moderate tone. At the time when the preceding account was published, she had not recovered the use of her speech.—*Rust's Magazine.*

*Stomach terminating in a Cul de Sac.*—The child was born in the eighth month of uterogestation, and appeared externally, to be well formed. Six hours after birth it vomited a brownish matter, not unlike half digested blood or meconium. In a little while hæmatemesis made its appearance, and returned periodically until its death, which took place at the expiration of sixty-five hours. The child during the act of vomiting, gave no indication of suffering. After death, a large quantity of blood flowed from the mouth and nose, and the whole body assumed a deep livid colour. On dissection, the stomach was found much distended, occupying nearly the whole of the left side of the abdomen; the liver descended very low, and between these two organs, the intestines were seen, much attenuated, and agglomerated so as to resemble a bundle of worms. The internal surface of the stomach was spongy and inflamed, blackish in some places, and presented globules of air, which appeared to have been the result of gangrene. The pylorus communicated with a pouch or second stomach, which had no other opening into it than that of the pancreatic duct, which was situated in its fundus, so that no communication existed between the stomach and intestines.—*Journal des Progrés, &c.*

*Polysarcia.*—October 17, 1825, Professor Graefe was requested to visit a butcher, thirty seven years of age, who in consequence of an excessive deposition of adipose matter throughout his entire system, was attacked with dyspnoea and impending suffocation. These symptoms were obviated by venesection, repeated twice, and even three times a day, catharsis to the extent of from sixteen to thirty stools in the 24 hours, and the exhibition of digitalis to increase the urinary secretion; the patient, who for a long time had lived exclusively upon flesh, was limited to a strictly vegetable diet, with lemonade or pure water for drink.

The alvine evacuations resembled a solution of soap in water, and exhaled, as did also the

body of the patient, a greasy and nauseating odour. Some time before his illness, he weighed 363 pounds, which fifteen days of the above treatment reduced to 313, at the period last mentioned, nausea and colic pains having been occasioned by the purgatives, Prof. Graefe had recourse to the tincture of iodine, which was given in the dose of twenty drops, four times a day, and continued several months with the greatest success. The dyspnœa had already much diminished when he commenced the use of iodine, and disappeared altogether under the influence of this medicine. A considerable reduction was also effected in the weight of the body; on the 14th Nov. it amounted to 316 pounds; 293 on the 14th of the month following; on the 20th Jan. 284; February 16th, 267; and finally in June, it did not exceed 209—a diminution of 154 pounds in the space of nine months.

This excessive secretion of adipose matter commenced about four or five years preceding his last illness, and appeared to arise from a morbid activity of the digestive organs; the patient being tormented with an almost insatiable hunger, which he endeavoured to relieve by eating enormous quantities of meat to the extent, for a considerable time, of sixteen pounds at a sitting.—*Archives Generales de Médecine.*

*Vaccination.*—In a paper read before the College of Physicians, Dr. Macmichael stated on the authority of a letter recently received from India that, “from vaccine matter having lately failed in Egypt in a great many cases, medical gentlemen were led to institute many experiments, by which it has been ascertained, that by inoculating a cow with small pox taken from the human body, fine, active vaccine matter is produced. At the time the letter was written, there was a Greek child at Mocha, that had been successfully vaccinated with matter direct from the cow, produced as above mentioned; and the virus taken from its pustules had acted with the best effect on several other children at Suez, where former attempts had failed.”—*Lond. Med. Gazette.*

*Cantharidine.*—M. Bretonneau has recently made some experiments to determine the quantity of this vesicating principle, the discovery of which is due to M. Robiquet; contained in the different insects belonging to the family cantharis; according to him it exists in greatest abundance in a species of *coleoptera*, of the genus *mylabris*, to which the name of *variabilis* has been applied by M. le comte Dejean. The following is the process which he employs for procuring the cantharidine. A tube of glass, sealed at one end, is filled to about one third with the insect in coarse powder, sulphuric ether is then poured in, so as to cover the powder to an extent of some lines; the tube well closed is subjected to a temperature of 40° cent. After it has fallen to 30°, and there is no longer dan-

ger of the evaporation of the ether, a ball of cotton is forced into the tube by the aid of a metallic rod, and strongly pressed upon its contents; in an instant, the liquid which the powder had imbibed, is absorbed, clarified, evaporated and leaves a fatty substance which it held in solution. There is a coloured oil furnished by some exterior organ, which combines sometimes with the adipose matter, properly so called, and communicates to it a foreign tinge. With these two substances, the cantharidine is found combined. M. Robiquet has shown that it may be diffused in the fixed oils which thus acquire the vesicating property in a very high degree, a piece of paper, moistened with the mixture, becomes an excellent epispastic, readily adapting itself to the most irregular surfaces; according to M. Bretonneau, no vesicating application is so well adapted to the treatment of erysipelas of the face.—*Gazette de Santé.*

In the April number of the London Medical Repository, Mr. Daniel recommends the internal exhibition of the acetate of lead as an effectual remedy in salivation, and several cases are detailed in which it was successfully employed. Some patients require but a single dose of the salt, and the largest quantity he has ever had occasion to give, was eighty grains in scruple doses. It occasionally produces nausea; but this, he states, does not depend upon the quantity taken, the smallest dose producing that effect as frequently as the largest. His usual dose is ten or twelve grains, combined with the same quantity of pulvis ipecacuanha compositus, or pulv. jalapi, and taken in a little treacle, night and morning, occasionally interposing some gentle laxative. A grain or two, he observes, repeated every two or three hours, will not produce the same effect as one large dose, and the repetition of small doses is more liable to affect the system, though the quantity taken in the same space of time, be not equal to that given in large doses.

*Sulphur in Assafœtida.*—The existence of this substance in assafœtida has been recently ascertained by M. Zeise, the following is a summary of the experiments which led to its detection. If assafœtida be treated with a solution of caustic potash, and an acid be then added, there results an effervescence, occasioned by the escape of a gas which imparts to paper moistened with a solution of the acetate of lead, a colour similar to that caused by sulphuretted hydrogen. If assafœtida be dissolved in alcohol, evaporated to dryness, and nitro-muriatic acid be poured upon the residuum, a liquid is obtained, in which sulphuric acid may be detected. When the volatile oil of assafœtida in a state of purity is burnt, a strong odour of sulphurous acid is produced; the same oil when burnt together with potash, leaves behind a mixture of carbon and sulphuret of potash. M. Zeise states that he has ascertained its presence in

several other organic substances.—*Journal des Progrès, &c.*

*On a New Acid existing in Iceland Moss.*—The reddish purple colour which is produced by adding a decoction of Iceland moss to persalts of iron, has been attributed to the presence of gallic acid, but is found by M. Pfaff to be occasioned by a new acid body which may be separated in the following manner. A pound of the lichen cut small is to be macerated in solution of carbonate of potassa, until all that is soluble is separated; the above quantity will neutralize two gros\* of the carbonate. The filtered liquor is to be precipitated by acetate of lead, and the brown precipitate produced, when well washed, is to be diffused through water, and sulphuretted hydrogen passed through it until all the lead is separated. The filtered liquor is acid, and by spontaneous evaporation, yields dendritic crystals. The crystals, when heated, carbonize, but produce no odour like that of tartaric acid, and lime is left. If they be dissolved and acted upon by alkaline carbonates, carbonate of lime is thrown down, and alkaline salts, containing the new acid, are produced.

The potash salt crystallizes in quadrilateral prisms, needles or plates, and is not deliquescent. The soda salt has similar characters, and the ammonia salt crystallizes in needles. These salts abundantly precipitate the acetate and muriate of iron of a red brown colour; they precipitate sulphate and nitrate of zinc white; muriate of manganese slightly of a clear brown colour; barytic and strontian salts abundantly white; being mixed with strong solutions of muriate or acetate of lime, they gradually produce an acicular crystalline white precipitate; acetate of silver yields an abundant white precipitate, which does not change colour in less than twenty-four hours: they do not precipitate salts of glucina, magnesia, alumine, uranium, nickel, copper, cobalt, gold or platina. This substance has been named the lichenic acid, and is distinguished from boletic acid by the different character of its vapour, and by forming an insoluble salt with baryta.—*Bull. Univ. A.* viii. 270.

*Efflorescence.*—The following observations are by M. Gay Lussac. Many salts when exposed to the air, are well known to effloresce; that is, they fall to powder and lose their water of crystallization; and it is generally supposed that salts after efflorescence are perfectly anhydrous. Having been long convinced that this opinion is not correct, I have made some experiments upon the principal salts which are efflorescent in a high degree. Crystallized sulphate of soda, exposed to the air even when it is not very dry, readily loses all its water of crystallization. Phosphate of soda becomes readily opaque without losing its form. After three months' exposure to the air, it contained on the 18th of July, 7.4 of the

12 proportions of water, which it contains in its usual state. Reduced to powder, and thinly spread upon paper, it contained on the 26th of July 5.65 proportions of water;—again exposed to the air during a hot and dry period, it contained on the 31st of July only 5.65 proportions;—afterwards exposed till the 21st of October, the weather having become colder and more damp, it was found to contain 7.2 proportions of water: some phosphate which had been calcined, absorbed in five days' exposure to the air, nearly half a proportion of water.

Carbonate of soda behaves on exposure very much as the phosphate: it becomes opaque, loses much water without altering its form; but I have never found it anhydrous after exposure.

It results from these observations, that some salts completely lose their water of crystallization by efflorescence; but that others retain variable quantities, according to the hygrometric state of the air. I do not assert, however, that the water may not remain in definite proportions; it merely appears that in the phosphate and carbonate of soda, which retains a proportion of water of a certain number,—the seventh for example,—differs but little from that which unites the proportion, immediately above or below.—*Ann. de Chim.* xxxvi. 335.

*Nature and Composition of the Sweat and Insensible Perspiration.*—This subject formed the prize question of the Heidelberg Medical Faculty in 1821; and the following are the principal results obtained by the experiments of the successful candidate, Dr. Anselmino. The insensible perspiration was collected by confining the arm for some hours in a long glass jar, the mouth being secured to the arm by fine oil-cloth, and the whole kept cool by a stream of cold water. By this method about a table-spoonful was procured in five or six hours. It was neither acid nor alkali. It gave a copious precipitate with lime water; emitted ammonia when it was evaporated to dryness, with a little sulphuric acid, and then decomposed with potass; and gave out a strong acetic odour when it was digested with litharge, evaporated to dryness, and then treated with dilute sulphuric acid. Hence it contains carbonate and acetate of ammonia. If the arm was allowed to touch the glass vessel, other salts were also found in the fluid condensed on its inside; but these salts belong properly to the sweat. The volatilizable part or the sweat is similarly composed, but it contains an excess of acetic acid. The residue of the distillation at 212° consists of chloride of potassium, acetate of potass with an excess of acid, a small quantity of chloroid of sodium, phosphate of lime, a small quantity of alkaline sulphates, a trace of iron, a matter analogous to salivary matter, and another resembling osmazome. It does not contain lactic acid, the acid once supposed by Berzelius to be the lactic, being, as that author subsequently conceived, the acetic rendered less volatile by

\* About one hundred and twenty grains.

being united with animal matter. The existence of iron in the sweat has been since confirmed by Professor Gmelin, who was induced by Anselmino's researches to inquire into this particular. Anselmino's Essay likewise contains a few remarks on the nature of the sweat in diseased states of the system; but his experiments on this department have been hitherto confined to its state in rheumatism. The sweat of a person who had been long affected with rheumatic fever, and profuse perspiration, was of the same nature and composition as natural sweat. The critical sweat of a case of acute rheumatism contained albumen, and the product of the distillation was not acid but alkaline. Next day, after the crisis had been accomplished, it did not contain albumen.—*Zeitschrift für Physiologie, Ed. Med. & Surg. Journal.*

*On the Presence of Ammonia in Argillaceous Minerals.*—Being engaged in the examination of different specimens of gypsum, M. Bouis observed, that traces of ammonia were evident in one containing much argillaceous matter. The peculiar odour common to argillaceous minerals when breathed upon, was very striking in this specimen of gypsum; when a portion of it was moistened with solution of potash, and muriatic acid brought near, white vapours were produced, and reddened litmus paper was very quickly rendered of a blue colour in its vicinity.

It was now suspected that all mineral substances emitting an argillaceous odour, contained ammonia; a great number of specimens were tried, being moistened with solution of caustic potash, and examined by litmus paper. In no case was ammonia absent, and with common clay it continued to be evolved for more than two days. Amongst the substances tried, were pipe clay, other clays, numerous gypsums, Paris plaster, steatite, &c. The antiquity of the mineral seemed to have no relation to the ammonia.

M. Bouis concludes that, in all cases, the argillaceous smell of minerals is connected with, and dependant upon, the presence of ammonia, the latter being the vehicle of this particular odour.—*Annales de Chimie.*

*Sulphate of Quinine in Rigours following Wounds, &c.*—Professor Graefe, of Berlin, has successfully employed this remedy, combined with opium, in those violent rigours which occasionally supervene upon extensive wounds and the more important surgical operations, and so frequently terminate in death. In the greater number of cases, they may be considered as a purely nervous affection, which assumes an intermittent form, without however conforming to any regular type, occurring sometimes twice, or thrice, and at others only once, in the twenty-four hours. The danger is great in proportion to the shortness of the intervals, and the duration of the paroxysm. In the cases alluded to, two grains

of the sulphate, with one of opium, were given at first every hour, afterwards every two hours, the second day every four hours, and the third, at still longer intervals.—*Revue Medicale.*

*Proportionate Number of Relapses consequent upon the different Methods of treating Syphilitic Diseases.*—The following summary is taken from a report on this subject, recently published in Sweden. From 1822 to 1826 inclusive, 16,985 venereal patients were treated in the hospitals of this kingdom. Of this number 39½ per cent. were treated upon the dietetic plan; six weeks were in general sufficient for a cure, when the symptoms were not very violent. The relapses amounted to 7½ per cent. 49½ per cent. were treated upon the mercurial plan; the number of relapses amounted to 14 per cent. The fumigatory method, by means of cinnobar, was adopted in 6¼ per cent; the relapses were 22 per cent. A local treatment was pursued in 5¼ per cent., and of these, the number of relapses amounted to 7 per cent.—*Journal Complementary, &c.*

*Large Biliary Calculus.*—Mrs. E——, aged 55 years, residing at Whilton, Northamptonshire, had been subject for a number of years to occasional attacks of spasms in the epigastric region, which had been allowed to subside and recur without an examination of the excretions. About the 30th of December, after three or four days of occasional violent pain and sickness, a calculus was voided of nearly a cylindrical form, weighing two hundred and seventy-eight grains; *one inch and three quarters in length, and three inches and a quarter in circumference.* It may be as well to state, that large doses of opium, hot fomentations, with aperients, produced a speedy recovery.

A similar case is recorded in vol. x. of the Quarterly Journal of Arts and Sciences, transmitted to the editor by Sir E. Home; the calculus weighing thirty-nine grains less than the preceding.—*Lon. Med. Gazette.*

*Case of Rupture of the Stomach, by Prof. Busch.*—Madame F., 23 years of age, of a very delicate constitution, had suffered from various diseases, and frequently complained of great pain in the abdomen. Her first labour was rather difficult, and the application of the forceps was necessary. She was afterwards attacked with peritonitis, from which she recovered by appropriate treatment. No danger was now apprehended. During a tranquil and deep sleep her attendants heard a sudden crack, resembling the bursting of a distended bladder. The patient awoke, screaming violently, and with the utmost distress depicted in her countenance. She felt assured that rupture of some internal part had taken place. Nausea and vomiting quickly succeeded. She threw up a quantity of matter, which had the appearance of bile mixed with blood. Her restlessness increased: the belly was very

painful, and much distended. In about 48 hours she died, all the symptoms of distress having previously disappeared. Upon examination of the body evident traces of inflammation were found throughout the intestines. The external organs of generation were cedematous. The uterus was healthy. The stomach was empty, and ruptured throughout nearly its whole extent. The cavity of the abdomen contained a quantity of a yellow and very acrid fluid. The hand and arm of the surgeon were subsequently much inflamed, and in some parts ulcerated, by handling the parts. From the symptoms under which this patient had long laboured, it was presumed that some disease of the stomach had been going on for a considerable time.—*Journal für Geburtshulfe*.

**Iodine in indurated enlargement of the Uterus.**—Dr. Thetford has published a paper in Transactions of the Dublin College of Physicians, in which he describes the complete success which attended the use of iodine, in a case of indurated enlargement of the uterus. On examination, the os uteri was found projected into the vagina, nearly to the labia; and the uterus was ascertained to be of osseous hardness, and of so considerable a size as nearly to fill the whole of the pelvis. The patient had nearly fallen a victim to extreme costiveness and retention of urine, before she was visited. These inconveniences being removed, mercurial alteratives were had recourse to; but it was not until the tincture of iodine was prescribed that any hope of decided relief could be entertained. Dr. Thetford began by seven drops, three times a day, in a wine-glass of water; and afterwards increased the dose to ten drops, which he found it unnecessary to exceed. Progressive absorption of the diseased substance of the uterus rapidly took place, and the catamenia (the patient was 40 years of age) were regularly restored. This favourable change was effected in about six weeks after the iodine plan of treatment was begun.

**Iodine in Arthritis.**—At a recent meeting of the Institut royal de France, M. Gendrin announced, that for some years past he had successfully employed this remedy in gout, occasioning the absorption of the arthritic concretions, and preventing the return of the paroxysm. Seven cases of the disease in its acute and chronic form, were mentioned as having been cured in this manner.—*Jour. de Chimie Med. &c.*

**Animal Matter in Mineral Waters.**—A green matter is deposited from the water of the hot alkaline springs of Vichy, in France. It was analysed by VAUQUELIN, who found it to resemble the white of an egg. It is worthy of remark, that springs in the south of France, and in the north of Italy, which issue from primitive rocks, should contain this substance, whose composition is so nearly the same as that of organic matter.—*Edinburgh New Philosophical Journal*.

**Simultaneous Luxation of both Arms.**—A labourer, bearing a heavy load, fell forwards, and attempting to save himself, threw out both his hands: an acute pain in each shoulder, and a luxation of each os humeri, was the immediate consequence. The patient was brought to the Hôtel Dieu on the 1st March: in the evening this double luxation was immediately detected and reduced. We notice this case, because we believe it to be singular in its kind; at least we do not think that such another has been recorded.—*Nouv. Biblioth. Med.*

**Preparation of Hyposulphuric Acid.**—According to M. Heeren, to obtain the greatest quantity of this acid in the process of passing sulphurous acid over black oxide of manganese, the temperature should be low, and the oxide finely divided. The largest portion of hyposulphuric acid is formed at the commencement of the operation.

**Syphilis communicated by Leeches.**—A German Journal, *l'Indicateur Westphalien*, cites a case of syphilis occurring in a child in consequence of the application of leeches which had already been employed on a person affected with that disease. This circumstance, if well founded, evinces the necessity of greater care in their employment.—*Nouv. Biblioth. Medicale*.

**Division of the Soft Palate.**—At a late meeting of the Academy of Medicine, M. Roux stated, that, within the last three years, he had performed the operation for the divided palate twenty-eight times. In the five years preceding these, he had only performed the operation twelve times, making in the whole, since he first undertook the operation, forty times. Nineteen of the cases were in the most favourable circumstances for the operation, and among these it failed only in six, the parts being left in such a condition as to allow of the operation being repeated. In the remaining twenty-one cases, there was a separation of the hard as well as that of the soft palate; in nine, the operation succeeded; in the others, considerable relief, though not a complete cure, was afforded.

**Rob of Laffecteur.**—M. Tapis some time ago announced that he had succeeded in separating corrosive sublimate from this compound by means of ether, and a commission was appointed by the Society of Pharmacy to report upon his communication, which as his experiments did not succeed when repeated by them, they did unfavourably. That gentleman has again devoted his attention to this subject, and the result has been a complete confirmation of his former statements. He has extracted two grains of corrosive sublimate from eight ounces of the rob.—*Jour. de Chimie Medicale, &c.*

**Tables of Mortality.**—M. Odier has been lately engaged in investigations on the ave-

rage duration of life in Geneva, all of which show a gradual extension of the period. The inquiries of M. Odier go as far back as 1560, and may be fully relied on. From 1560 to 1600, the average duration of life was eighteen years and five months; from 1600 to 1700, it was twenty-three years and four months; from this time, the period became gradually longer; and, from 1815 to 1826, the average was thirty-eight years and ten months. *Biblioth. Univer. de Genev.* t. xxxvi. 36. 1827.

*Hereditary Predisposition to Cataract.*—A striking instance of this kind, recently occurred in the practice of M. Dupuytren; a grand mother, her daughter, and three grand children, were all affected with the disease.

*Accension of Arsenical Cobalt Ore.*—M. Boulay has had occasion to observe that, having powdered a large quantity of Arsenical Cobalt Ore, the mass of powder heated of itself, without any application, and ultimately took fire. —*Ann. de Chimie.*

*Iodine in Cadmium.*—Iodine is found in the great zinc foundry at Konigshute, in Upper Silesia, in the cadmium which accompanies the zinc ores. —*Poggendorf's Journal.*

*New Method of preserving Crystals of Salts.*—Mr. Deuchar, in a communication to the Wernerian Society, mentions that crystals of efflorescent and deliquescent salts can be preserved from decay, if the air of the jars in which they are kept be impregnated with oil of turpentine. This is effected by pouring a very small quantity of the oil over the bottom of the jar. —*Ed. New. Phil. Jour.*

## New Publications.

*Manuel de Therapeutique et de Matière Médicale suivi d'un formulaire pratique;* par L. Martinet, docteur en Médecine, &c.

*De l'emploi de l'opium dans les phlegmasies des membranes muqueuses, sereuses et fibreuses;* par S. L. Brachet, medecin de l'hôpital et de la prison de Lyon, &c. &c.

*Recherches sur la Nature et les causes, prochaines des Fievres;* par A. N. Gendrin, M.D. rédacteur du Journal General de Médecine, 2 vols. in 8vo.

*Lectures on Anatomy, Surgery and Pathology;* including Observations on the Nature and Treatment of Local Diseases, delivered at St. Bartholomew's Hospital. By John Abernethy, F.R.S., Surgeon to Bartholomew's and Christ's Hospital, &c. &c. &c. London, 1828, 8vo. pp. 585.

This volume contains all of Mr. Abernethy's Surgical Lectures, as well as the Physiological and Pathological Observations which he delivers in his Anatomical Course. The name of the Author is a sufficient recommendation to the work; but, did it require more to recom-

mend it, we might say, that so much useful information cannot be acquired from any other work of the size that we are acquainted with.

*A Disquisition on the Nature and Properties of Living Animals, with an Inquiry how far our Knowledge of Anatomy and Physiology is consistent with the Belief of a Soul and a Future Life, and on the Intellectual Difference between Man and Brutes.* By George Warren, Surgeon, pp. 144, 8vo. Longman and Co. London, 1821.

This work proves the author to be a man who thinks for himself. Some of his views are original, but we do not profess to know whether they are right or wrong.

*The Morbid Anatomy of the Brain.* By Alexander Monroe, M.D. F.R.S. and F.A.S.E. &c. &c. Vol. I. Hydrocephalus. Illustrated by Copperplates—8vo. pp. 200. London, 1828.

*A Practical and Pathological Inquiry into the Sources and Effects of Derangements of the Digestive Organs, embracing Dejection, and some other Affections of the Mind.* By Wm. Cook, Esq.—8vo. pp. 290. London, 1828.

*Munro's Illustrations of the Anatomy of the Pelvis.* Folio.

*Recherches Anatomiques, Physiologiques at Pathologiques sur le Systeme Veineux.* Par M. G. Breschet, M. D.

This work will be published in 20 or 22 livraisons, each containing six Lithographic plates, which will appear regularly every two months. The first and second numbers have already been published.

*Cases illustrative of the immediate effects of Acupuncturation in Rheumatism, Lumbago, Sciatica, Anomalous Muscular Diseases, and Dropsy of the Cellular Tissue;* selected from various sources, and intended as an appendix to the author's treatise on the subject. By James Mors Churchill, F.R.S. &c. &c. 12mo. pp. 101.

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MEMOIR ON SPRAINS. By M. A. PELLE-  
TIER, Hospital Surgeon at Mans.

It is to be regretted that, while cases presenting any thing rare or peculiar, are generally told with minuteness, little or no notice is taken of diseases that are apparently slight, but which, from their great frequency, ought to be well known, and have some proper rules of treatment laid down. This particularly applies to the subject we have chosen, as *sprains* are thought to be curable by any body, and quacks employ, to cure them, means that are sometimes dangerous.

To be precise, we would define sprains to be the forcible extension of the white fibrous tissues in the vicinity of joints, and serving them as means of union, such as ligaments, capsular fibres, and tendons, which for the most part act as ligaments. The causes of sprain may be distinguished into direct or determining, and indirect or predisposing. The direct causes generally are all efforts supported by the articulations, either in the exercise of different professions or in the different accidents to which men are liable. As these efforts must separate the articular surfaces, so they must have the effect of stretching the ligaments more or less. Therefore, those joints most exposed to great efforts are the most likely to be sprained, particularly if these joints do not possess a certain laxity, capable of permitting considerable movements without much extension of their ligaments. The scapulo-humeral joint, in consequence of possessing this laxity, although partly exposed to such accidents, is rarely a subject of sprain: on the contrary, the tibio-tarsal, radio-capsule joints, &c. whose articulating surfaces are closely united, give the most frequent examples of sprains. The tibio-tarsal articulation is particularly so, and in itself presents more cases of sprain than all the other joints of the body. It can equally well be conceived that the ligaments in these articulations most exposed to support violent efforts, are most liable to sprain. So M. P. remarks, that he has found that, out of ten sprains of the tibio-tarsal articulation, eight at least affect the external ligaments; the foot having more disposition to adduction than to abduction. Among the in-

direct causes, some are relative to the mode of life or profession of individuals, others to constitution. In the first class may be placed the custom of taking violent exercise, the use of high-heeled shoes, pattens, &c.: in the second class we find considerable fatness in conjunction with a lymphatic temperament, the one increasing the weight of the body, and the other relaxing the ligaments; scrofula, &c.

*The nature and character of Sprains.*—It would at first appear that so simple an accident, particularly when there is no tearing of the ligament, could hardly cause any troublesome symptoms; but theory teaches, and experience every day shows, that fibrous tissues, which could be divided by a sharp instrument without pain, are never submitted to violent distention without causing much pain, and symptoms more or less severe; but the vital properties being naturally very obscure in these tissues, they cannot immediately be exalted, so that some days often will elapse between the distention and the development of the inflammation caused by it. And such is precisely the source of the imprudencies which patients commit, in continuing to exercise a limb after it has suffered a sprain. Suppose a violent extension to have been applied to the foot, the patient experiences at the moment a severe pain in the distended ligaments. This soon ceases, and he resumes his exercise, which he had for a moment interrupted. After some time, the vital properties develop themselves gradually in the distended ligaments; and inflammation shows itself; the pain becomes intolerable, and, if the subject is scrofulous, the consequences may be most serious. M. Pelletier thinks that, from neglect, a sprain becomes a greater evil and more difficult of cure than a fracture.

*Diagnosis of Sprain.*—An unfailing method of discovering sprain is to put the limb in the situation it was at the time of the accident, and gradually and carefully to bend the joint in the same way. For example, if an external sprain of the foot is suspected, to carry it gradually into adduction; soon pain begins, which disappears on moving the joint the opposite way.

Sprain should never be considered as a trivial accident. It often happens that what at

first appears simple, shows in a few days very urgent and distressing symptoms, when neglected or improperly treated. The greatest precautions are necessary, and it is better to err on the safe side by too much than too little care. To establish the basis of a regular treatment of disease, it is indispensable to be acquainted with the progress of the disorder, and to mark precisely its different periods: this is particularly applicable in sprain, since its treatment varies much in its different stages, and what would be serviceable at one stage may be most improper at another. Let us suppose a sprain entirely left to nature's efforts, and going on as far as it can, we can distinguish four periods: 1st, the forcible extension of the ligaments, with severe but generally evanescent pains; 2d, the development of acute inflammation in the parts injured; 3d, the change from acute to chronic inflammation; 4th, organic degeneration of the inflamed parts, and accidents attending it. After describing each of these periods, we shall point out the appropriate treatment.

First period—the forcible extension of the ligaments. This begins at the instant when the accident has happened, and lasts generally from twenty-four hours to two or three days. It results from several causes. It may be asserted that, in general, inflammation manifests itself so much the more quickly as the extension has been great, the subject lymphatic, young, and gifted with much vitality: on the contrary, so much the more slowly as the sprain has been slight, the subject aged, or of a constitution less irritable. When sprain takes place, there is acute pain in the distended ligaments: sometimes this pain disappears very quickly, and the person resumes the exercise he was about. Now this is the insidious part of it, and favours the development of further mischief, by preventing persons from taking that rest which is so essential to the cure. Soon after the extension there comes on a puffy swelling in the neighbourhood of the parts affected, a true active œdema, sometimes even ecchymosis, greater or less. These must not be considered as the result of inflammation, (which has not had time to develop itself yet,) but as the effect of a lymphatic afflux into the cellular tissue, where the pain was felt. What proves this is, that the swelling gives only an œdematous feel, without heat or local pain on pressure, and which quickly disappears with rest. Surgeons, who have not sufficiently observed this complaint, are deceived by this disappearance of the swelling, which they regard as the principal symptom; and look upon the disease as terminated when this is dispersed, that is, on the second or third day. So that they give up all further treatment, and permit exercise at the very time when inflammatory symptoms are about to appear.

Second period—development of active inflammation in the distended ligaments. The tissues of the living animal economy, when subject to the influence of excitation, *cæteris paribus*, inflame with a rapidity corresponding

to their supply of blood-vessels, and to the degree with which they are endued with well developed vital properties. But this inflammation is not immediate: it is, in fact, rare that it manifests itself before twelve or twenty-four hours in tissues the most disposed to its invasion; and, in those less disposed to its influence, many days elapse before it appears. This is in conformity with what happens after grand operations, particularly amputations of limbs, when numerous and different tissues are divided. We must not, then, be astonished that inflammation of fibrous tissues, whose vital properties are so obscure, does not appear till a distant period from the time of the accident producing it. This circumstance, attention to which is of the greatest moment in the treatment of sprains, is then quite naturally explained by the mode of organization of the affected tissues. Generally, inflammatory symptoms manifest themselves from the second to the fourth day, and very frequently when the first pain is quite gone. If there is little swelling, the parts sprained have a natural appearance, and the patient, not understanding his situation, often refuses to keep at rest. Spirituous applications appear to do good by helping to dissipate the swelling during this treacherous calm; but soon the scene changes. The patient feels in the injured part pain, at first dull, then more severe, at last more acute, preventing the possibility of moving the joint without anguish. Soon after, a shining appearance, heat and swelling, painful feeling of distention, throbbings and dartings in the part, come on; and few medical men will not now acknowledge the necessity of rest and antiphlogistic means; but scarcely are these first symptoms diminished, when the patient gets tired of confinement, and wishes to resume his ordinary avocations; while the surgeon, thinking he has now only to forward resolution, instead of feeling that there is still inflammation to destroy, abandons too soon the antiphlogistic system, and passes to a resolutive treatment, thus recalling the inflammatory symptoms, and making them chronic. Here terminates the second period. We may easily imagine what must be the consequence of applying in this stage spirituous resolving embrocations, &c. We have seen from it the inflammation rapidly extend itself to the synovial membranes and neighbouring tissues, bringing on disorganization and suppuration of these tissues, caries of the bones, hectic fever, and death.

Third period—change from active to chronic inflammation. The affected joint becomes the seat of an habitual irritation, too weak, when attended to, to bring back inflammation in the acute form, but sufficient to make it pass into the chronic. This joint then becomes enlarged, and towards night is swollen, feels stiff: the patient, not aware of the mischief that may follow, continues his exercise, and the more willingly as he looks upon the play of the joint as absolutely necessary to its gaining strength. We have seen cases kept up this way for years.

Fourth period—"Degeneration Lardacée," lardaceous degeneration of the inflamed white tissues. The continuance of chronic inflammation of the affected ligaments, the cure of which is the work of years, is of itself troublesome enough, and yet this is by no means the most fatal consequence that may occur from neglected sprain. Often, indeed, the chronic inflammation kept up by exercise and other means, extends itself by degrees to the cellular tissues, the synovial membranes, periosteum, and even to the bones themselves. This inflammation becomes more intense according as it attacks parts more vascular and nervous, causing pains which daily aggravate till they force the patient to keep his bed entirely, and the surgeon to have recourse again to antiphlogistic means. But it is too late; the return of the lancinating pains announce a lardaceous degeneration, which, once established, there is no escaping from. The joint loses its natural form, puffs up, becomes round, the skin covering it becomes semi-transparent, and of a milky white; a constant feeling of deep distention, and at times violent lancinating pains, torment the patient. Often abscesses in the synovial membranes under the periosteum, denudation and caries of the bones, with fistulous openings. The lancinating pains become almost constant, with sleeplessness, marasmus, hectic fever, night sweats, colliquative diarrhœa, and, if the part is not sacrificed to the preservation of the individual, death is not slow in bringing to a conclusion this deplorable scene. Such are the terminations of sprains, neglected or empirically treated.

We now proceed to the *treatment*.

There are few diseases against which have been devised more numerous, much varied, and sometimes more opposite modes of cure. Must we infer from this that the authors who have mentioned them have been deceived, or that the disease may be cured by methods essentially different. The most opposite means may, we doubt not, cure a sprain; but this truth, which astonishes the ignorant man because he cannot understand it; which makes him consider the curative art as merely conjectural, and depending for its results upon chance, explains itself quite naturally to the physician, who reasons and draws his curative means from the result of observation.

We have already distinguished four periods in the history of this complaint, and the explanations we have added must make it clear that, if each period called for certain means, these means ought to be different when applied at these different periods. It is precisely for want of this important distinction, and from having considered sprains as always alike, whatever stage they might be in, that the same confidence has been extended to cold applications and emollient poultices, to leeches and stimulating applications, to Goulard water and camphorated spirit, rest and exercise, &c.; and that, in consequence, a perfect chaos has arisen from the *ensemble* of these means of cure. What is still more to be lamented is, that the use of certain means at

particular periods being supported by the sanction of celebrated men, ignorant quacks employ the same means in all cases without distinction, and in all the stages of the disease. It is easy to conceive what mischief may arise from such abuses. There is but one way to avoid these fatal errors, and to establish fixed rules for the treatment of this disease, viz. to review the four periods as they occur, and to point out successively the treatment suitable to each. If the surgeon is called to a patient in the first period, when there is considerable pain caused by violent extension, there are two points to be attended to. The first is to examine if there is rupture of any of the ligaments, which might be determined by estimating the force of the cause, the situation of the parts at the time of the accident, the continuance of pains, and the rapid swelling of the neighbouring parts. This last symptom is important to note: it renders the development of inflammation almost inevitable, whatever precautions are adopted, and therefore at once indicates the necessity of antiphlogistic means. The second thing to be attended to is to allay the pains and prevent the accession of inflammation; and, among the means of doing so, *rest* is the first; without this all others will fail; and, in conjunction with rest, we must make use of *revulsive* or *resolutive means*. These are usually looked upon to consist in certain external applications, which have the power of hastening back the circulating fluids into the great vessels, from whence they had been sent with so much increased energy as to distend the capillary vessels of the part affected. Without cavilling about this definition, we shall distinguish these means into three classes, essentially different in their manner of acting: 1st, cooling resolvents; 2d, astringent resolvents; 3d, irritating resolvents.

*Cooling Resolvents*.—In this class are all those applications which diminish the heat of the part, as snow, cold bath, &c. It has long been recommended to plunge the affected part into very cold water immediately after the accident. Now this we can imagine may sometimes prove serviceable, but in others may be most pernicious. To satisfy ourselves of this, let us analyze its manner of action. If the action of cold is sufficiently strong, but only temporary, it causes at first a numbing of the part, a sort of local asphyxia; all the organic energy appears to come to the aid of the part threatened with mortification, and by a reaction, the force of which is in proportion to the strength and age of the patient, strongly awakens the life of that part, pushes towards it with force a great quantity of blood; it becomes, in fact, a centre of irritation, sometimes of instantaneous inflammation. One has only to plunge the hand into snow or very cold water, and observe the effects, to be satisfied of this. This means, then, when employed only for a short time, by bringing on powerful reaction, acts as a very active excitant. Some authors, for this reason, place heat and cold on the same parallel, calling the first a

direct, the second an indirect excitant, their results being similar. If the action of cold is energetic and long continued, the parts submitted to its influence are as it were paralyzed, and this suspension of their vital properties may cause the extinction of their irritation. This is what happens in a part frost-bitten. After all these considerations, one may perceive that the immersion must be, *cæteris paribus*, prolonged as long as reaction is to be feared; longer, consequently, in a young than an old subject, in a vigorous than in a weakened state. If one might fix a general period during which immersion should be continued, we would say from eight to twelve hours. We have adopted for the employment of this means a method which appears to obviate the greater part of the inconveniences which may attend it. It consists in plunging the affected part into water, the temperature of which is only a few degrees below that of the skin, and gradually diminishing the temperature till the requisite cold is induced. By doing this we almost always avoid the inflammatory reaction, which might show itself after twelve or fifteen hours cooling. There are circumstances which ought to prevent this means being used, as, for instance, in women near the menstrual period, in persons subject to internal inflammations, and in all immediately after meals, as we have seen it, when this was not attended to, cause violent vomitings.

*Astringent Resolvents.*—This includes those applications which, applied topically, cause a constriction of the parts, without any apparent inflammatory reaction, as Goulard water. The best way of applying this is to keep the part constantly covered with compresses moistened with this fluid during several days. We would prefer this plan to the immersion.

*Irritating Resolvents.*—By this is meant camphorated spirits, soap liniment, eau de Cologne; all, in fact, which have alcohol for their base. So far from being resolvent, we are inclined to think that their effect is to bring on inflammation where it was not before, and increase it if previously there. They should never be applied in the first or second periods; in the third they are sometimes applicable. Much mischief is done by the indiscriminate applications of such medicaments, and therefore they ought to be proscribed in these cases.

*Compression.*—Some writers have recommended the use of compression in the first period of this complaint, considering it as opposed to the tumefaction of the parts affected; but reason and experience demonstrate that this means is then most hurtful. It can only be useful when the patient is getting better, and beginning to take exercise.

*Narcotics.*—Any means capable of removing or even allaying the pain must be serviceable: in fact, the pain acts as an exciting cause to inflammation, which it is so essential to prevent. Narcotics joined with alcohol, such as the absurd and contradictory association of an irritating and calming medicament, like laudanum, are hurtful. A simple aqueous decoction

of opium, or the poppy heads, or infusion of saffron, employed in fomentation or poultice, is generally successful in allaying pain.

*Local Bleedings.*—The application of leeches is sometimes useful, sometimes even indispensable, in the treatment; sometimes, however, it is hurtful. For example, when the sprain is slight, the subject weak, of a dry nervous temperament, there is no need for them; the other means suffice: but if the sprain is violent, the subject young, vigorous, of a lymphatic or sanguineous temperament, and if the swelling immediately becomes great, then leeches must be freely had recourse to. They should always be applied to the most cellular and fleshy parts, and several inches away from the strained parts. When applied immediately to the injured parts, they constantly produce troublesome effects. The irritation caused by the bites unites with that from the sprain, the swelling increases frightfully, and the inflammation becomes more intense.

*Emollient applications.*—Under this head we range poultices, warm baths, fumigations, and fomentations. These means, the advantages of which are incontestable in the treatment of sprain, are often very successful after leeching. We have observed that, when they are applied before the leeches, and particularly when very hot, they increased the swelling, and promoted the development of inflammation, rendering the pain sometimes so intolerable that the further use of them becomes impossible. We should then always premise a copious local depletion, and regulate the temperature, so as not to increase the heat in the affected part, which is already too considerable.

*Treatment of the second period of Sprain, or acute inflammation of the distended ligaments.* Although in the first stage many means, apparently opposite in their mode of action, have produced equally satisfactory effects, we must not expect this in the second. There now exists an inflammation, painful in its nature and difficult to root out, owing to the organization of the affected tissues, and dangerous in its consequences, by its contiguity to a joint which it may involve. Only one kind of treatment now remains, namely the antiphlogistic. Thus the cold bath, but particularly alcoholic preparations, may produce the most fatal consequences. To remove all causes capable of inciting or increasing inflammation,—to diminish the quantity of blood carried to the part inflamed,—to oppose, as much as possible, the exaltation of vital properties in the affected tissues,—to destroy, or at least lull, pain,—to avoid or oppose the irritation of organs which sympathise more directly with the diseased parts,—such are the principal indications and the basis of treatment at this period. The complete rest of the affected joint, the horizontal position to prevent swelling, leeches in abundance, and with the precautions above mentioned, and then large and thick poultices made of linseed meal and decoction of poppies, are to be had recourse to.

If poultices keep up the heat too much, fomentations may be substituted. The intimate connexion between the intestinal canal and the joints is now well known: it is, therefore, necessary to put the patient on a mild regimen, to avoid whatever may possibly produce gastro-intestinal irritation. It is amply proved by experience that indigestion, the abuse of spirits, &c. have an evil influence on sprains.

*Treatment in the third period, when the inflammation has become chronic.*—When the patient has not attended to giving the joint rest for a sufficient time, or when the treatment has been improper, &c. the inflammation of the distended ligaments may, in moderate cases, insensibly diminish in intensity; but, being always kept up by improper treatment, or injudicious exercise, it at length passes to a chronic state, and may thus perpetuate itself indefinitely. Consulted under such circumstances, the surgeon must not lose sight of the fact that this chronic inflammation, although it does not offer any very pressing symptoms, is not the less an exaltation of vital properties, which it is necessary to oppose by appropriate means: therefore exercise, exciting resolvents, and all stimuli, of whatever nature they may be, are directly mischievous. It is by not attending to these principles that physicians prolong such cases during years; and, what is more distressing, cause, by the empirical applications made use of, a degeneration of the affected tissues, inflammation and caries of the bones, and even loss of limb. It is then essential, in this third period, to employ still the means recommended in the second, but modified according to the different form of the inflammation. Thus rest is not so essential, at least, unless the mischief is so advanced, and disorganization so much to be dreaded, that it only remains to attempt a cure by ankylosis, and then the limb must be fixed in the most convenient position. But, should this not be the case, the joint should daily be moved gently, taking care, as much as possible, during those movements, to place the affected ligament in a relaxed position, so as not to bring on again fresh distentions. Leeches may be applied with much advantage, if there is swelling and pain; five or six, every two or three days, will be sufficient. Here it is particularly necessary to apply a principle in pathology too little known. In chronic diseases the treatment should be chronic,—that is, should act in a slow but continued manner. It is by employing local bleedings in this manner for thirty or forty days that the most satisfactory results have been obtained, not only in sprains, but in almost all slow inflammations, even with threatening of disorganization. When the inflammation is quite dissipated, and the joint in an indolent state, and still a puffiness and soft feel in the ligaments remain, we may then have recourse to resolvents, but cautiously, and without ever painfully increasing the sensibility of the parts affected. Goulard water, hydrosulphureous baths, and light frictions with camphor and calomel, mixed with lard so as to make an oint-

ment, moderate exercise, the part being properly supported by bandages and laced stockings, if it is the foot.

*Treatment of the fourth period, when there is disorganization of the inflamed parts, and other accidents.*—The further we advance in the different periods of this complaint, the treatment becomes the more difficult and uncertain. When chronic inflammation has disorganized the white tissues,—when the ligaments, tendons, and synovial sheaths are already converted into a white mass, homogeneous and lardaceous, there scarcely remains any hope either of restoring these parts, or of stopping the progress of this fatal disorganization. When this alteration, generally known by the name of white swelling, is recent, and the health of the person good, the case sometimes ends favourably; but, if it is of long standing and deep seated, with lancinating pains, and particularly if the patient be scrofulous, all attempts at cure are useless: we can only palliate. Many means have been recommended in this fourth stage, as any thing which has for its object to destroy a deep-seated irritation by a superficial one, either of the skin or subcutaneous cellular substance, such as ammoniacal poultices, spirituous liniments, blisters, setons, moxas, cauteries, &c. When these means have succeeded, and there no longer is deep-seated inflammation, and no disposition to relapse, even under these favourable circumstances, too strong applications have reproduced the deep-seated inflammation by the extension of the superficial one, and have hastened the progress of a disorganization which had perhaps been stopped in its course. Judge then, what disorders these applications must occasion when applied to parts already inflamed and the seat of the white swelling. The pain speedily becomes insupportable, swelling and heat come on; sometimes suppuration in the interior of the joint, with hectic fever, &c. till death closes the scene. We have seen subjects in the flower of their age, and whose general health did not appear much changed, perish within twenty or thirty days from the time of applying these means. One must, then, never lose sight of these results, and dread them still, even when the joint appears the most favourably disposed to stand without injury the use of moxa, setons, blisters, &c., since the inflammation caused by these means may extend to the subjacent tissues, and there bring on the symptoms which have only been lulled.

If, then, there is any hope of cure, it is more prudent and certain to employ those means recommended in the third period, modifying them always according to the circumstances: rest, partial bleedings from the part, emollient and calming topical applications, and, lastly, gentle resolvents, ought to form the basis of the treatment. If these means will not do, no other will; the disease is incurable, and the complaint goes on from bad to worse, till he either dies or submits to an operation by which the affected parts are removed. But this amputation is not always practicable, either from

the situation of the complaint, (as, for instance, when seated in the pelvic articulation, or in the vertebral canal,) and also from various circumstances, which may contra-indicate its employment. It may then be useful to point out some fundamental rules in those cases where we might save a patient by sacrificing a part for the preservation of the whole, and those in which the operation would only add new sufferings without the chance of benefit. Now, where the patient has had his constitution generally and extensively altered before the local accident which calls for the amputation, it would not only be useless to propose amputation, but it would do harm. This then is one case where we should not amputate.

When the patient has had pretty good health before the accident, and chronic suppuration, with sanious absorption which usually accompanies it, has brought on a constitutional derangement, "*cacochymie*," attended with dyspnœa, dry cough, especially when the patient makes a deep inspiration or the chest is struck upon, colliquative diarrhœa, slow fever, partial night sweats;—amputation under these circumstances might remove the first cause of the symptoms, but will not destroy the effects produced on the respiratory or digestive organs. These are not capable of retrograding, because they depend not only on the chronic inflammation of the tissues affected, but also on the organic lesions produced in these tissues by prolonged inflammation. This then is another case unfit for amputation.

When there is a decided scrofulous taint in the constitution, it should forbid the operation.

When a patient is originally of a sound constitution, and experiences a deep-seated chronic inflammation, with disorganization, in one of the principal joints, as the foot, knee, wrist, or elbow, in consequence of a local accident, such as a bruise, a wound, or a sprain, if that patient is nearly free from fever, the digestive organs in good order, the chest sound, and none of the internal viscera are affected, even although he should be exceedingly reduced in flesh and strength, we must not despair; for, by removing the affected part, we remove the focus of disease. This then is a case in which amputation is prudent and necessary, and such patients will recover sooner from amputation than those where they are in high health when the operation is performed; for, in the first case, the system being reduced to a kind of languor and inertness by its losses and sufferings, does not present those febrile and inflammatory reactions, which in the other may sometimes compromise the success of the operation, and cause a fatal termination.

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OBSERVATIONS ON FEVER. Addressed to a Medical Friend, by Dr. Bow, of Alnwick.

(Continued from Vol. II. page 16.)

In some work or other I have read as the opinion of its author, that the animal system was possessed of one dose of vitality, which

could not become superabundant in one part without a proportionate deficiency in some other part, and vice versa. Such a law, I suspect, obtains with nervous influence. If there be too great a determination of it to one particular point or surface, it will be proportionally withheld from other parts; and if we diminish the sensibility of a part, so that usual impressions are not recognised, the usual force of influence is not transmitted to such part, and consequently it is transmitted to some other part or parts in greater force. If to the healthy subject we administer a brisk cathartic, we by it cause a determination of nervous influence greater than natural to the gut; therefore, during the operation of the medicine, there will be felt a greater or less degree of muscular debility and of coldness of the surface, feelings plainly arising from deficiency of nervous influence, owing to the demand for it elsewhere. Although the *general* action induced by a cathartic be sedative, the cathartic itself is a stimulant, and directly acts as such. On the other hand, if we administer a moderate dose of opium, we thereby diminish the sensibility of the stomach, and consequently the usual force of nervous influence is not elicited; it is, therefore, transmitted elsewhere in greater force, giving rise to hilarity of mind, increase in the force and frequency of the pulse, increased warmth of the surface, with moisture of the skin; yet, though these symptoms be demonstrative of excitation, they are not the immediate effects of the opiate. Opium is a direct sedative, acting upon the nervous extremities, producing a kind of torpor of them, by which they are more or less unfitted as instruments of transmission of impression to, or of influence from, the nervous centres. If the dose of opium be increased, there is torpor communicated to the brain itself, by which it becomes unconscious of all impressions, and sleep is the result. If opium act by diminishing the sensibility of the surface to which it is applied when administered internally, and if, in consequence of this effect, there be a greater determination of nervous influence to the surface of the body, we ought reasonably to expect that its external application would, by diminishing the sensibility of the skin, cause a greater determination of nervous influence to internal organs; and this we find to be the case. In debilitated habits, I invariably find that opium applied externally acts as a laxative, and I am not only in the habit of so applying it with the view of moving the bowels, but also of improving the appetite; effects very different, indeed, from those resulting from its natural administration.

If a patient labouring under typhus fever be left to himself, or if the treatment during the first stage, and three first days of the second stage, be inertly conducted, and if nature do not assist him by some critical discharge, then is he in danger of ulceration of the mucous and muscular coats of the intestines; a circumstance which happens so frequently, at least on the continent, as to have given rise to the celebrated though delusive

doctrine of Broussais. I have said that nervous energy generally begins to decline on or after the third day of the second stage, after which time, if the irritation on the surface of the body continue, nervous influence will still continue to flow to the skin, though actually not in such great force as on the preceding days of the same stage, yet in greater force in proportion to that transmitted to other parts; for we find that, the weaker the patient, the more easily is the stream of nervous influence directed to or from any particular surface. Therefore, the irritation of the surface continuing, and as now nervous influence is more easily diverted, internal organs are in a greater degree deprived of it. And that the mucous membrane of the intestines should be the part to suffer in consequence of the diminished supply, is just what might have been expected from the distribution of the branches of the sympathetic nerves; for, although these nerves send branches to every part of the body, their principal expenditure is on these two extensive secreting surfaces, the skin and mucous membrane of the bowels: hence an agent acting on the one surface must necessarily induce a contrary action on the other. Indeed, either of these surfaces is so liable to be affected by causes operating on the other as to be a circumstance of daily observation. Nervous influence being then almost wholly diverted from the mucous surface of the intestines, infarction of its follicles, venous congestions, ulcerations, and gangrene follow. I have not been present at any dissection where the ulceration alluded to has been witnessed, nor have I seen a more accurate description of it than that by Dr. Hewett, in the *London Medical and Physical Journal* for August and September, 1826; and I think whoever peruses the cases and observations there given will at once recognise disorganization from want of action rather than ulceration consequent to inflammation. As a proof that this want of action arises from the determination of nervous influence to the surface, owing to the superior sensibility of the skin, I may observe that such appearances may be found to follow a preternatural determination of influence to the surface, however caused; for unless upon this principle, it would be difficult to explain the appearances found by Dr. Hewett in the young woman who died in consequence of extensive burn. If this view of the subject be correct, the conclusion to be drawn from it is of the utmost importance in practice. With the view of turning the stream of nervous influence inwardly, I frequently employ opium externally during the second stage of fever, and from that ever get what I expected, viz. an improved state of the stomach and in the appearance of the tongue; and as I must declare my astonishment at the rapid recoveries of some, may I not believe that, by determining nervous influence to the intestines, I have unwittingly accomplished a more essential point, that of arresting or preventing the ulcerative disorganization of the mucous coat of the intestine?

The perusal of Mr. Ward's communications to the first and some of the succeeding volumes of the *London Medical and Physical Journal* led me to the employment of opium externally in fevers, and I know not why the cases and facts stated by him have not urged the generality of practitioners to adopt the practice, unless, indeed, the supposed *modus operandi* of opium. For if opium acted only on the system after having gained the circulation, the effects following its exhibition would be the same in every instance. But, as this is not the case, there being a very sensible difference in the effects of its external application, and as the mere mode of its introduction, whether by lymphatics, lacteals, or otherwise, is inadequate to explain the difference, perhaps this difference may be supposed, by those who favour the idea of absorption, to rest more on fancy than on truth.

In prolonged fevers, the low muttering delirium, the restlessness, and the denied sleep, are, I think, caused by the deficient secretion of the fluid destined to lubricate the meninges of the brain. If such be the case, we can readily discern why we may succeed in procuring quiet sleep by opium externally applied, when, internally administered, it might aggravate the symptoms we mean to remove. For, by applying opium to the skin, we diminish its sensibility, we produce a torpor of the extremities of the sentient nerves, by which a preternatural determination of nervous influence to the surface is prevented. This nervous influence being now determined inwards to parts much in need of it, the lubricating secretions become more copious, irritation is diminished, and sleep follows.

But it is not the secretions only which become more copious or more perfect in consequence of the morbid determination of nervous influence to the skin being put a stop to; for the patient awakes with an appetite for the food which before he loathed, proving that the gastric fluid hath become more copious or more perfect. The mahogany colour of his cheek is changed to a brighter red, proving that the change of the blood in the lungs hath become more perfect. His pulse, which was running towards an hundred and twenty, weak and irregular, is now regular and below an hundred, proving that the determination of nervous influence to the heart is now sufficient to regulate its action; although this difference in the action of the heart may, in some measure, proceed from the more perfect arterialization of the blood.

To understand why opium might aggravate the symptoms we intended it to remove, when given internally, I have only to refer you to what I have said of its *modus operandi* in the healthy subject. It determines nervous influence to the surface of the body. Now, the symptoms we wish to remove are owing in a great measure, to a morbid determination of nervous influence to the surface. If by the greatness of the dose we succeed in procuring sleep, it is far from being quiet or sound, and the patient awakes unrefreshed: nay, it is

proverbial that, in such cases, benefit seldom arises from sleep effected by opium. Insensibly have I been led to make the above and many other remarks not immediately hinging on my subject, yet, as I conceive they are not altogether out of place, I trust you may also be of that opinion.

I will now propose to you the following condensed hypothesis, which will at one glimpse give you my idea of the chain of actions which constitutes fever:

*Impaired energy and functional disorder of the nervous system, and, in consequence, prostration of strength and imperfect secretion generally; irritation from the imperfect products of secretion, and, in consequence, increased nervous energy and sense of heat; absorption of the imperfect products, and, in consequence, inordinate vascular action.*

The first link of the above, as you must remark, is also the first link of the Cullenian theory. I have employed the expression, "nervous system" instead of brain, and perhaps such alteration was unnecessary; but of that more hereafter. I have added "functional disorder," because, by doing so, I think I meet one objection which has been started, viz. that diminished energy of the brain is not necessarily followed by fever. I can readily suppose that the energy of the brain may be impaired to a degree even greater than that necessary to produce fever, without fever being the result; but this impairment must be so gradual as at first to effect the secretions but triflingly, and then, instead of fever, we have, according to circumstances, a disease to be found in the class Cachexia. I regret I can go no further with Cullen; for I think it is needless to call in the aid of an ideal power in the animal economy to perform indirectly that which we can conceive to be performed directly by a simple and natural process; a process which we often artificially imitate to effect the same end.

One of the functions of the nervous system being secretion, or rather (to make use of language which may not offend an anti-neurologist,) the brain having an influence over the secretory apparatus, I have noted as a consequence of impaired energy, imperfect secretion generally; and, as I have already said, it is owing to the imperfect state of the secretions throughout the body, that a hot stage follows the cold one. The irritation produced by these imperfect products is the cause of the reaction, and in the greater sensibility of the skin we have the reason why the reaction is first observable on the surface. If imperfect secretion generally be admitted as a consequence of impaired nervous energy, it follows that the imperfect products must not only irritate, as would foreign matter, the nervous extremities with which they come in contact, but also that they must be absorbed and enter the circulation likewise in the shape of foreign matter. And if these imperfect products be sufficient to irritate, and to produce inconvenience, as soon as they are separated from the blood, in the parts with which

they come in contact, it is but reasonable to conclude that, when absorbed, they will excite, by irritation, the heart and arteries to increased action.

It is impossible for me to foresee all the objections you may start to the above hypothesis, for, as the father is frequently the only one who is blind not only to the moral, but even to the physical, defects of his child, so I, aware of my partiality, must look to you to point to its failings: it fails not, however, in simplicity, and it speaks neither of unmeaning sympathies, of morbid associations, nor of imagined *vires medicatrices naturæ*.

As I am one of those who believe that all fevers are produced by the same proximate cause, I meant here to show the adaptation of the above theory (if I may so name it) to some particular forms of fever; but, with a view of bringing this communication to an end, and upon the consideration that, if it explain the phenomena of the common continued fever of this country, it will be equally explanatory of the leading characters of all fevers, I will reserve what I have to say for some future epistle. Yet there is one form of fever which I cannot avoid adverting to in this place, inasmuch as it stands recorded to this day as an insuperable objection to the Cullenian theory; to the very stage which alone is correct, and which I have borrowed as the basis of my own. It is said that in symptomatic fever there are no symptoms of impaired energy of the brain. Be it so; and partly owing to that it is that I have said "nervous system" in my condensed view, instead of "brain," as written by Cullen. No acute inflammation can possibly happen in any part of the body, unless there be a preternatural determination of nervous influence to the part; consequently, there must be a deficiency of it in other parts of the body, and, owing to this deficiency, the secretions of such parts are imperfectly produced. These imperfect products at length irritate, are absorbed, and fever established. Thus, though this fever does not commence with symptoms of depression so marked as to constitute signs of impaired energy of the brain; yet I presume it will not be denied that there is disorder of the nervous system, consisting in impaired energy generally, the increased local determination being its remote cause. We have now a fever corresponding in its leading features to the stage of excitement in idiopathic fever. This fever is not of long continuance, for the absorbed offending matter, after a time, is determined to, and finds an outlet at, the wound or injured part, in the form of pus. The symptomatic fever I allude to is that which alone should be called so, viz. that which surgeons recognise as such.

The fevers said to be produced by the local affections called phlegmasiæ is, for the most part, if not always, an idiopathic fever. It is an inflammatory fever in which, owing to certain causes, an irregular distribution of nervous influence gives rise to topical inflammation. In the same way topical inflammation

obtains in typhus, but here we never think of calling the fever symptomatic.

## ERYSIPELAS.

*To the Editors of the London Medical Gazette.*

GENTLEMEN,—Some recent interesting discussions about Erysipelas, induce me to submit a sketch of my own experience and meditations upon that important subject. Wherever my doctrines or practice differ from the common routine, they have been sanctioned by mature pathological considerations, and attended with a satisfactory proportion of success.

The professional history of Erysipelas affords a remarkable example of the necessary alliance between physic and surgery; since it embraces questions of vital consequence, alike affecting the constitutional functions of the human frame, and particular parts.

Those physicians who assume to understand local and external diseases from general views of constitutional errors; or surgeons who undertake the charge of *apparently* local maladies, without a competent knowledge of their dependence on, or connexion with the whole living system, may be regarded as equally unfit to direct the treatment of Erysipelas. I take it for granted, that the majority of the faculty are agreed as to the constitutional source of Erysipelas; for every unprejudiced observer may easily detect the coincidence of a derangement in the animal system, whenever the outward demonstration of that disease becomes unquestionable. The numerous descriptions of Erysipelas, adopted by nosologists, are perhaps more curious than useful for practical purposes; while they inveigle the student into unprofitable technicalities, to the detriment of a better occupation, (*id est*) upon the evidence of conspiring symptoms.

The healing art is intricate because of its multitudinous facts, and complex rationale: its purposes are also often uncertain, from the difficulty of discriminating those signs of diseases which are infallible and decisive from such as are of subordinate value.

If the diagnosis of each disease could be accurately defined, and the specific remedies assuredly marked out, then the historical precedents of the art would, if faithfully displayed, suffice to make good empirical practitioners; but the records of twenty centuries exhibit the precepts and laws of medicine as unsettled as they were in the days of Hippocrates. It has, therefore, become expedient to attempt to establish this deeply responsible vocation upon scientific principles, by enlarging and spreading the anatomical knowledge of animal structures generally; and by chemical researches, as to the essential constituent elements of those organic compositions which are peculiarly placed within the dominion of life. Under a wisely-founded physiology, the surest systems of pathology

and therapeutics may be formed by legitimate inductions, showing the necessary dependencies between the mechanical structures, compositions, and functions of living bodies; the several errors happening in their actions, textures, or constituent materials; and finally, by philosophic reasoning, to point out the *modus operandi* of remedies, and a scientific way to further improvements.

After this preliminary exposition, I may venture, with more propriety, to address myself to professional scholars, and to those who desire to see the healing art promoted by a closer union with the exact sciences.

Erysipelas appears to be a humoral disease, arising from vitiated blood: it is occasionally epidemic, but more commonly sporadic; and even when seeming to be epidemic, the exciting cause may be often discovered to be some unwholesome food, beverage, or contaminated local atmosphere. Assuredly the common source of Erysipelas is improper diet, through which the blood is supplied with crude or noxious materials; and, although the attack is vulgarly ascribed to "*catching cold*," I am fully convinced that such excitement only gives activity to a previously existing morbid humour. Those violent eruptions, vulgarly, but not illogically, called surfeits, are the immediate consequences of one pernicious meal; and they generally display Erysipelas in its most malignant character; while moderate and habitual errors in diet mitigate the acuteness of the disease, and keep the patient in a state of continued liability to relapses, whenever exposed to any extraordinary vicissitude, and which thence assume either a mild or chronic type.

It must be allowed that Erysipelas is frequently first observed as a local affection, and apparently limited to the corion or true skin; but the eruption is always preceded by some derangement in the alimentary functions, and for the most part by loss of appetite, nausea, and constipation, followed by the other common signs of incipient fever. The devoted part first becomes hot, and feels as if influenced by the radiant heat of a fire; an itching or pricking sensation follows, and soon afterwards, under an aggravation of those symptoms, vesications arise, like those produced by artificial blistering. In this local inflammation there is seldom the same degree of redness or tumescence as in common inflammation. The colour of the serosity under the detached cuticle is the same as that of the serum of the blood in the affected individual; but if it happens to be more yellow than is natural, some practitioners then assign the chief cause of the disease to the liver, and hastily resort to mercurials. Perhaps all the varieties of Erysipelas ought to be ascribed to the several deviations in the acrimony, or in the quantity of the exuding morbid humour, and in some degree to the constitutional habit of the patient. If Erysipelas were essentially dependant on disease of the liver, it would necessarily be a common attendant on jaundice; but experience shows the contrary.

According to my observation, the disorders of the liver are rather the consequences of morbid states of the stomach than their antecedents or causes. In every chain of intricate events, it is of leading importance to discover the *constant* order of the phenomena, and in no profession is it more difficult, or of more portentous influence, than in the healing art. Having often, during a long professional life, detected an intimate connexion between Erysipelas and previous crudities in the stomach, (in many instances those were of the acid kind, traceable to acidifiable diet, such as oleaginous or tainted fish, raw vegetables, fruits, or sweets) I was thence induced to examine the serosity first effused in Erysipelatous vesications, and always found it to contain a free acid.

Those opinions have been supported by the testimony of several modern writers, who extol the efficacy of ammonia as a medicinal remedy in Erysipelas. Habits of pathological meditation, added to extensive experience, have lately induced me to think freely upon the nature of many diseases, and to recollect former evidences of the *juvantia* and the *lædientia* of practice. My views of Erysipelas, thus matured, are, that it is a humoral and constitutional inflammatory disease, occasioned by alimentary crudities; and because certain vegetable acids, and acidifiable viands, are often the notorious antecedents of the disease. I believe, as before stated, that the dominant error in the morbid fluids is acid. The causes which determine constitutional diseases to fall especially upon particular parts, are not well understood, but if we regard the corium as a compacted reticular tissue, analogous to the general cellular membrane, with only additional secretory or excretory vessels, we may class them together as serous membranes; and to those structures Erysipelas is chiefly directed. In its local habitudes Erysipelas is prone to excessive diffusion, owing to the absence of that adhesive boundary which limits common inflammation; and its remarkable tendency to sphacelate may be attributable to the destructive acrimony of the morbid humour, favoured by a constitutional debility, incident to the malady. From long continued opportunities of anatomical inspections, I am led to conclude that the frequent mortality attaching to Erysipelas must be ascribed to its untractable character, but I am also well assured that Erysipelatous inflammation more often attacks the great continuous serous membranes of internal cavities than the profession are taught to believe. To this, I fear, we may justly assign the many unexpected failures in the treatment of peritoneal and pleuritic inflammations, when they are wholly intrusted to sanguineous depletion. Those inflammatory affections which are purely vascular errors, united with plethora, may be always subdued by bleeding, purging, and abstinence; but not so if the whole system is vitiated by noxious fluids, derived from unwholesome diet, from imperfect digestion, or from undue retention of corrupted fæces.

After estimating the impure atmosphere of large cities, and the numerous unhealthy customs of their inhabitants, we need not wonder at the fatal results of many surgical operations, and of diseases in the metropolis, when compared with country practice.

Erysipelas may occur in either feeble or plethoric habits, and consequently demands a plan of treatment suited to the existing condition of the patient. I have, however, so often seen dangerous inflammation of the vein after bleeding in this disease, that I prefer to diminish any morbid excess in the volume of blood by cupping. When the attack has been sudden, and the stomach is known to be loaded with crude food, an emetic of ipecacuanha is advisable, and it should be immediately followed by a cleansing cathartic, given in reiterated doses until the lower bowels are thoroughly emptied. If the patient be of a full habit, and the pulse ample, abstinence from food and drink should be recommended, because the functions of the stomach are especially deteriorated in Erysipelas, and diluents promote all the alimentary fermentations. Mercurial purgatives are objectionable, because they produce putrescent excretions; and also, because of the uncertainty as to their injurious constitutional effects on certain individuals. Saline cathartics seldom cleanse the bowels effectually, and they mischievously augment the fluidity of the aliment.

The purgative which I ordinarily advise is composed of one drachm of powdered jalap, two scruples of sulphate of potash, and half a drachm of carbonate of soda, mixed with eight ounces of infusion of senna; of this two or three table-spoonsful should be taken every second or third hour, until copious and cleansing evacuations are procured. From a conviction that alimentary acidity is generally prevalent and requires to be subdued, I give from ten to sixteen grains of sub-carbonate of soda every four hours, in barley-water. If the mouth is parched, soda water proves very refreshing. Where great debility is obvious, the carbonate of ammonia, in doses of from five to eight grains, should be preferred to soda; and three or four grains of aromatic powder may be joined with each dose. The oily spices contained in the aromatic confection often prove offensive, and so do the distilled waters, impregnated with essential oils. Opium has been held in considerable repute as a remedy in Erysipelas, but the injudicious employment of that potent drug has seemed to me so often hurtful, that I consider its administration demands much circumspection, and that it is seldom proper.

Similar objections may be urged against the heedless prescription of wine, malt liquors, or venous spirits; and perhaps the notorious mortality among Erysipelatous patients may be justly imputed to a doctrine which excludes Erysipelas from the class of inflammatory diseases, and puts forth the mystical phantom of diminished vitality. The local treatment of Erysipelas is subordinate to the removal of the exciting cause, and to the

restoration of constitutional health; it is, however, the especial province of surgeons to watch its visible changes, and to make suitable applications. When hot excoriations have succeeded to broken vesications, simple lime water is a soothing remedy; it should be used through the medium of soft linen, and often renewed. When neither pain nor heat attend the eruption, a lotion composed of twenty grains of carbonate of ammonia, dissolved in eight ounces of water, may be employed in the place of lime water.

When drying crustations occur, the part may be beneficially covered with the thin smooth oiled silk, used by umbrella makers, and which will secure a moist skin, by preventing evaporation. If the subcutaneous tissue be invaded by the morbid secretion, and destruction is spreading through that extensive connecting medium, free incisions ought to be made in the most prominent or dependant parts, with the caution to avoid the exposure of the articular ligaments and tendons; and also under the prudent avoidance of extending wounds beyond the hope of healing, or which might add a new danger from their excessive length. Warm fomentations promote suppuration, but poultices are often more troublesome than useful, from their pressure. After sphacelus has become unquestionable, and putridity invades the part, we are happily now possessed of an effectual antiseptic, which I regard as one of the most valuable contributions of modern chemical science to the healing art;—I mean the chloride of lime. This estimable compound arrests the putrefactive fermentation; by which, not only the local condition of the mortified parts, but the general health and comfort of the patient, are essentially benefited.

Whether Erysipelas appears on the surface, or is suspected to have invaded internal parts, it is to be considered an insidious and dangerous disease, always requiring the watchful care and skill of the practitioner. If, however, a total reliance is placed on drugs, without directing the diet, little success will attend such limited endeavours. I have witnessed many fatal relapses of Erysipelas occasioned by the neglect or misdirection of diet; and I believe that the frequent returns of the disease are entirely attributable to improper diet. It is vexatiously absurd to see rows of alkaline draughts, and a beverage of lemonade, placed at the same time before a sick patient, and to hear that the doctor advised veal broth (which always turns sour on a weak stomach,) while the patient was labouring under a diseased acidity, for which the appropriate chemical antidotes were at the same time ordered. If I have been occasionally more fortunate than my brethren, in some dangerous cases, I am willing to assign the chief merit to a rational plan of diet, always carefully prescribed to correspond with the passing state of the patient, and with the medicinal remedies.

Gentlemen, I have thus far trespassed on your forbearance, with a recital from my per-

sonal experience and cogitations, without noticing those historical records which would have led to endless disputation, or involve this subject in unprofitable wrangling. The scope of medical science is hourly expanding, and an inquisitive public anxiously expect the establishment and exposition of its unmisgified rational practice. My utmost hope from this desultory sketch, is to awaken the exertions of medical philosophers, and, through them, to improve and spread the collateral and direct sciences of our art. If neighbouring nations have taken a temporary lead in physiology, let us avail ourselves of their industry and ingenuity, and unite our efforts to build up a system of pathology and of therapeutics, which may command the respect, and fix the confidence, of future ages.

Gentlemen,

Your obedient servant,

ANTHONY CARLISLE.

From the London Medical Repository and Review.

CASES ILLUSTRATING THE ASSISTANCE AFFORDED BY THE STETHOSCOPE in instances of *Doubtful Pregnancy*. By F. G. PROBART, M.D. F.L.S.; late Senior President of the Royal Medical Society of Edinburgh; one of the Physicians to the Suffolk County Hospital, at Bury St. Edmunds, &c.

*To the Editors of the London Medical Repository.*

Gentlemen—The interesting and important discovery of M. Kergaradec, of Paris, of the unequivocal signs furnished by means of auscultation in some cases of otherwise doubtful pregnancy, has hitherto attracted in this country but a small share of that attention which has been justly paid to stethoscopic investigations. Should you think the following illustrations of M. Kergaradec's discovery entitled to the honour of public perusal, I will beg the favour of you to dispose of them in any way you may judge best suited to the subject.

I am, Gentlemen,

Your most obedient servant,

F. G. P.

Bury St. Edmunds, April 11th, 1828.

CASE I.—A young woman, housemaid in a family of great respectability, applied to me for advice under the following circumstances. She had not menstruated for five months, having, she believed, taken cold the last time she was unwell. She complained of pain and swelling in the hypogastric region; her bowels were costive; she had no appetite, and felt general uneasiness. The hypogastrium, felt through the dress, was tense and unyielding, and the patient affirmed that very slight pressure occasioned her considerable pain. I suspected pregnancy; but as she negatived the possibility of it, some leeches were directed to be applied to the lower belly, a mild aperient, and rest. On the following day I was request-

ed by the mistress to visit the girl, and arriving at the moment the leeches were being applied, I availed myself of so excellent an opportunity for a stethoscopic examination. The "*bruit placentaire*" was instantly detected about midway between the umbilicus and superior anterior spinous process of the ilium; and between the former and pubis; the heart's contraction in the fœtus beating, in frequency, double that of the mother. I communicated this result of my examination to the girl, but she repeatedly, and most unequivocally, denied that she could be pregnant; at length, however, after remonstrating with her on the absurdity of vainly endeavouring to conceal that which a very short time must inevitably disclose, she acknowledged having "kept company" with a man-servant, in the house; and I was now permitted to acquaint her mistress with her real state. The girl left her place the following day, and was soon after delivered.

CASE II.—A married woman, from a neighbouring village, of emaciated appearance, the mother already of two children, consulted me on the state of her health, which another gentleman in the profession had represented as hopeless, from an opinion that she was "in a deep decline." There were considerable difficulty of breathing, some cough, occasional perspirations and debility. By means of the stethoscope, however, I found the lungs healthy, but the heart was beating with a vastly augmented sound, and audible all over the chest, as well posteriorly as anteriorly, but unaccompanied by any preternatural impulse. I concluded, therefore, that the above symptoms were the consequences of simple dilatation of the heart, and a suitable treatment was adopted, from which the poor woman derived so much relief, that she believed herself cured. She very soon again became alarmed at a gradual increase in the size of her belly, which her neighbours persuaded her arose from dropsy; and she, therefore, again presented herself to me. Being engaged at the time with other patients, I did not enter into a minute inquiry; and, taking it for granted that there was effusion, a natural sequela of the affection of the heart, I prescribed a combination of aperients and diuretics. Her size still increasing, she determined to pay me another visit; and, now doubting the accuracy of my former hasty diagnosis, as to the effusion, I auscultated the abdomen, and in a few minutes discovered the sounds of a fœtal circulation. Willing as this person evidently was to believe the information I now communicated, she could not be convinced of the fact, as she had not felt the motion of the child, nor had experienced any of those sensations that marked her former pregnancies. Before her confinement, however, which happened about ten weeks subsequently, she was satisfied of its truth, and had made the usual preparations.

CASE III.—A young woman was admitted under my care, into the Suffolk Hospital, supposed to have ovarian dropsy. In the hurry attendant upon admitting patients, I made no

examination at the moment, but desired the woman to remain in bed the following morning until I had made my visit, with a view to more accurate observation. The account given of herself was, that she had suffered with her present complaints for two years, during which time she had been severally treated by a physician and two surgeons. There was apparent tenderness in the hypogastric region, upon slight pressure; and a spherical tumour was very palpable, extending from the pubis to the umbilicus, which so very nearly resembled the pregnant uterus about the seventh month, that I, without hesitation, put the question as to the possibility of pregnancy. The girl gave me the most solemn assurances it could not be, and said that, in consequence of her appearance, she had more than once in the course of the last two years been so charged by her neighbours; and that so lately as only a fortnight ago, this report having again spread through the village, she, for the satisfaction of her mother and friends, submitted to an examination by the medical gentlemen who attended the parish, and who then declared that she was not pregnant. Still, however, very doubtful of all this, I had recourse to the "*Experimentum crucis*," when my suspicions were immediately realized in the existence of a loud "*bruit placentaire*," with that still less equivocal sign, the action of a fœtal heart. The girl, notwithstanding the positive manner in which I now insisted upon her situation, persevered for a long time in affirming her innocence; but ultimately she acknowledged "an acquaintance" with a young man in the village: she was nevertheless sceptical on the score of pregnancy, as she continued to feel exactly as she had done for two years back. Her mother called at my house the next day, before going to see her daughter at the hospital, to hear my opinion of the latter, and manifested a disposition to quarrel with me on hearing the unwelcome tidings I communicated. The patient returned home with her mother, and was, a month after, delivered of a still-born child.

There cannot, I think, be any doubt that the subject of this example was ignorant of her situation; for it is quite true that she had been ailing for two years; that she had menstruated but once during that time, which event appears to have happened a short time before the "acquaintance," and that the apothecary had declared her case not to be one of pregnancy.

From the *Lancet*.

A CASE WHERE THE RIGHT CAROTID ARTERY WAS SUCCESSFULLY TIED, on the distal side of a large Aneurismal Tumour. By GEORGE BUSHE, M.D. of the Royal College of Surgeons in Ireland, and Assistant Surgeon to the Forces.\*

Mary Covis, æt. 36, in March, 1826, observ-

\* A short account of this case is contained in the *Journal of Foreign Medicine*, Vol. i. p. 381.

ed a pulsating tumour about the size of a small hen's egg, in the upper part of the lower third of the right side of the neck, corresponding to the situation of the carotid artery, which, to the present period, (Sept. 2d, 1827) has progressively enlarged. A prominent tumour now extends from the clavicle on the right side, upwards to near the os hyoides, pressing on the trachea, and bearing it off to the opposite side, and passing under the sterno-mastoid muscle (which is stretched over it) to nearly an inch beyond its outer border. The integuments of this side of the neck, particularly in front of the tumour, are thin, shining, and loaded with varicose veins. The tumour is firm at its circumference, but soft in its centre, and, by pressure, its size can be diminished; it pulsates strongly, the throbs being synchronous with those of the left ventricle of the heart. The stethoscope indicates it to be purely aneurismal, and, by this instrument, the left side of the heart appears to be acting on an increased quantity of blood, with more force than natural. The poor woman is much emaciated, and her deglutition and respiration, which have been much impaired for some months, are now almost annihilated.

6. Dr. Clarke, physician to the forces; staff-surgeons Burton, Clarke, and Daun, with other officers of the medical staff, and most of the neighbouring practitioners, assembled this day; and, after considering the case, were of opinion that an operation would be fruitless, in consequence of the advanced stage of the disease, and moreover, from the magnitude of the tumour, it would be impossible to find the carotid artery.

11. This morning, my medical friends wrote to me saying, "That as Covis had not swallowed any thing for the last nine days, her respiration was becoming more alarming, and her voice had almost failed her, added to her own supplications and the entreaties of her friends, they wished that I might now attempt to secure the carotid on the distal side of the aneurism." Accordingly, at three o'clock, P. M. I proceeded as follows:—The patient being placed on a table, with her head and shoulders slightly elevated, I endeavoured to gain as much space as possible, by bending her neck to the opposite side; but this I found totally impracticable, from the dreadful suffering and sense of immediate suffocation she experienced on the slightest attempt to accomplish this end; therefore, with her head rather inclined to the diseased side, I commenced an incision at the angle of the jaw, (which was very prominent,) and carried it obliquely downwards and forwards for two inches, in a direction corresponding to the anterior edge of the sterno-mastoid muscle; by which the external jugular vein was exposed, crossing the centre of the wound; and fearing troublesome hæmorrhage from its division, I included it within two ligatures, and then cut between them; the cervical fascia and platysma being dissected off, the anterior edge of the sterno-mastoid was readily brought into view, by the side of which lay a tortuous

plexus of dilated veins; these being pushed off with the handle of the knife, I endeavoured to divaricate the lips of the wound with brass retractors, but from the tense state of the sterno-mastoid muscle, I found it almost impossible; and the slightest attempt produced much pain and more laboured respiration, with swelling of the cervical veins, and, from these difficulties, added to the great depth at which the carotid lay, in consequence of the magnitude of the tumour forcing all the parts out of their natural situation, I found it necessary to divide the integuments on the upper part of the tumour to the extent of an inch, and by this expedient I was enabled to lacerate the cellular tissue in the neighbourhood of the hyoides, which bone I at length reached, and found that it was situated between one and a half and two inches from the surface; then, by a little manipulation, I succeeded in pushing the tumour somewhat downwards, and to the opposite side, thus exposing the sheath of the vessels, which, when I had removed the descenders noni to the outer side, was opened in the usual manner; but, as far as I could judge in the deep and bloody cavity, with the great impediment of an enormous internal jugular vein. The portion of the artery immediately above the aneurism was dilated, and not more than half an inch of its extremity appeared sound, on which, with a common silver probe, I placed a single silk ligature. The wound was lightly dressed, and the patient put to bed, not having been more than fifteen minutes on the table. Before I tied the ligature, I allowed many of the gentlemen present to examine if the artery was really included; as I feared a difference of opinion might exist as to my veracity, a short time before having read in the *Lancet* a lecture of Mr. Liston's, in which he scouted the idea of Mr. Wardrop's ever having placed a ligature on the carotid, beyond an aneurismal sac.

Immediately on the tightening of the ligature, Dr. Clark observed to me, that the tumour was more soft and less prominent than before the operation; and this, to those who heard the observation, and myself, was obvious enough; but, if I recollect well, a similar change was observed in the case operated on by Mr. Lambert. In the commencement of the case it may be read, that the poor woman had not swallowed any thing for nine days; that her respiration was particularly distressing, and that her voice had almost failed her; but the reader will not be a little surprised when I say, that before the wound was dressed, she swallowed nearly ten ounces of wine and water, and such was her relief, that she actually thanked me for the benefit I had conferred on her, and, if allowed, would have prolonged the conversation.

So far I have copied verbatim from notes taken as the circumstances occurred; but the operation being safely executed, a continuation of the details, after this manner, can be no more interesting; and as long narrations never fail to exhaust the most studious, I have thought proper to condense the remainder of

this case. Until the 14th day she required four small bleedings, to keep down arterial action, and ease pain of the right side of the heart; gentle enemata were employed to regulate her bowels; her respiration and deglutition rapidly improved; her food consisted of strong beef tea and jelly, and she had easy nights, without the slightest tendency to orthopnoea, from which she had suffered much previous to the operation; the tumour rapidly decreased, and its pulsations became more weak. The ligature was cast off on the 19th day,\* and the wound was healed on the 27th, at which period she left her bed, the tumour being reduced to one half its bulk, and almost free from pulsation.

At the present time, April 19th, she is in perfect health; there is scarcely a remnant of the tumour, the inordinate action of the heart has ceased, and her respiration and deglutition are natural.

I shall now conclude this brief paper with a few concise observations which perhaps may not be useless. Dilatation of the vessel may have been the first morbid change in the above case; but when I saw the woman, there was a sac communicating with the artery, as described by Scarpa. The carotid was undoubtedly secured on the distal side of the tumour; and I never tied, or saw an artery tied, on the cardiac side of an aneurism, where the cure was more perfect. I have not seen any of Mr. Wardrop's cases, nor that of Mr. Lambert, but I have read of them all in the *Lancet*; and I feel convinced that the Editor of that periodical has performed a public good, in recommending the ligature of arteries on the distal side of aneurisms, in cases where it is impossible to follow the precepts of J. Hunter. For the last six months I have kept this case from the public eye, that I might speak firmly and without contradiction as to its termination; and though it is foreign to my occupation in life, and my desire also, to litigate on any subject, professional or otherwise, yet I cannot refrain from stating that, in my opinion, Dr. James Johnson has not acted judiciously in ridiculing a foreign journalist because he had offered praise to English surgeons for the advancement they had made in this department of their art; and if heretofore Dr. Johnson could find no successful operation on the principle of Deschamps, let him profit by this, and in future learn to be more guarded when

\* This period will no doubt appear long; but when I secure vessels for aneurism, I never employ the force recommended by the late Dr. Jones, as I am convinced the division of the two inner tunics is not necessary for the effusion of a sufficient quantity of lymph to become the medium of adhesion; and I have but little doubt, that if surgeons would use less force on the application of these ligatures, secondary hæmorrhage would be a less uncommon consequence of operations for aneurisms. The period is not far distant when I may submit this subject more fully to the public.

criticising matters of serious importance to the advancement of professional knowledge and the well-being of mankind.

As every fact that can tend to prove or disprove an unsettled point, must be considered more or less valuable, it may not be useless to subjoin, as an appendix to this case, a concise account of a patient who, in the summer of 1823, was admitted into the Whitworth Medical Hospital, Dublin, under the care of Dr. Cuming. His complaint, on admission, was registered as paralysis of the right arm; but a large tumour being discovered in the axilla, the late Professor Todd was called to see the patient; and, after careful examination, he gave it as his opinion that it was an aneurism, which, from its magnitude, had lost its pulsation; but, to settle the point, he punctured it, and florid blood followed the insertion of a probe. When passed inwards for about three inches, the nature of the case being decided, the man was removed to the Richmond Surgical Hospital, where, from an attack of erysipelas consequent on the puncture, he died in a few days. Mr. Todd requested that I might examine the body, and, from notes made after the dissection, I abstract the following:—"The aneurism, which was of large size, occupied the axilla; the sac, in many places, was almost absorbed, and adhered firmly to the upper and outer part of this cavity; when opened, it contained large quantities of laminated fibrine, and, in its centre, was a cavity holding about eight ounces of coagulated blood; communicating with the cavity, there was an opening

of this size  in the axillary artery,

below which the vessel was obliterated for the space of half an inch, corresponding to the situation where the sac so firmly adhered." Here, then, was a case where the aneurism was undergoing a spontaneous cure; in consequence of the pressure of the tumour having obliterated the artery on its distal side; and I look upon it as a valuable fact, towards confirming the utility of reviving the operation of Deschamps; and so much was I impressed with this opinion, that before Mr. Wardrop published his first essay, I recommended the operation in a case of large carotid aneurism, in a public hospital; but my chance was to be laughed at. However, when I again meet the two surgeons who so wantonly ridiculed me, it will be my turn to laugh at them.

From the London Medical Gazette.

#### VACCINATION.

NO. III.

To the Editor of the London Medical Gazette.

SIR—One of the most popular explanations of the occurrence of small-pox subsequent to vaccination, is that which attributes it to deterioration of the virus—the natural result of its passing through the bodies of so many individuals; and the obvious remedy for the evil, therefore, is more frequent recourse to

the cow. This doctrine has been repeated over and over again, until, by mere habit, it has come into general notice, making its impression on the public mind, *non vi, sed sæpe cadendo*. It is time that the truth or falsehood of this statement should be ascertained; and it was originally my intention to have made this the first subject of inquiry in the present communication, but the appearance in your columns of a letter signed M. D. obliges me to clear the ground by a few preliminary remarks.

I cannot bring myself to believe that that letter expresses the general opinions of the profession on the subject which it undertakes to discuss; but the notice which you have thought proper to take of it, has given it an importance to which *per se* it is scarcely entitled. The writer is pleased to say, that my single remark on the theory of spurious cow-pox (occupying exactly three lines of your small columns,) exhibits a *strain of sentiment* calculated to do much mischief; and he inquires, with great formality, whether I believe in an imperfect cow-pox, when the self-same letter (No. 1,) concludes with an expression of my intention to inquire into the probable sources of *imperfection in the vaccine process*.

The remaining questions which the writer of that letter has put, show that he has only thought very superficially on the subject which he discusses; for he mixes up many doubtful, and some quite inaccurate statements, with others that are clear and undeniable. I shall take the liberty of offering a few comments on these questions, the more willingly, as the discussions to which they will lead fall in with some which the undisturbed course of my argument would have naturally suggested.

I cannot avoid entertaining the suspicion that your correspondent, M. D., has never hunted sufficiently through the older writers to know what they meant by spurious cow-pox; for he talks of it as not affording the *full* amount of protection. Dr. Jenner defined that to be a spurious cow-pox "which is incapable of producing any specific change in the constitution, but which leaves it as susceptible of the small-pox as any other common cutaneous disorder." The original notions on this subject were, that there are three diseases of the cow's udder and of the horse's hoof, which have been indiscriminately termed cow-pox; but that only one of these three is the real preventive of small-pox. The other two were called *spurious* cow-pox; but it was confidently maintained that these two spurious disorders were capable of being continued by successive inoculations,—frequently showed an *exact similarity* in many of their appearances to the true species,—and that it required the discrimination of the exercised practitioner to distinguish the one from the other.\* This was the original doctrine

of a spurious cow-pox; and I repeat what I said before, that such an idea no longer disturbs our minds; and that the doctrine, as thus announced, was, I firmly believe, a mere *phantom*. It will be observed, that the term *spurious*, as originally applied to cow-pox, had reference to the *primary* source of the pock, which was bad. I am well aware that the phrase was at one period employed (even by Dr. Jenner himself) to express the altered condition of a *genuine* cow-pox; but the term *degenerated* cow-pox was afterwards substituted, which, in the lapse of time, gave way to those which are now in common use, viz. *irregular* or *imperfect* cow-pox, which are always understood to mean modifications of a pock *originally perfect*. This important distinction between a pock originally bad, and one rendered imperfect by accidental circumstances, though obviously known to your correspondent, is not clearly stated by him.

In his desultory mode of treating the subject, he next adverts to two points in the doctrine of vaccination, which he takes for granted as being true, though they are certainly open to much doubt, and, as far as my present experience goes, are actually contrary to the fact. These are, first, the question whether genuine cow-pox can ever pass through a system so as to excite a local affection, but to afford no constitutional security whatever; and, secondly, whether such an irregular or *imperfect* pock is capable of being perpetuated by inoculation, so as not to afford the due measure of security against the small-pox. Your correspondent evidently means to decide both questions in the affirmative, though the obscurity attaching to his use of the term *spurious* gives him a loop-hole at which he will perhaps desire to escape. On the first of these questions I offered an opinion in my last letter. My belief is, that however imperfect the pock may be, provided some areola be formed, a certain portion of protective influence is imparted to the constitution, though that be slight, and probably temporary. This, however, is a point still open to discussion. Dr. Jenner held, at one time, (I do not know if he continued to maintain the doctrine) that virus taken from a true vesicle at a very late period, produced an imperfect disease; and your correspondent probably alludes to this when he asks, "whether it be of moment that the virus be employed in its active or efficient condition?" The facts, are, I believe, these: after the tenth and eleventh days, the virus becomes so *diluted*, that it is extremely difficult to reproduce the disease by it. Out of a dozen incisions made with such lymph, not more than one or two will prove effective; but that one is just as good, and just as effectual in preserving against the small-pox, as lymph of the seventh or eighth days. The unanswerable argument in favour of this position is, that the *scabs* of cow-pox, moistened with a little lukewarm water, will produce the disease in all its purity; but out of twenty or

\* See "Address to the Public," by H. W. Jenner, 1799. Pages 9, 12, and 13.

thirty incisions made with such a virus, not more than *one* will be found to take effect.

Again, your correspondent asks, with a sort of triumph, "have I forgotten that an irregular or imperfect pock, when excited, is capable of being *perpetuated* by inoculation?" If he means to speak of an imperfect pock, the offspring of a *perfect* one, my reply is, that I have not forgotten it, because I never knew it. In fact, I know that the direct reverse is true; and that, as the modified small-pox will produce in the unprotected the true (perhaps confluent) small pox, so will cow-pox, degenerated by some peculiarity in the habit of an individual, reappear when transplanted into a healthy, well predisposed subject, in all its original purity and perfection. I have proved this in numerous cases at the Small-Pox Hospital, and it is an important practical fact, of which, if your correspondent should doubt, he may, at any time, with perfect safety to the individual, convince himself by actual experiment.

And this brings me, at length, to the question of recurrence to the cow, the point from which I diverged in pursuit of your correspondent M. D. It is insinuated by many, that we should recur for fresh supplies of lymph to the cow, because the virus necessarily degenerates by passing successively through so many human bodies. I deny this assumption *in toto*. So far from seeing any evidence of degeneration, I am sure that the lymph with which I vaccinated many children on Monday last, was as perfect as the best lymph of 1799. The elevation and pearl-like colour of the vesicles on the ninth day, the extent and circular shape of the areola, the regularity in the stages of the disease, and the colour and form of the resulting scab observable in the cow-pox of the present day, are such as leave nothing to desire. I have already expressed my firm conviction, that by the careful selection of healthy and well predisposed children, the pock may be restored from an imperfect to a perfect state; and it naturally follows, that by a similar care, it may be kept up for any length of time in that desirable condition. But a question of great practical moment meets us here. What is the proportion actually found to exist, in practice, between the perfect and imperfect pocks? for this naturally leads to the inquiry, what is the minimum of subjects on whom you can operate, so as to ensure at all times lymph in a state of perfect activity? I have no experience of the fact; but from all I have heard and read, I cannot doubt that if three or four children are *successively* vaccinated from each other, *all* of whom are, from various causes, ill-disposed to take the disease, the matter degenerates, and at length wears out altogether. A good illustration of this doctrine may be found in a letter from Mr. Fergusson, on the state of cow-pox at Sierra Leone.\* I have

heard it calculated, that even supposing the subjects to be well selected, one out of every five vaccinations fails the first time; but as the constitutional disposition to receive the cow-pox kindly can never be predicted with perfect precision, it follows, that in order to secure a permanent supply of genuine efficient cow-pox, two or three children at least must be vaccinated at the same time; and when we further take into account the certainty of occasional failure in the operation, the chance of the child's being attacked by some other disease, the great probability of inattention or obstinacy on the part of some parents, it may safely be concluded, that to ensure a steady supply of recent and perfect lymph all the year round, 500 children at the least must be vaccinated annually. This I am inclined to consider as the minimum of vaccinations at which any establishment can secure its own supply of perfect fresh lymph at all times.

But to return to the consideration of that popular question—how far it is proper, and even necessary, to have occasional recourse to the cow, and to allow the present sources of vaccine lymph to die out? To my mind many powerful arguments suggest themselves against the adoption of such a measure. 1. It is by no means easy to find the true cow-pox even in a large dairy. I have been given to understand by those most conversant with the subject, that a twelvemonth often elapses without its being seen. 2. There must be always some doubt as to the purity or genuineness of the new stock, until the experiment of variolous inoculation has been subsequently made; which parents, who subject their children to vaccination, are very seldom disposed to allow. 3. The true vaccine lymph, as first taken from the cow, is frequently of a more acrid nature than that which has been assimilated to the human constitution by frequent successive inoculations, and consequently the first trials are likely to produce glandular swellings and other inconveniences, and thus occasion distrust rather than increased confidence. Fourthly, and lastly, it is not found that the cases of small-pox, after vaccination, are comparatively more frequent among persons recently vaccinated. It is true that we hear of such cases much more frequently than we did ten or twenty years ago; but common experience will bear me out in saying, that the occurrences of which I speak are principally met with among persons vaccinated from fourteen to four-and-twenty years ago. When we do meet with children under ten years of age affected by small-pox, subsequent to vaccination, the disease is, for the most part, very mild, scarcely deserving a higher title than that of varicella.

These are the objections I have to offer against the proposal to revert frequently to the cow. It appears to me not only uncalled for by the circumstances of the times, but in some respects hazardous; nor am I convinced that it would tend, in any degree whatever, to diminish those unpleasant occurrences which are now bringing vaccination into some dis-

\* Journal of Foreign Medicine. Vol. 1. p. 494.

credit. On the contrary, it appears to me that a very different train of measures must be resorted to, to meet the exigencies of the case. Instead of seeking for new and stronger lymph, that which we have must be diligently fostered and encouraged; and the supplies of it rendered as copious, as perfect, and as easily accessible as possible. These I believe to be the real and efficient remedies for the evil now so generally admitted; and it will be the object of my next (and concluding) communication, to show what these measures are, and how they may, most advantageously to the public, be carried into effect.

I have the honour to be, Sir,

Your very obedient servant,

GEORGE GREGORY.

From the Medico-Chirurgical Review.

**THE MORBID ANATOMY OF THE BRAIN.** By ALEXANDER MONRO, M. D., &c. &c. Vol. I. *Hydrocephalus*. 8vo. pp. 200, with 5 coloured plates. Edinburgh and London, 1827.

Whether we reflect on the mortality occasioned by hydrocephalus, or on the learning and opportunities for observation of the author, we cannot but conclude, that the volume under review will attract considerable attention. Dr. Coindet has stated that twenty thousand deaths annually result from hydrocephalus in France—while Dr. Alison informs us that 40 out of 120 patients die of this disease in the New Toyn Dispensary. According to the late Dr. Davis, of London, 8 out of 45 deaths, in the Universal Dispensary, were produced by hydrocephalus.

In respect to the author, it appears that it is now more than twenty-five years since he began to direct his attention to organic disorders of the brain—but, after long observation and patient investigation, he had nearly given up the undertaking, from the difficulties of the subject, when the death of a relation, by a species of hydrocephalus, to him quite novel, gave a new impulse to his researches, and enabled him to accumulate the mass of facts herewith presented to the public. We shall do most justice to the author, and confer most benefit on our readers, by confining ourselves, in this article, almost entirely to the task of ANALYSIS, since the facts are already in a very concentrated state, as well as in very great number.

The work is divided into two chapters—one containing the morbid anatomy—the other the symptomatology, &c. of hydrocephalus. We shall prefer reversing the order which Dr. Monro has chosen, and take his second chapter first. This second chapter, indeed, of itself, offers ample scope for an analytical article. We shall follow the sections seriatim.

**SECT. I.—HYDROCEPHALUS, IN WHICH THE SKULL RETAINS ITS USUAL SIZE AND FORM.**

Of this, there are two very different species—the one decided in its character at the outset; extremely rapid in its progress, proving

fatal in three, four, or five days; very rare in its occurrence, and, according to our author, undescribed by any other writer. The other species is obscure in its origin; slower in its progress, (being of three or four weeks' duration;) very frequent in its occurrence, and described by many writers.

*Most Acute Species.*

"This rare form of the disease is very sudden in its attack. There is no previous head-ach, drowsiness, stupor, nausea, vomiting, paralytic state of any part of the body, or any one symptom denoting a derangement in the functions of the nervous system.

"It begins like the croup. The child awakes in the night in a state of extreme agitation, and much flushed, and with a quick pulse; he is hoarse, and the sound of the voice when he inspires is similar to that in croup,—the sound seems to come from a brazen tube, which is contracted at a certain part.

"Children who are stout and healthy, are equally liable to this disorder as the feeble and emaciated. And I have seen a patient, on the very day he was attacked by this disorder, who seemed very cheerful, and took his meals well, and was to all appearance in the most perfect health.

"The giving an emetic relieves the breathing, and, upon examining what has been rejected by vomiting, it is found to be evidently undigested."\*

Some cases are given by Dr. M. in illustration of this very fatal disease.

*Case.* At half past ten o'clock, P. M. (Sunday) a stout boy, 20 months old, awoke suddenly from his sleep, (which had been apparently tranquil) as in a fright, and coughed violently, with a croupy sound, his breathing being difficult and quick. He seemed feverish—was thirsty—unwilling to lie down—pulse 150 in the minute, and rather hard. He had been in apparently perfect health during the preceding day, which was intensely cold. He had had two natural motions, and ate his food heartily. Under the impression that the disease was common inflammatory croup, an antimonial emetic was administered, which operated mildly, and brought up some ropy

\* "When this sheet was about to be sent to press, I learned from Professor Burns, that he had described, in his Principles of Midwifery, this very acute form of hydrocephalus, which he attributes to an affection of the origin of the eighth pair of nerves, induced by the state of the extremity of the fifth in dentition acting on its origin, which is near the eighth. The recurrent seems more immediately in fault, producing a temporary paralysis of the muscles of the glottis rather than a spasm. It by no means ends necessarily in hydrocephalus, but it sometimes does, as, after the child has been apparently well for weeks or months, he is carried off by hydrocephalus, which change is first indicated by general convulsions. Few children recover when the original attack is accompanied with convulsions, yet the case is not altogether hopeless."

mucus, bile, and *undigested* turnip and carrot, eaten the preceding day. Mr. Bryce and Dr. M. saw him a quarter before 12, when an ipecacuan emetic was exhibited, and afterwards the semi-cupium. When the vomiting subsided, three grains of calomel were given, and the child fell into a sleep at 4 the next morning, when a large blister was applied to the larynx, but slipped down on the sternum. At 8 A. M. (Monday) a dose of castor-oil was given to work of the calomel. Three offensive stools, one of them green, were discharged. At 9 o'clock that morning, Dr. M. and Mr. Bryce found the child more lively, his breathing less frequent, and his voice stronger. He now sang and laughed, but his voice was still husky. At 4 o'clock that day, feverish symptoms came on, and his breathing was more affected. Tepid lavations and a dose of calomel were prescribed, and he passed a tolerable night. *Tuesday morning* found the little patient in nearly the same state; but in the evening there was another exacerbation of fever, cough, and difficulty of expectoration. At 10 P. M. the pulse was 200, and very hard and sharp. He breathed rather like an asthmatic than a croupy patient—countenance much flushed, and indicative of great distress. Mr. Bryce examined the child's mouth, and found the gums much swelled and inflamed, and the dentes caninæ, on each side, about to cut the gum. The gums were incised freely, and there was much discharge of blood. The child now started much more frequently than before. Five leeches were applied to one of the legs, and much blood was evacuated by means of immersion in warm water. Three grains of calomel were given, and procured a large healthy evacuation. We need not pursue the details any farther. The child died the next day (Wednesday) at 5 P. M. The dissection is very interesting, and we shall give it in the words of the author.

*"Dissection twelve hours after death.* The cavity of the abdomen being first examined, the intestines were found in their usual situation, of a white colour, and moderately distended with air. The stomach, which was much contracted, lay in the left hypochondriac region, exhibited a blanched appearance; and, when slit open at the lesser curvature, it was found empty, and its mucous membrane was uniformly healthy, and the rugæ were very distinct. The mesenteric glands were of their natural colour, size, and consistence, and all the abdominal bowels. The large blood vessels also were almost empty.

"All the bowels of the thorax were sound.

"The veins on the fore part of the neck were quite empty; the trachea externally was of a silvery whiteness. An incision was made from the upper part of the thyroid cartilage into the division of the wind-pipe; the mucous membrane lining the larynx was of a white colour, and in every respect healthy. There was no appearance of the croupy membrane, no thickening, no ulceration, nor any mark of preceding inflammation. The trachea and branches of the wind-pipe, as far as they

could be traced, were also healthy and free of mucus. The fauces and pharynx were also sound.

"*The brain was large, firm, and healthy;* and the veins entering the sinuses were distended with dark blood. *The upper surface of the brain, particularly the superior part of the posterior lobes, was covered with a transparent gelatinous effusion. On opening the ventricles, about an ounce of colourless serum was discovered, which had raised the fornix considerably.* The medulla oblongata and tuber annulare were found floating on a great quantity of clear serum. The veins covering the tuber annulare and medulla oblongata were distended with blood, so that they exhibited a deep scarlet colour. All the nerves at their origin were sound, *except the fifth and eighth pairs, which were also of a deep scarlet colour, and covered with turgid vessels.*

"*On removing the brain, by cutting through the medulla oblongata, a considerable quantity of serum rushed from the upper part of the spinal canal.*

"The vessels of the spinal marrow were turgid, those at the cervical portion of a vermillion-red colour, and those of the lumbar portion of a dark red hue. *The eighth pair of nerves was of a deep uniform red colour along its whole tract, as far as its branches going to the lungs.*"

The author proceeds to the particulars of some other cases of this formidable disease, which is not confined to the infantile state. Thus, a woman, aged 65 years, was subject to spasms in her stomach, and obstinate constipation. She died very suddenly. On dissection no morbid appearances could be found in the stomach or intestines. "There was considerable effusion of a watery liquor, and also distention of the vessels of the pia mater over the eighth pair of nerves within the membranes, and at the base of the brain." The following case occurred in the practice of Drs. Monro and Saunders.

"J. B., æt. eight months, a plump stout child, became remarkably fretful, and would take no kind of food. Next day he was seized with slight cough and hoarseness, which gradually increased, and also with difficulty in swallowing; so that a part of his food, upon his endeavouring to swallow, was rejected through his nose. Pulse quick.

"On the following day, the symptoms had materially abated, especially the cough, and he expectorated a good deal of viscid phlegm.

"During the night, and still more during next day, he became much more uneasy; his breathing became more oppressed; and the voice was so hoarse and shrill, that several old women who saw him, said *he had the croup*. There was great heaving and agitation of the chest.

"On the subsequent day, the breathing was very laborious, and much hurried; pulse very rapid, and between 120 and 130 in the minute; and next day he died.

"*Appearances on Dissection.*—Skull very vascular. There was a slight effusion of

serous fluid between the arachnoid coat and pia mater; and about  $\frac{3}{4}$  of a similar fluid was contained within the ventricles of the brain. *The vessels of the pia mater at the corpora quadrigemina and tractus optici, and at the origin of the eighth pair of nerves, were much distended with blood.*

"Lungs quite sound.

"No morbid appearance was discovered in the larynx and trachea. A small quantity of mucus, which had more of a greenish hue than in the sound state, ran out on opening the windpipe, and there was some frothy mucus within the smaller branches of that tube.

"The only morbid appearance in the abdomen was a very considerable enlargement of the mesenteric glands.

"The symptoms in the preceding cases corresponded in my opinion with the phenomena which occur upon irritating the eighth pair of nerves of the inferior animals.

"The croaking sound of the voice, and the difficulty in breathing, were probably to be imputed to the effects of the irritation, to which the laryngeal nerves had been exposed, for the stretching or dividing these nerves in a living animal produces the same effects.

"The functions of the lungs and stomach are suspended; from the irritation applied to the eighth pair of nerves, which also happens when these nerves have been divided. If the eighth pair of nerves of an animal be divided, and if that animal be allowed to take food soon after the division of the nerves has been made, vomiting in a short time ensues. Precisely the same took place in the child above described,—and the food is found to be unchanged,—which also happens in this form of hydrocephalus. In the case above mentioned, the vegetable substances which the child had taken in broth, *were indigested, though they had been within the stomach for twenty hours.*

"In the child's case detailed in page 70, &c. the difficulty in breathing gradually increased, and the face became purple towards the conclusion of the disorder; and, upon examination after death, the air tubes and cells of the lungs were found filled with a frothy fluid; and hence the lungs did not collapse. All which circumstances take place when the eighth pair of nerves of an animal have been divided.

"In the cases above detailed, the difficulty in breathing was much increased by the effort of vomiting; and the same happens when the eighth pair of nerves of an animal have been divided."

Another case is quoted from the work of Mr. Swan on the nervous system, which bears considerable analogy to the above.

#### *Diagnosis of this Hyper-acute Form.*

It is of some importance to be able to distinguish this dangerous modification of hydrocephalus from the more common forms of croup. It is not, according to our author, dependent on the state of the weather, the locality of residence, or any of the predisposing causes of croup; "but is rather connected

with the period of teething, and nervous irritation." The following are the more remarkable distinctions between this disease and the common croup.

"The patient, at the outset of the disease, seems in a state of nervous irritation, often starts in his sleep, and in a short time the disease assumes the appearance rather of a spasmodic affection of the larynx, than of the inflammatory croup; but there is not so much wheezing, and the disorder is not mitigated by the expectoration of ropy mucus, or by vomiting; and what is discharged by vomiting may be observed to be indigested.

"The purple colour of the face is a striking feature of the earlier part of the croup, but it is not obvious in this disease until twelve or fourteen hours before death, and is probably owing, as in moribund animals, to a weakness of the muscles of inspiration.

"The longer the duration of this disease, the less shrill and hoarse the voice becomes; whereas in the common croup the contrary takes place.

"Coagulable lymph sometimes forms a distinct tube within the larynx and windpipe in the advanced stage of common croup, and may be perceived moving within it, which never happens in this disorder.

"This disorder may be readily distinguished from inflammation of the lungs;—there is no cough, or pain in any part of the side or breast, which is increased by inspiration;—in proof of which, the first mentioned patient repeatedly sang during the progress of the disorder, and as loudly as when he was in the most perfect health. This form of hydrocephalus may be distinguished from the more common form of the disease; as the sight is not impaired, and there was no dilatation of the pupils, and little or no delirium.

"Lastly, when the voice becomes weak or shrill, from any cause compressing the windpipe,—as by matter collected behind it,—by diseases of the bronchial or thyroid glands,—by aneurism of the aorta, or carotid artery,—or by any other cause compressing and straitening the windpipe, the alteration from the natural sound of the voice is permanent; whereas, from this disorder it is but temporary."

The method of treatment may be soon despatched. The rapidity of the disease, and the seat of the effused fluid, leave little to be done by art. The exhibition of a large dose of calomel, and the application of numerous leeches to the head, with a blister to the nape of the neck, at the very commencement of the attack, seem to Dr. Monro the most proper remedies.

We now proceed to the more common form of the disease.

#### ACUTE HYDROCEPHALUS.

It must be confessed that there is not one pathognomonic symptom of a collection of water within the brain. The characters of the disease are deduced rather from a chain or combination of symptoms—and hence the

nature of the disease remains often undetected until it is beyond the reach of art, unless the most scrupulous attention be paid to the most minute phenomena. The following graphic description of the early symptoms which characterize hydrocephalus, is deserving of record in this place. We could not abbreviate without injuring them.

"The earlier symptoms are those of irritation, the latter those of oppression.

"The early symptoms are generally so mild as to attract but little attention, and often escape the notice even of the parents of the patient. The disease is most frequent in persons of a scrofulous constitution, where there is little energy of the system, and little activity in the vascular system; or, it is either the consequence of some organic disorder of the brain, or its investing membranes,—or of such disorders as impede the free return of blood from the head, or the free circulation through the bowels of the chest.

"The early symptoms of the more mild form of the disease, are, a sense of general uneasiness over the body, accompanied by languor, paleness, and collapse of the features; the eyes look languid, have not their usual brilliancy, and frequently are turned upwards; the child is peevish, which is strongly expressed in his countenance; the countenance has no longer the bloom of health; his head feels heavy, and the child, unwilling to run about, seems easily fatigued, and always desirous of lying down on the top of the bed. His appetite is bad, and capricious—and seems unwilling to take the trouble of eating, and there is often bilious vomiting.

"If the child be old enough to express his feelings, he complains rather of a dull noise in his head than of acute pain, and this increased somewhat on pressure. He is very restless in bed, and seems much disturbed in his sleep.

"In a short time the symptoms become more urgent, the skin becomes very dry, and the lips are cracked. There is a permanent lassitude; the bowels become torpid; the child complains of constant pain over the eyebrows, which are swollen, and of giddiness; nausea and sickness succeed. There is a total loss of appetite. The senses of vision and hearing are morbidly acute; the light is intolerable,—the patient turns away from it,—keeps his eyelids half closed,—is awakened from his slumber by the slightest noise, and often starts from sleep screaming. He will not allow his pulse to be felt.

"Many children suffer very little, if at all, from headach, at the commencement of the disease; and in those who have, we observe that there are remissions.

"The headach is relieved in the earlier stage of the disease by purgatives.

"The cheeks are much flushed, especially in the evening, and one cheek is frequently more flushed than the other; his nose is very itchy, and his tongue hot and dry. The patient awakes generally in a fright, and screaming; seeming to suffer excruciating headach, to relieve which, he clasps his hands

on his head; even when not disturbed, he sleeps only for two or three hours at a time; he suffers from sickness, expresses disgust for every kind of food, and vomits especially when moved; and, as Doctors Fothergill and Quin have well observed, 'the sickness and headach sometimes alternate with each other.'

"These symptoms are accompanied by heat of the skin, and quick pulse, amounting to 100 or 110 in the minute, and but rarely by thirst.

"One of the most striking features of this disease, is a torpid state of the system;—salivation is not readily excited; nor does a blister rise well; and very considerable doses of antimonial powder do not occasion even a slight degree of moisture on the skin.

"The patient is costive generally, and is not easily moved by a purgative medicine.

"The faces have an oily appearance, sometimes a very fetid smell, are generally of a green or brown colour, somewhat like chopped spinage—which is not owing to calomel, as it is observed when no calomel has been given, and are in consistence somewhat like glue. Towards the conclusion of the disease, diarrhœa sometimes comes on to such a degree, that opium is required to check it.

"The urine is sometimes retained for a longer time than during health, and there are also instances in which the little patient had a desire every hour to pass water;—and I attended a child afflicted by this disorder, who passed for some days very little urine; but within four or five days of his death, he passed fully as much urine as when in perfect health. Often the power of expelling the urine is completely lost, so that the regular use of the catheter is required.

"The position and gestures of the child in bed, and the effects of motion, merit attention, being very characteristic of the disease.

"A child with this disease cannot bear the erect position, or to be moved, evidently from the headach he suffers; he is tolerably easy only when in bed, and excluded from light. In the earlier part of the disease, he cannot sleep with the head low; he lies in bed with outstretched arms, which has a tremulous motion—are often directed towards the head, or firmly clasped upon it; he is constantly turning and tossing from one side of the bed to the other, as if he could not find any easy posture, and generally kicks the bed-clothes with one or both feet, and very frequently groans much, as if under the influence of pain.

"The gait of the patient is peculiar; he totters, and cannot walk with his usual firmness, one side of the body being weaker than the other; he lifts his legs very high, and takes long steps of unequal length, like a paralytic person. This is a bad symptom, and generally connected with disease at the basis of the brain.

"In the progress of the disease, the sleep is still more disturbed, and the breathing becomes more irregular and difficult. The breath, according to Dr. Whytt, has a sickish and most offensive smell, which he never had observed in any disorder.

"There are also generally symptoms of a

derangement in the functions of the alimentary canal. Dr. Carmichael Smyth has remarked, 'Their abdomen is commonly flat, and compressed in a manner I have seldom observed, except in persons affected with the painter's colic.'

"The tongue is foul; sometimes it has a shining red appearance, or it is covered with aphthæ.

"There is much variety as to the severity of the symptoms, which merits much attention, as demonstrative of the cause of the disease, its seat, and as regulating the prognostic, and mode of treatment.

"Dr. Abercrombie has observed, when 'the effusion was combined with that peculiar destruction of the central parts of the brain, which I have given my reasons for considering as the effect of inflammation of these parts, there has been severe and deep-seated pain.'

"The combination of the above symptoms, the morbid sensibility as to light and hearing—great irritability of temper—the head hotter than the rest of the body, and often covered by a clammy perspiration, sufficiently mark this disorder in its early stage.

"After the disease has been of a few days duration, the pulse, which had been quick and irregular, sometimes becomes slower than in health, and also irregular; which, according to Dr. Whytt, marks the second stage of the disease."

The second stage need not occupy us long. In this stage, the pulse is slower than in the first, or even than in health, besides being unequal and irregular. The skin continues hot, or even hotter than before, while most of the symptoms above enumerated increase in severity. There is a want of correspondence in the movements of the eyes in this stage, and some degree of strabismus may be observed, while the patient's sufferings are evidently rendered more acute. He groans more frequently—gnashes his teeth—loaths every kind of food—sometimes has violent vomiting, while the headach is so acute as to occasion loud screaming. About this period, some patients experience a temporary remission of the more marked symptoms, from which the friends—and occasionally the medical attendants, draw false conclusions.

### *Third Stage.*

This may be termed the stage of collapse. Irritability changes into stupor and drowsiness, and the body often becomes more cold than natural. The pulse is now very rapid—sometimes amounting to 200 in the minute.

The duration of this stage is very various—from ten or twelve hours to several days. The child is unable to turn the head or move the body—he sinks down in bed—is very drowsy, and often insensible to all irritations. One side of the body is frequently paralytic, which is a bad symptom. The eye-lids often droop, to a certain extent, over the pupil, and, on raising them, the cornea appears muddy, and the pupil is much enlarged. The sight is indistinct or double—indeed all the senses, ex-

cept that of hearing, appear blunted. Emaciation proceeds rapidly. Sighing is very common. The discharge from blisters applied to the head is remarkably fetid. The pupil is sometimes dilated and insensible to light—sometimes contracted—which is equally a proof of extreme blindness and insensibility of the retina. Delirium is a frequent symptom, at this early period of the disease; and comes on much more suddenly than in fever. Towards the conclusion, the muscles of the face and extremities are often convulsed, while the thumb and fingers are strongly bent inwards, the pulse being imperceptible at the wrist. We need not pursue the details of this last scene. They are too familiar to the practitioner.

The *peculiarities* in this complaint are very deserving of attention, as tending to puzzle the medical attendant. We shall extract a few of these from the text of our author.

"1st. I have seen several instances in which a considerable quantity of water had been collected within the ventricles of the brain, and in which these ventricles had been considerably enlarged, notwithstanding which, there were no peculiar symptoms which indicated its presence."

"2dly. The disease sometimes begins by repeated convulsive fits, or by a spasmodic contraction of the extensor muscles of the head; by which it is forcibly drawn backwards, and kept so until a day or two before death. This symptom was so strongly marked in two cases, that the mothers of the patients recognised the disease in another child, from the peculiar manner in which the head was drawn backwards; so that the disease somewhat resembles tetanus, but the head was not so rigidly drawn back as in that disease, for it might be bended forwards; but as soon as the hand was taken away, the patient drew it back again.

"These convulsive fits were the first symptoms of derangement in the functions of the system which attracted the attention of the parents, and which led them to seek for medical aid.

"Squinting is sometimes observed about the beginning of the disorder; and the pupil is much dilated, and has somewhat of a reddish colour.

"In some cases there is no affection of the pupil during the whole course of the disease.

"The disease sometimes commences by sleepiness.

"Excessive costiveness is on some occasions the primary symptom of this disease."

Sometimes persons affected with hydrocephalus are seized with hemiplegia, and die in the course of a day or two—and, on dissection, three or four ounces of water will be found in the ventricles of the brain. The disease sometimes begins like acute rheumatism, and the symptoms of cerebral affection do not show themselves till three or four days before death. In some instances, there are no symptoms of irritation, but merely those of compression, as insensibility, coldness of the head and extremities, slowness and irregularity of

the pulse and breathing. A day or two before death, Dr. M. has seen little patients recover their senses, take food, and show symptoms of recovery; but suddenly the phenomena of effusion come on, followed by convulsions and death. Squinting is by no means a constant symptom. There are instances in which the disease has terminated favourably after convulsions, blindness, and delirium, had taken place, and, after the patient had been supposed to be dying. Some patients continue sensible during the whole progress of the disorder, except for a few hours before death.

#### CAUSE OF THE EFFUSION.

This, like most other topics of causation, has occasioned great discussion and discrepancy of opinion. According to Quin, there is but one cause—"a morbid accumulation of blood in the vessels of the brain—sometimes proceeding to a degree of inflammation—and generally, but not always, producing an extravasation of a watery fluid." Drs. Rush, Yeats, and Golis, (of Vienna) also consider the disease as the result of inflammation and turgescence. Before subscribing to this hypothesis, which must operate so powerfully on practice, Dr. Monro thinks it necessary to inquire, "whether this disease usually occurs, in persons who are disposed to inflammatory disorders, at or near the meridian of life, when the human body is most liable to suffer from inflammatory diseases." Although several points in the following train of reasoning are defective, if not erroneous, yet Dr. Monro has adduced sufficient evidence to prove the fallacy of that modern doctrine, (to which we lately alluded, when reporting Mr. Lawrence's sentiments in the Medico-Chirurgical Society) which represents all serous effusions into any of the cavities of the body, as invariably the products of inflammation.

"With regard to the first of these points, it may be observed, that hydrocephalus is so rare after puberty, when the constitution is most liable to inflammatory disorders, that Dr. Cullen,\* and other writers of eminence, have described it as being peculiar only to infancy. That the disease is rather to be imputed to debility follows from the well-known fact, that hydrocephalus is frequently a disease which may be traced to bad nursing, improper food, dentition, the sequel of the most tedious and debilitating disorders, as whooping-cough and scarlatina. Besides, hydrocephalus is often a disease of the fœtus in utero, which nips the bud before or soon after it is blown; for the child dies soon after birth, after having made a few laborious and convulsive respirations.

"If Dr. Quin's theory had been well founded, hydrocephalus, like an inflammation of the lungs, and other inflammatory complaints, should have been most prevalent amongst robust men during the prime of life, when the human frame is most prone to other inflammations; whereas, it is a disease of infancy, of

debility, and very often connected with a scrofulous habit of body. If it be supposed that hydrocephalus is always connected with inflammation of the brain, and that inflammation gives rise to the softening of that organ (which is the favourite opinion of Lallemand, Rostan, and others,) in that case, the brain should be found invariably in a softened state, which is not consonant to my observations."

"Dr. Mills of Dublin has published twelve cases of hydrocephalus, several of which appear to have been connected with inflammation of the brain, but no mention is made of softening of the brain; on the other hand, in one of these, the brain is said to have been *harder* than natural.

"Dr. Hooper, at page 23, in his explanation of his Plate, representing inflammation of the brain, has observed, 'a pulpy state of the part is *now and then met with*.'

"Dr. Watson also has described a case in which the septum lucidum was of an unusual thickness and firmness.

"If hydrocephalus had originated from inflammation, it should have been more frequently the *immediate* consequence of external violence. Whereas Dr. Cheyne observes: 'With extensive opportunities of seeing hydrocephalus, I have *not met one instance* of its having been directly, and I believe only where it was *indirectly, occasioned by external violence*;' and when (as sometimes happens) it originates from such a cause, the effect does not follow until months or years have elapsed, and when a debilitated action of the blood vessels has been induced by the violence, just as palsy is sometimes a consequence of external injury, which palsy is followed by dropsy.

"If inflammation of the brain had given rise to this species of hydrocephalus, the attack of the disease should be sudden and well marked, and its course rapid, like to that of phrenitis; whereas the origin of the disease is generally not well marked; indeed, so much so, as often to escape the notice of the parent, and even that of the experienced physician.

"It may here also be observed, that I have made experiments upon several rabbits and pigs, with the view of determining this question, but though I excited inflammation of the brain by trepanning the skull, and cutting off a portion of the dura mater, the effusion of a watery fluid within the head did not follow.

"It is admitted even by those who impute hydrocephalus to an inflammation of the brain, that the symptoms of phrenitis are well marked, whereas those of hydrocephalus are often very obscure; indeed, in some cases, there is *no one symptom indicating the effusion within the head*. I have met with four cases in which a watery fluid was collected within the ventricles of the brain, and in all of these there were none of the symptoms during life which led to the most distant suspicion of water being lodged within the ventricles of the brain.

\* "See his Definition of the Disease."

\* "Vide Case described at p. 42, in which there were marks of preceding inflammation."

"If this species of hydrocephalus be owing to an inflammatory state of the brain, there ought to be no distinction as to the symptoms, origin, progress, and consequences of phrenitis and hydrocephalus.

"Inflammation of the brain is thus defined by Dr. Cullen: '*Pyrexia vehemens; dolor capitis; rubor faciei et oculorum; lucis et soni intolerantia; pervigilium; delirium ferox, vel typhomania.*'

"The symptoms of this species of hydrocephalus do not correspond with the above definition.

"One of the most striking features in inflammation of the brain, is the state of the pulse; but that character is also wanting, for the state of the pulse is widely different from that of a person afflicted by apoplexy or inflammation of the brain. It is not full, as in the former, or hard, as in the latter. It is no doubt quick, as in other diseases which are the effect of debility; in the same manner as the pulse rises after a great deal of blood has been drawn.

"Beside, no one author who has described the symptoms of phrenitis, has stated that the pulse becomes slower some time after the commencement of the disorder; and, on the other hand, bleeding from the eyes, mouth, bladder, and intestines, which are so frequent during phrenitis, have seldom or never been observed to occur during the progress of hydrocephalus. If hydrocephalus depended upon a slight degree of inflammation, the disease would be more frequently cured.

"There is a great difference between the watery fluid which is effused in this species of hydrocephalus, and that in the former most acute species which is connected with inflammation. The latter is turbid, and masses of coagulable lymph shoot through it, and it resembles water accumulated within the abdomen in cases of dropsy of the belly, originating from peritoneal inflammation. Whereas the former is as clear as spring water; and as Bellini, Boerhaave, Du Haen, Malpighi, Drs. Watson, Carmichael Smyth, and Coindet remark, the effused fluid does not coagulate by the application of heat or the mineral acids."

The analysis of the hydrocephalic fluid by Dr. Trail, is in support of our author's opinion. At the first tapping, no coagulable lymph was found, as the fluid had not been derived from an inflamed surface; but, after tapping the brain, which operation had probably induced some degree of inflammation, the nature of the effused fluid was materially altered—it contained a certain proportion of coagulable matter.

Dr. M. observes that, an inflammation of the brain is frequently the immediate sequel of insolation, or of external violence. But water in the head is not occasioned by similar causes, nor is the disease more frequent in warm than in cold climates, like phrenitis—nor is it the immediate effect of external violence—nor of trepanning the skull, and afterwards injuring the brain of the animal, a fact which he ascertained by repeated experiments on animals.

"Injuries of the head, no doubt, sometimes give rise to hydrocephalus, but the effect does not immediately follow the cause; the fluid is not effused until after the lapse of several months or years, when the vessels have become debilitated, in consequence of the previous over-excitement. (To employ Dr. Cheyne's own words,) 'By calling into play, what, from a good and fortunate management, had hitherto become latent; I mean, a scrofulous condition of the system, which I have regularly observed to follow a severe accident, and which wonderfully favours the establishment of hydrocephalus.'

"Some advocates for the opinion that hydrocephalus originates from inflammation of the brain, have imputed the disorder to a fault in the digestive organs.

"But a fault of the digestive organs, so far from adding to the vigour of the constitution, produces a very contrary effect, and, by diminishing the vis vitæ, tends to avert or to remove a disposition to inflammation.

"The morbid appearances most frequently discovered on dissection, are generally hostile to the hypothesis, that hydrocephalus acutus is connected with inflammation.

"The pia mater very seldom exhibits, in cases of hydrocephalus, those appearances which, according to the late Mr. J. Hunter (a very competent judge,) are the genuine marks of inflammation.

"That distinguished surgeon has observed (p. 281.) 'When inflammation takes place in parts that have a degree of transparency, that transparency is lessened. This is probably best seen in membranes, such as those membranes that line cavities, or cover bodies in those cavities, such as the pia mater, where, in a natural state, we may observe the blood-vessels to be very distinct. But, when we see the blood-vessels fuller than common, yet distinct in such membranes, we are not to call that inflammation.'

"There is no part of the body in which it is so difficult to make the distinction between the presence or absence of inflammation, as in the pia mater.

"The pia mater did not bear, even according to Dr. Quin, all the characters of genuine inflammation,—it was not thickened,—it did not adhere ultimately to the substance of the brain, as in cases of inflammation of that organ. According to that distinguished pathologist, Dr. Baillie, 'when the pia mater is inflamed to a high degree, pus is formed.'

"Dr. Quin does not state that he discovered pus upon examining the state of the brain of persons cut off by hydrocephalus.

"Besides the thickening of serous membranes, as the dura mater and arachnoid coat, a preternatural adhesion of these generally follows the inflammation of these membranes. The result of my post-mortem examinations is, that, in a very few cases only, there were appearances of preceding active inflammation.

"It may be argued by the advocates for an inflammation of the brain being the invariable cause of hydrocephalus, that the slightest de-

gree of inflammation of that organ, or of its investing membranes, may not be appreciable by our imperfect senses; in the same manner as we cannot suppose (as has lately been stated,) that there is no difference in the purity of the air in the centre of London, and that at the top of the Malvern hills of Worcestershire.

There is no one point more difficult to determine, whether a morbid accumulation of blood really exists; and *granting that it does exist, whether it be connected with inflammation or not?*

"Until the precise caliber of the blood-vessels shall be ascertained, it is quite impossible to say when blood can be said to be accumulated in these vessels. Blood when accumulated in the blood-vessels, must occasion an enlargement of these, which does not suddenly happen.

"Inflammation is not the necessary concomitant or sequel of an accumulation of blood in the blood-vessels, the eye, for instance, often acquires a red colour, though the pulse be not accelerated, and though there be no degree of pain in the eye or forehead, and no degree of impatience of light, or any other symptom of active inflammation.

The vessels of that part of the brain which was lowest, were, in *many instances, fuller of blood than those of the uppermost part of that organ, which evidently is connected with the position of the head rather than with inflammation.*

"As the different advocates for the opinion, that hydrocephalus is connected with inflammation, have published very different statements as to the kind and degree of the inflammation, it does not seem to me to be against the rules of evidence, to suppose, as it is so difficult to distinguish the genuine characters of inflammation, that, in some of the instances, *no degree of inflammation had existed.*"

There are other arguments, Dr. M. thinks, which might be urged against the phlogistic doctrine. The same treatment has not been pursued in hydrocephalus as in other inflammations. If it originated in this last, it should be generally removed by the lancet. But local depletion is found more beneficial, and Dr. Quin himself acknowledges "that such patients do not bear large bleedings well." After leeching, Dr. Quin recommended mercury, as an efficacious remedy. Dr. Monro considers mercury as a stimulant and not a sedative—hence it ought to be injurious in purely inflammatory affections. This argument of Dr. Monro is one of the weakest which he has brought forward. Whether mercury is a stimulant or sedative may be a matter of opinion, or, indeed of doubt—but that it is injurious in inflammations is negated by facts and daily observation. The following passage does not quadrate with the modern doctrine which ascribes all dropsical effusions to preceding inflammation.

"Of the identity between hydrocephalus and dropsy there is at least presumptive, if not positive, evidence.

"There is an analogy between the cause of hydrocephalus and dropsical disorders. Hydrocephalus, like dropsy, more frequently originates from debility than from inflammation, and is a frequent disease of infancy,—a period of life when the human frame readily bends under the pressure of every cause, which enfeebles the power of the constitution, as of long continued fever, scarlatina, phthisis pulmonalis, marasmus, measles, whooping cough, and diseases of the liver, spleen, and mesenteric glands.

"Dr. Hamilton senior has justly observed, 'that hydrocephalus often steals slowly on the devoted victim, with symptoms resembling incipient marasmus.'

"Till some better theory is established, it is not unreasonable to suppose, that *the marasmus of which I have treated, may, on some occasions, give rise to hydrocephalus, by impairing the vigour of the constitution, and favouring serous effusion in the ventricles of the brain.*

"It may be added, that Morgagni has mentioned the case of an elderly woman, which strongly illustrates the great influence of debility in producing hydrocephalus; it is said, 'she sank progressively to her grave, as under the pressure of age.'

"The detraction of blood has sometimes occasioned dropsy of the abdomen or chest. The same observation may be extended to hydrocephalus."

The experiments of Dr. Seeds and Dr. Sanders have shown that the excessive detraction of blood gives rise to effusion of serum within the head.

An impediment to the free return of blood from the venous system is, according to our author, the most frequent cause of hydrocephalus, as, also, of other dropsies. Hence, hydrocephalus is frequently connected with tumours of the brain—with an enlargement of the pituitary gland—tumours in the neck—hypertrophy of the heart—disease of the lungs—"or with any cause which impedes the free return of venous blood, as tumours pressing upon the superior longitudinal sinus, torcular Herophili, or the jugular veins."

"Hence we meet with the effusion of water within the ventricles of the brain of criminals who, when in perfect health, have been killed by suspension. It is remarkable how soon the effect follows the operation of this cause. I found about half an ounce of water at the basis and within the lateral ventricles of the brain of a criminal, whose body I examined immediately after execution; and in other criminals, after the lapse of a few hours.

"Dr. Kellie of Leith, in his very ingenious paper upon death from cold, has observed, 'The effusion which was discovered within the heads of our subjects, can hardly be regarded as a post-mortem production; nor can it be presumed that it existed previous to their exposure on that night which terminated their existence. The perfect parallelism of the two cases,—their agreement with another case by Quetmalz,—their simultaneous exposure and death on the same night that another individu-

al died under similar circumstances, render such a supposition highly improbable. If this serous effusion were not a post-mortem effect, and if it had no existence previous to the exposure of the individuals, then we must conclude that the whole, or the greater part, was effused in the short interval between their exposure and their death."

A suppression of accustomed evacuations is acknowledged by all writers to be a very frequent cause of dropsy, and it may be extended to hydrocephalus. Dr. Golis, a warm advocate for the inflammatory origin of dropsy, tells us that, "when the urine is unnaturally scanty, the physician ought to be on the watch for an affection of the head." Hydrocephalus is very often connected with scrophula, and is most frequently found in scrophulous families. Dr. Percival, of Manchester, remarked that, out of 22 children who died of hydrocephalus, 11 were manifestly scrophulous.

"The appearances on dissection, in cases of hydrocephalus, scrophula, and dropsy, are similar.

"When water has been accumulated within the ventricles of the brain, scrophulous tumours in the vicinity of the venous sinuses, or in other parts of the brain, are occasionally found, scrophulous tubercles of the lungs or liver, and a scrophulous enlargement of the mesenteric glands. See cases above described, at p. 49.

"The analogy between hydrocephalus and dropsy, may be further traced from the effect of the remedies by which both diseases have been occasionally relieved or cured.

"Hydrocephalus is occasionally removed by the same remedies as other kinds of dropsy, though, on account of the peculiar structure of the brain, and the effects of the disease in deranging its structure, the cure is more uncertain.

"From what has been above stated respecting the origin, causes, and appearances on dissection, instead of regarding hydrocephalus as generally connected with inflammation, it is rather to be imputed to scrophula, or to those causes which occasion a derangement in the circulation of blood through the brain, through the bowels of the chest and belly, than to inflammation of the brain, or what has been called sub-inflammation by authors, and which acts peculiarly on a scrophulous habit."

The prognosis is generally unfavourable, though by no means so invariably so, as some writers would teach. Whytt acknowledged that he never cured a single patient who had those symptoms which certainly denote the disease. Fothergill made a similar confession. The reasons why hydrocephalus is so generally fatal, are ably summed up by Dr. Monro in the following manner.

"1st, Because when water has been accumulated within the brain, the functions of that important organ are, by the pressure of the accumulated fluid, more or less deranged, and the pressure probably occasions a deviation from the healthy structure of those very important parts of the brain which are in the vicinity of the ventricles; to which is to be

added the influence of water, when once accumulated, giving occasion to the rapid effusion of more fluid, from its compressure upon the veins proper to the membrane which lines the ventricles, the large vein of Galen, and the velum interpositum.

"2d, The disease often proves fatal, because it is often connected with an organic disorder of the brain, or of its investing membranes, or with organic disorders in the neck or bowels of the chest or belly, which in many cases cannot be removed.

"3d, Because, if we adopt the opinion of some authors, that it arises from a congestion of blood in the veins, the observations and ingenious experiments of Dr. Kellie have shown, that it is very difficult to remove such a congestion. The case detailed in pages 112 and 113 of this Essay, and the experiments of Dr. Seeds, show that large bleeding is followed by effusion of fluid into the ventricles.

"4th, Whether the softening of the brain be the prelude to the effusion, or the consequence of it, considering the disorganization of the brain thereby produced, it is difficult, probably impossible, to remove it."

Notwithstanding the above propositions, which are but too well supported by the experience of the profession, there are not wanting many instances in which all the more usual symptoms of the disease, already enumerated, have yielded to the efforts of nature, or to the means which the healing art has supplied. Drs. Monro, Percival, Dobson, Rutherford, and many others, concur in this opinion. "There are also a few examples in which the effusion of water has ceased, after the head had attained an extraordinary magnitude."

*Treatment.*—We have been much more minute in our analysis of the other chapters of the work before us than we shall be on this. The treatment of a disease will vary much according to the pathological notions which the practitioner may have imbibed.

"If the disease be supposed to originate in debility, in laxity of the brain and its vessels, it may possibly be averted,—by avoiding cold,—all vicissitudes of the weather,—and every means by which the bodily strength may be impaired,—and by endeavouring to improve and invigorate the constitution, by generous diet, wine, and the keeping the bowels regular,—and by removing irritation, and the irregular action of the chylo-poietic viscera,—by warm clothing, and moderate and daily exercise in a pure atmosphere.

"By adopting such a mode of treatment, I think I have had the satisfaction of averting the disease.

"But, on the other hand, should the disease be supposed to proceed from the over-excitement of the vessels of the brain, and if inflammation be the primary cause, and the effusion merely the effect, an attempt should be made to remove that state by the general and topical detraction of blood, low diet, by refrigerant applications to the head, by purgatives, and by setons applied to the neck, by

spices and blisters, and by avoiding all such causes as induce plethora.

"In my description of the symptoms of the subacute hydrocephalus, I have endeavoured to point out to my reader, that a derangement of the functions of the alimentary canal gives rise to the earlier symptoms of hydrocephalus. The patient's stomach is disordered; the abdomen is often tense, and painful; the stools are like clay, of a variegated colour, and of a very offensive smell; hence the necessity of evacuating the contents of the intestines by calomel and other purgatives.

"I have stated torpor of the intestines to be a consecutive symptom; and this merits peculiar notice, as indicating a derangement in the functions of the brain, in which case, a large blister should be applied over the head.

"Calomel, combined with James's powder, is of great use in restoring the healthy functions of the bowels."

Fothergill, Rush, Cheyne, and Carmichael Smyth, have highly recommended the daily use of the more powerful cathartics, as gamboge, colocynth, scammony, combined with calomel, and these in large doses, on account of the torpor of the bowels.

"In general, children bear calomel better than adults. If calomel be given in an over dose, it produces colic and severe diarrhoea, and sometimes inflammation of the intestines. A grain may be given every third hour to a child of a year old, and the dose is to be repeated until a free evacuation has been produced. But should the child have acute pain in the belly, the medicine must be given up, for if more be given, an inflammation of the intestines will probably follow. I have given three grains of calomel, three or four times a day, to children of ten or twelve years of age, afflicted by this disorder; and it did no more than keep the bowels open, or, at most, produced only three or four stools."

Dr. M. informs us that, by the application of a large blister, composed of tartar emetic and wax ointment, to the head, and the use of calomel, combined with James's powders, he has cured the disease, not in one, but in several instances. Dr. Rutherford, his colleague at the Dispensary, adopted the same plan, and has stated to Dr. Monro that he was equally successful. Dr. M. was first led to the employment of James's powder, from the recommendation of Dr. Cheyne, who speaks highly of its efficacy in this dangerous disease. "If there be pain and tenderness in the region of the liver, the patient will derive much benefit from the application of leeches to the abdomen." Dr. M. prefers one large blister of tartar emetic to a succession of small ones composed of cantharides. The latter occasion strangury and much irritation. But the fact is, that the operation of the one is very different from that of the other. The eruption of pustules, and the production of vesication, are very dissimilar processes, and effect very different actions in the animal economy. We will not renew the discussions respecting the efficacy of mercury in this disease. If we

are acquainted with the sentiments of writers and practitioners generally, on this point, we will say that the utility of the medicine under consideration, is put beyond the possibility of cavil. It is true that some have recommended calomel in large doses—some in small. Perhaps there is little difference in the ultimate effects of both plans. When the head is affected, there is usually such torpor of the bowels, that large doses of medicine produce no more action in the intestinal canal than much more diminutive quantities. Hence the great deception into which practitioners may be led. Dr. Monro has given us the following document from his father, with which we will conclude this analysis.

"After mentioning twenty-two cases of this disorder, in which he had unsuccessfully employed mercury, he states:

"As in the greater number of the above cases, the disease had made considerable progress before I was called; and, as most of the patients survived but for a short time thereafter, the effects which the mercury may have, if given on the first appearance of the symptoms, are by no means fully determined. And, as I have repeatedly found, in other dangerous species of the natural encysted dropsy, particularly in hydrothorax and ascites, that mercury combined with squills or other diuretic medicines, in such quantity as to salivate in a slight degree, contributed much to the relief or cure of the patient, I would recommend the further trial of it in hydrocephalus. At the same time, considering the importance, sensibility, and delicate texture of the parts which are affected, and total failure in the cases I have described, I cannot help suspecting that several late writers are much too sanguine in their expectation of removing hydrocephalus by the use of mercury."

We must now close this article, promising to furnish our readers with some important information from the same work, respecting certain pathological conditions of the brain, in one of our succeeding numbers.

We have restricted ourselves, in this paper, almost entirely to the humble, though not the less useful, labour of analysis. But the talented author will excuse us, if we venture to dissent a little from him, as to the part which inflammation takes in the serous effusion which is usually found in the disease under consideration. We have often expressed our opinion, that it was too much the fashion to consider phlogosis as the root and branch of the effusion. But we cannot, consistently with our own observations, go the length which Dr. Monro has gone, in the opposite view of the pathology of hydrocephalus. We are convinced that, in a great majority of cases, the effusion which we find in the heads of infants who die with the common symptoms of hydrocephalus, is the result of an inflammatory process more or less acute—and, consequently, that our remedial measures should generally embrace local depletion from the head—especially in the early stage of the disease. At the same time, we are glad to have the

authority of Dr. Monro, in support of a doctrine which we have long advocated—namely, the possibility of watery effusion from other causes than inflammation.

Although we mean to return to Dr. Monro's work again, we cannot take leave of it, without expressing our sincere esteem for the amiable and talented author. The small volume which he has now published, abounds in rich materials, without the slightest tincture of wild hypothesis—arrogance—presumption—or the prevailing sin of the day—censure of the opinions or practice of his brethren. Dr. Monro is a worthy representative of the old school of amenity, liberality, and true love of science? If *primus et secundus* Monro could start from their marble cerements, and view the distractions of medical society at the present moment—if they could read the malevolent and ignorant effusions of a *Scotus*, indited in the Intellectual City—they would, doubtless, join in the doleful verse of their countryman, Smollett—

“Mourn, hapless Caledonia, mourn—  
Thy banished peace—thy laurels torn!”

From the *Lancet*.

#### ON A VARIETY OF CLUMP FOOT. By M. Holtz, of Strasburg.

Among the malformations of the feet, some are congenital, others occur after birth, generally before the age of puberty, and sometimes without any very evident cause. The most frequent congenital distortion of the foot, is that which the ancients called *varus*, when the foot is turned inwards; the second distortion is that in which the foot is turned outwards; it is called *valgus*, and is extremely rare. But we find no where another variety of clump-foot in which the point of the foot is turned backwards, so that the person walks almost wholly on the dorsum of the foot.

There was for twelve years, in the Civil Hospital of Strasburg, a pensioner, a watch-maker by trade, who presented this variety of congenital distortion in both feet. His legs were thin, and the calves scarcely developed. Both feet were turned backwards, as from forced extension. The dorsal surface had become the inferior, and the plantar surface the superior. Walking was not painful; he could march easily and without support. He did not suffer the inconvenience of those whose feet are turned inwards, and who are obliged to raise one in order to advance the other. The knees were always a little bent, so as to maintain the centre of gravity in equilibrio. He uniformly rested his weight on the tarsus, rather more on the external than on the internal surface of the foot; the metatarsal bones and the toes did not touch the ground when he remained stationary. He wore common half boots, the heels of which were turned forwards, and the points backwards; they were laced at the back part.

In 1825, at the age of 64, he was affected

with pulmonary catarrh, for which he was placed under the care of Professor Lanth; after a short time the man died, not of the complaint for which he was admitted, but of cancer of the stomach. The feet presented the following peculiarities:—when compared with the feet of persons whose walk is natural, they were small, and the skin covering the part which rested on the ground, was callous, and folds in different directions were observed on the plantar, which, in this case, was the superior surface. In order that the description may be as clear as possible, I will examine first the state of the bones, then that of the ligaments, and, lastly, the muscles.

The bones of the leg were well formed, and in their natural relation to each other. The foot was turned backwards, which occasioned the astragalus to be dislocated forwards, the os calcis forwards and outward, and the cuboid downwards on the calcaneum; but there was no anchylosis nor change in the state of this bone. The dorsal surface was very convex, excepting at the spot which touched the ground, whilst the inferior was very concave, without either of the edges appearing shortened or changed in its figure.

The os calcis, instead of touching the ground and completing posteriorly the plane on which the foot rests, was drawn upwards by its posterior part, into which the tendo-achillis is inserted, and at the same time directed outwards. The superior articulating surface was not in direct and immediate relation with the inferior surface of the astragalus; but it corresponded to its external surface, where, in the natural state, the external angle is situated, without, however, touching this surface in its whole extent. The superior articular surface of the astragalus was turned directly forwards and a little downwards, instead of being in contact with the articular surface of the tibia; its posterior surface also looked forwards, and the tibia rested on the inferior, in a great degree, and the small process of the calcareum. The inner surface was free, and gave attachment to ligamentous fibres; it was not in contact with the internal angle; the outer surface looked towards the superior surface of the os calcis. The tibia rested on the inferior surface of the astragalus, and the inner angle was situated above the scaphoid; the outer rested between the astragalus and os calcis, on a part of the concave surface placed above the tendo-achillis, and on the inner and back part of the inferior surface of the astragalus. The connexions of the scaphoid with the astragalus were more natural; the scaphoid was, however, turned a little backwards. The cuboid rested by its posterior on the inferior surface of the os calcis. The articular part of the astragalus and os calcis gave attachment to ligamentous fibres. The three cuneiform bones, the metatarsal bones, and the toes, had not experienced any sensible change in their position.

The articulation between the os calcis and astragalus differed, in many respects, from the natural state. The articular surfaces of these two bones were not in contact with each other:

a space existed between them, at the posterior and outer part of the foot; this space was filled by a fibrous and ligamentous substance.

The muscles of the foot had undergone some remarkable changes. The tendo-achillis, which was strong and thick, kept the astragalus separated from the ground, at the same time that it drew the foot inwards and backwards. The tibialis anticus, from its direction and attachment, drew the foot upwards and inwards. The tibialis posticus drew the whole foot backwards and upwards, principally its inner edge. The flexors acted in the same way, as well as the muscles which are attached to the plantar aponeurosis. The abductor minimi digiti was not relaxed, as it is said to be in clump-footed persons, and as one might be led to imagine it would in this case, but it was contracted so as to draw the outer edge of the muscles inwards and upwards. The extensors were weak and elongated, and, consequently, did not counterbalance the action of the flexors.

From what I have said of the state of the parts, both hard and soft, which enter into the composition of the foot, and which contribute to its motion, it will be seen that the principal cause of this irregular arrangement was the dislocation of some of the bones of the tarsus, and that the ligaments and muscles only acquired consecutively their unnatural situation.

Scarpa pretends that the astragalus is not deformed, at least, in common cases, in these deformities; and he feels assured that the other bones of the tarsus are not dislocated, as is generally supposed, but only turned on their maller axis. This case differs altogether from those which I have observed, not only in the degree of turning of the foot upwards, and its direction backwards, but also from the astragalus being evidently concerned, as well as from the dislocation of this bone, of the os calcis, and cuboid. The attachments of the ligaments were little altered from the natural state; their direction only was changed, and consequently their action; which, although weak, tended to increase the deformity, and to retain the bones in their unnatural situation. The muscles had not any unnatural attachments; but the contraction of some, and the weakness of others, from their great extension, impeded the return of the foot to the normal state. Moreover, the tendons having an unnatural direction, the contraction even of the weakened muscles, such as the extensor communis digitorum, and extensor proprius pollicis, tended to increase the mal-position of the foot.

The causes which may determine or favour the development of like deviations of the feet *after birth*, are well known. But those which give rise to these deviations in the womb of the mother, are involved in great obscurity. Scarpa, Boyer, Delpech, thought that they had discovered the first cause of the irregular formation of the bones of the tarsus, or even of a single articular surface, viz. in the want of equilibrium between the muscles which move the foot, in the shortness of some of these

muscles, in an unnatural insertion of some of their tendons, &c. This, however, does not completely explain it. In the old man, who was the subject of my observations, no unnatural attachment existed; the bones were well formed, some articular surfaces were bare and rough, which, however, was not concerned in the cause of the deformity. There was certainly a want of equilibrium in the action of the muscles, which is evident, from the description which I have given; but nothing proves that the relaxation of some, and the increased action of others, had been the cause of this considerable turning of the foot upwards.

If we examine the fœtuses of different ages up to the ninth month of pregnancy, we observe a remarkable tendency of the feet to turn inwards. The legs bent and crossed, the feet resting in front and on the buttocks; the soles directed against them; the external edge looking downwards, and the point inwards. This state of parts is more marked, according to the youth of the fœtus; it is very well seen in a young born infant, even until it has begun to walk.

The cause of congenital distortions of the feet consists, then, in a change in the direction of these parts, produced by their mal-position in the womb of the mother, and rarely in an unnatural structure of the parts.

I regret to say, that we have no machine applicable to the cure of this kind of distortion.

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From the London Medical and Physical Journal.

#### CASE OF EXTRA-UTERINE FŒTATION.

By — TILT, Esq.

Sophia Lickish, æt. thirty-seven, was twice married; bore four children to her first husband, and, six years after her second marriage, again became pregnant. During her former pregnancies nothing unusual occurred, and her confinements were reported to have been short and easy; and nearly three months of the fifth had elapsed before any accident gave rise to untoward symptoms. While in the country, at the commencement of September 1827, she was exposed to unusual exertion in drawing water from a deep well, during which she felt as though something internal had given way; which was immediately followed by discharge of blood per vaginam, vomiting of bloody fluid, impaired appetite, and tenderness of abdomen. To these were shortly added retention of urine and constipation of bowels. Being no longer able to discharge the duties of her situation, she returned to her husband in town. Medical aid was now procured: her water drawn off with the catheter, and her bowels opened by medicine; which course of treatment was found necessary for a considerable time, when, the symptoms somewhat lessening in violence, fewer purgatives were required, and pressure above the pubes was found sufficient to empty the bladder.

Upon the 28th of January, 1828, I first saw her. She was in bed, to which she had been constantly confined since the accident. She was much emaciated, her general health very bad, and her strength nearly exhausted by frequent vomiting, a copious fetid discharge from the vagina, and incessant abdominal pain, which she compared to lingering labour-pains. She now considered herself more than seven months gone, and was unusually large. Although the symptoms which existed, when conjoined to the history of her case and her present appearance, left little doubt in my mind as to its real nature, yet, anxious to neglect nothing which might assist my diagnosis, I requested an examination, which was readily granted; but nothing could be satisfactorily ascertained. The os tinæ could be nowhere felt; no part of the child was recognisable; and a tumour of considerable size lay pressing against the sacrum. Two medical friends (Dr. H. Davies and Mr. Clark,) were also allowed to examine her, but nothing more was elicited. These circumstances, viewed in connexion with the symptoms above mentioned, led us to conclude that the child was extra-uterine, and likewise to suspect retroversion of the uterus.

By the administration of small opiates, the irritability of her stomach was diminished, the intestines were kept open with castor oil, and her health gradually improved under a soothing plan of treatment.

Up to the 10th of February, nothing occurred of any importance; but, on the morning of that day, without any apparent cause, the vomiting returned; pressure above the pubes relieved the bladder with difficulty; her countenance became much altered; and the motions of the child, of which she had been sensible for the last three months, together with the vaginal discharge, now ceased.

At the suggestion of Dr. H. Davies, the opiate was conjoined with chalk mixture; and a blister was applied to the epigastrium, on account of the irritability of the stomach, which had again returned. Although the blister did not rise well, and the draught was rejected, she felt relieved. On the day following, effervescing draughts, occasionally administered, with opium and capsicum, kept the stomach quiet, and procured some rest at night. Her bowels acted freely this evening without medicine, and the next morning her urine flowed, for the first time, without pressure over the pubes. Notwithstanding, however, these slight signs of improvement, she continued gradually to sink until the 14th, when she died.

An inspection of the body being a great desideratum, the friends were consulted, and permission was accordingly obtained. Dr. H. Davies and Dr. Dill were present at the examination, in which I was assisted by Mr. Bell, a friend of the former gentleman.

*Post-mortem appearances.*—The skin was universally jaundiced; the feet slightly œdematous; the abdomen very much, but irregularly, enlarged, being considerably fuller on

the left side; the mammae were flaccid and contained milk; and the external parts of generation were as tumid as in natural cases of labour. By moving the hand carefully over the surface of the abdomen, the head of the child could be distinctly felt, lying a little above and to the left of the umbilicus, and one of its extremities (whether hand or foot could not be satisfactorily determined,) was easily recognised resting above and to the right of the head. By tapping the surface over these two points with the finger, fluctuation was very palpable; and the tenuity of the intervening integuments, apart from the position of the fœtus and other corroborating symptoms, rendered the nature of the case completely evident. The arch of the colon was distinctly traced running across the epigastrium, immediately above the fetal mass, and the abdominal covering was, by its pressure outward, moulded into a form somewhat similar to itself.

Upon making a longitudinal incision through the parietes of the abdomen, it was found that a large quantity of fluid lay in the peritoneal sac, and that the dissection could not be prosecuted with the necessary precision until it was removed. An opening was, therefore, made in the lowest part of the right lumbar region, through which much bloody fluid escaped, and the remainder was extracted from above by a sponge, the whole amounting to four or five pints.

A transverse incision was now made, and the flaps everted with considerable difficulty, as the peritoneum was glued somewhat closely to that portion of the membranes lying over the child's head.\* The contents of the abdominal and pelvic viscera being now fairly exposed, a tolerably minute inspection furnished us with the following detail:

The peritoneum was highly inflamed, very dark, and considerably thickened. The small intestines were pushed up into the bosom of the diaphragm, and the omentum, which was wasted to a mere shred, lay reflected over the stomach. The liver was usual in size, but its peritoneal envelope, in common with that of the other viscera, was as much changed in texture as in aspect: nothing morbid, however, could be discovered in its internal struc-

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\* I think it is more than probable that, had the woman survived much longer, the abdominal integuments would have suppurated at this point, allowing the child to escape in detached pieces. See American Magazine for 1746, where a case is given in which several parts of a child came thus away, after remaining in the abdomen for several years, during which the woman bore six children: she, however, died. See likewise Journal de Med. Chir. &c. tom. xiv. page 443, for a case in which a complete skeleton was removed from the abdomen, through an aperture in the umbilical region formed by sloughing. The patient perfectly recovered, and ever after menstruated through this opening.

ture, and the gall-bladder and biliary ducts appeared natural.

The uterus was seen lying obliquely across the pelvis, with its fundus in the right lumbar region, and its os tinæ rather above, and to the left of the symphysis pubis. Its size was above that of its ungravid state: it felt tolerably firm, and was externally very dark coloured. Between it and the intestines, principally in the umbilical and left lumbar regions, lay the fœtus involved in its membranes; the placenta being exterior to, and betwixt them and the arch of the colon. A fillet of fibro-ligamentous substance, about an inch and half in width, ran down from the inferior surface of the arch of the colon over the membranes, and was inserted into the superior edge of the left ligamentum latum. This band being divided and turned aside, the subjacent membranes were carefully opened, and the fœtus exposed to view. It was smaller than the average of full-timed children, but must at least have been eight months old. It lay upon its left side, with its lower extremities stretched over the sacral promontory into the posterior pelvic cavity; and its head, which was somewhat reclined, resting upon its left arm. Its face thus looked towards the diaphragm, and its chest was presented to the left side. The left side of its head and face was flattened, in consequence of pressure upon the arm, but, excepting this accidental deformity, it seemed perfect and well shaped. As the motions of the fœtus had not been perceived for five days previous to the woman's dissolution, it was believed that the child had been so long dead; but, from abrasion of the cuticle and other signs of decomposition, it is likely that this event happened earlier than was suspected. There was no more than two or three ounces\* of fluid in the membranes, which was of a very dark colour, apparently from admixture of blood. The chorion and amnion were perfectly natural, differing in nothing from those formed in the uterine cavity; and exterior to these lay the sac,† or ventral uterus, within which the fœtal mass, excepting the placenta, was contained. The walls of this sac were about three lines thick, of considerable strength, perfectly smooth internally, and tenaciously adhering to the neighbouring parts. It was attached to both the broad ligaments, descended along the posterior surface of the uterus in company with the subjacent peritoneum, was reflected over the sacral promontory, adhered in its ascent to the sigmoid flexure of the colon on

the one side, and to the psoas muscles on the other, rose upon the lumbar vertebræ, swept over the internal surface of the placenta, and, turning a little above the umbilicus, ran down over the membranes to the ligamenta lata. It was nowhere connected with the chorion, and yet no space intervened between them. Above it, and considerably to the left side, lay the fillet before described.

The placenta was of enormous size,\* and very extensively attached. Its width was greater than usual, it was five inches in thickness, and could not have weighed less than five pounds. In structure it differed little from those met with in ordinary cases; perhaps, it was somewhat firmer. It adhered with great tenacity to the sac, excepting in the inferior part on the right side, where it was detached for a few inches, the intervening space being filled with coagulated blood.† Its supply of this food must have been very liberal, as its connexion with blood-vessels was very extensive. The right, and especially the left mesenteric and emulgent vessels seemed to be the main fountains from which nutriment was drawn for the fœtal system; but, besides these, there were many minor sources, since it adhered throughout to the inferior surface of the arch of the colon, a great part of the mesentery and meso-colon, to portions of the small intestines, and two or three of the superior lumbar vertebræ. It lay across the spine, but unequally, the greater part of it being on the left side; and the surface, by which it was connected with the mother, faced the diaphragm, from which it was separated by the intestines which had been forced up into the bosom of this muscle.

Nothing could be discovered amiss in the structure of the uterus, which was about the size of one in the fourth month of pregnancy.‡ Externally it was inflamed, and presented the same darkened appearance with the abdominal viscera. We have already stated that the sac adhered to its posterior surface, and, upon separating this membrane, for which considerable force was necessary, slight vascularity appeared behind and a little to the left of the right lateral ligament, but there was not a vestige of cicatrix, nor did any thing exist to sanction the idea of rupture of this organ. The os tinæ was dilated, and two

\* The average quantity of water contained in the amnion in ordinary cases is about two common pints.

† A case is recorded in the seventh volume of the New-York Medical Repository, page 221, where a sac of a very similar character was found, and the anterior portion of which, as in the present instance, adhered to the peritoneum lining the muscles of the abdomen.

\* In cases of extra-uterine fœtation, the placenta is much thinner, and, although more extended than uterine placenta, is considerably lighter and less bulky. I am not aware that there is any case recorded in which the placenta equalled the present, either in weight or size.

† The hemorrhage which resulted from this partial detachment of the placenta had, no doubt, accelerated the death of this woman; and it was the blood, then effused, that darkened the fluid which before lay in the peritoneum.

‡ It is generally found more or less enlarged in such cases.

fingers could be admitted with ease. The cervix appeared slightly vascular, but the internal surface of the fundus was pale and smooth. The right fallopian tube was pervious, of natural size, and complete; but there was no right ovarium to be perceived. The left ovarium was small, and contained several vesicles. The left fallopian tube was somewhat enlarged, and about the middle of its superior surface, near to the insertion of the fillet before mentioned, its coats were ulcerated for an inch and a half.\* Upon either side of this opening it was pervious, and its caliber throughout larger than that of the right. The vagina was quite healthy.

Several questions of importance must suggest themselves to the mind on reading the above detail; and the first in interest undoubtedly is, how did the fœtus make its way into the abdominal cavity? Did the ovum remain in the ovarium, and did the ovarium, by gradually enlarging, constitute the sac in which the fœtal system was contained? Or did the ovum, in leaving the ovarium, fall into the abdomen; not being grasped by the fallopian tube? Or did the ovum, during its passage through the fallopian tube, escape into this cavity? Or, finally, did it reach the uterus, and, by retroversion of this organ, constitute one of those cases described by Dr. MERRIMAN?†

It will be admitted, I presume, that the answer must be found in some one of these suppositions; and, although there are many circumstances of obscurity accompanying the history as well as the dissection of the case, and there may be arguments advanced in support of each, several reasons have induced me to conclude that it was through the left fallopian tube the ovum escaped into the abdomen.

We have no reason to believe that its escape was occasioned by the corpus fimbriatum neglecting to embrace it on its departure from the ovarium; and there exists no evidence to show that there ever was retroversion in this case. During the first six months of pregnancy, no examination per vaginam was made; and, although retention of urine and constipation of bowels are presumptive of this misplacement, yet these symptoms did not occur until the third month of pregnancy, when the fœtus, membranes, and placenta had evidently arrived at an unusual size, and, by their position behind the enlarging uterus, will satisfactorily account for their appearance. It is true that several medical gentlemen had examined her some weeks before her dissolution, and suspected retroversion; but the absence of the os tincæ, and the tumour that pressed upon the rectum, the

only circumstances on which our diagnosis was founded, were otherwise explained by inspection after death, when the uterus was found in a position the reverse of that assumed in retroversion.\*

The absence of the right ovarium was the only fact favourable to the supposition that this was a case of ovarium pregnancy; and, independent of the impossibility of determining whether or not it did ever exist, I think there are several circumstances tending to show that this solitary fact cannot be regarded as conclusive.† The fœtus lay almost entirely on the left side, and it was the right ovarium that was absent;—the adhesions of the sac, in which it was contained, were much more numerous on the left than on the right side;—the fillet which bound down the membranes was inserted into the left fallopian tube, near to the point of its ulceration, and lay entirely in the left side; and the greatest portion of the placenta was attached to viscera belonging to the left side. Besides, had this been an ovarian case, why did the woman pass the first three months of pregnancy in health and ease; and why did symptoms of disorder, when they did appear, come on instantly and with violence? Had the ovum never left the ovarium, the gradual increase of this latter organ would have prevented the sudden approach of constitutional derangement; and, although the disturbance occasioned might have been ultimately great, it would not have commenced with violence.

We are, therefore, inclined to believe that the ovum escaped, during its passage through the left fallopian tube, into the abdominal cavity, and was gradually invested with membranes and a false uterus. Could it be shown that the right ovarium had never existed in this woman, this view of her case would be demonstrably true, seeing that the ovum must have been impregnated in the left ovarium, and that the left fallopian tube was found ruptured. But, as we have nothing beyond presumptive evidence that such was the case, although highly probable, it cannot be considered certain.

The position of the child, and the strong and numerous adhesions of the sac in the left side,—the easy and natural course of her

\* It is extremely difficult, perhaps impossible, in some such cases, to determine when retroversion exists, and not a few have experienced the same difficulty with ourselves. See *Medical Commentaries*, vol. xx. page 254; *London Medical Journal*, vol. v. page 96; *Medical and Physical Journal*, vol. xi. page 293; *Transactions of a Society*, &c. vol. ii. page 287.

† Ovarian cases are not very common, and it seldom happens that the ovary reaches a large size. "It either bursts early, or inflammation and abscess take place, or the fœtus dies and is converted into a confused mass, or it excites dropsy of the ovarium." (*BURNS' Midwifery*, page 193.)

\* A case is detailed by Mr. TUCKER, in which the left fallopian tube was ruptured about the same part; and the death of the woman proceeded from partial detachment of the placenta.

† See his *Dissertation on Retroversion*.

pregnancy for the first three months; the sudden supervention of untoward symptoms; the feeling of something having given way internally while using violent exertion, instantly followed by derangement of the whole system, and a bloody discharge per vaginam; the continuance of this derangement, with increasing violence, from this period to that of her decease; the fact that the ovum does not leave the ovarium until six weeks or two months after impregnation, and the near correspondence of this period with that which elapsed prior to the accident; the ruptured condition of the left fallopian tube, and the formation of the fillet at the margin of the rupture; the appearance of the sac being that of a false membrane, and not of a bag covered with the peritoneum; the presence of the greatest part of the placenta in the left side, and the absence of the right ovarium;—are the principal circumstances by which we have been influenced in forming our opinion. Whether the ovum lodged for some time in the tube before the accident, or had merely proceeded so far through it when the accident occurred, are circumstances difficult to determine. Perhaps, the larger cavity of the left tube, and the slight difference\* which existed between the period that elapsed from the commencement of pregnancy until the injury was sustained, and that generally occupied before the ovum leaves the ovarium for the uterus, are circumstances favourable to the first supposition, yet cannot be deemed sufficient to establish the point.

That there would have been some discharge of blood after the accident might have been expected, but that this discharge should have been continued unto the period of the child's death, when it ceased, appears strange and inexplicable. Was this the menstrual fluid,† or some morbid accidental discharge? If the former, the ordinary course of nature in such cases was reversed; for, during the life and growth of the child the menses are discontinued, but return after its decease; and, if it were a morbid product, where was its source? Was it secreted by the vagina, or did it come from the uterus? or could it possibly have drained from the abdomen through the ruptured tube, as a part of that fluid, some of which was taken out of it after death? The internal surface of the uterus and vagina, excepting a small portion of the cervix of the former, was pale, and betrayed no symptoms

of having given rise to any such secretion, and the external characters of both these fluids were very similar; but, admitting that they were the same, we are furnished with no reason why the discharge was discontinued upon the death of the child, and, moreover, it is very doubtful whether fluid existed in the abdomen during all the time which elapsed from the occurrence of the accident.\*

Indeed, the entire case is so perplexing and obscure, that almost every part of it may be considered a subject for discussion and difference of sentiment; and, acting under this conviction, while for reasons adduced we have defended the views now given, all the minutiae of the case are laid before the reader, that he may have materials from which to draw his own conclusions.

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From the London Medical Gazette.

#### M. FERRUS'S TREATMENT OF INSANITY.

It has been often said that no physician has more followed the precept of Hippocrates, "to permit philosophy to take part in the study of medicine," than Pinel; and of his works, none have so strong a title to the term *philosophie*, as his Treatise on Insanity. Pinel threw around this dreadful class of diseases the light of reason; he traced the symptoms in a profound manner, distinguished the various shades of the disease with nicety, and overthrew all the ancient superstitions which associated these maladies with the powers of the demon, or the consequences of fatality. It is by pursuing the steps of this great master, that M. M. Esquirol and Ferrus direct the treatment of mental diseases. This malady, consequent upon organic lesion, is submitted to the general laws of Therapeutics. Whether, according to M. Esquirol, and some of the disciples of Pinel, the brain is not the organ always primarily affected; or whether, on the contrary, according to Gall, Georget, and Spurzheim, it is invariably the first and only seat of the disease; this important organ is now, at all events, the especial object of the physician's attention.

In all the establishments devoted to the cure of the insane, and particularly at Bicêtre, madness is, without doubt, considered as an affection of the brain, whether it be idiopathic, or the result of a distant morbid influence;—but what is the nature of this affection?—what organic state of the parts constitutes it?—There are two modes of considering this point.

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\* Although the period above specified is the average time spent by the ovum in the ovarium, yet it may certainly remain longer; but, supposing that two months of pregnancy had passed before its departure, a few days only would be the difference between that period, and that which elapsed between conception and the date of the accident, which occurred some time during the third month.

† Cases are upon record in which the menses flowed for several months during pregnancy. (Vide Burns, page 187.)

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\* From the inflamed condition of the peritoneum in this case, it is probable that the greater part of the fluid found in the abdomen after death, was the effect of peritonitis; and it is not to be conceived that such a disease could have existed long prior to the decease of the woman.

According to some, madness is an inflammatory disease; whilst others think that it is a nervous malady—that is to say, an aberration from the healthy function of the organs of the encephalon, without any appreciable physical modification in those organs. To which ever of these opinions M. Ferrus leans, his practice would lead to the belief that he is guided sometimes by one view and sometimes by the other; and since his practice is attended with numerous instances of success, it may not be unfair to conclude, that these opinions relative to insanity, are not incompatible with each other. In the first accession of mania in young and vigorous subjects, and especially when the complaint appears to be deducible from some external cause; when the phenomena of reaction are fully developed; when the congestion in the brain appears to be decided, or intense; M. Ferrus practises, and always with advantage, a large bleeding either from the arm or neck. The effect is, as it were, instantaneous; the delirium diminishes immediately. The bleeding may be repeated even three or four times during the same attack. Solitude, strict diet, diluents, and warm baths, complete this plan of treatment. Thus it appears that this species of mania is treated like acute and intense inflammations of the thoracic or abdominal viscera.

Those who think that insanity is the consequence of a latent chronic inflammation of the brain, or its membranes, will see, in the practice followed at Bicêtre, facts which may tend to support their opinion. With those maniacs who are moderately calm, whose constitutions announce but little energy, and yet in whom the local pain and the heat of the head are intense, M. Ferrus applies leeches to the temples, or cupping glass to the sutures, or the actual cautery to the nape of the neck. Purgatives, pediluvia with mustard, also assist the action of the local bleedings in such cases. This treatment must be continued with perseverance; but it must be admitted that this is not the plan which, at Bicêtre, is found to afford the greatest sum of success. It is only applicable to those in whom the disease has lasted a considerable time; and among these are numbers in whom the complaint is innate and hereditary. Thus it becomes necessary to change the temperament of the individual; and medicine, it must be confessed, has but little power in that respect.

As we have before hinted, there are many cases wherein M. Ferrus departs from the antiphlogistic plan. We have often seen antispasmodics, and even narcotics, prescribed at Bicêtre; such as camphor, in small doses, either the fœtid gums, the extracts of hyoscyamus, stramonium, belladonna, &c. It is particularly in monomania that these measures are had recourse to; and especially in those cases of monomania in which the hallucination affects some of the sensorial powers. The subjects of this form of mania are generally of a weakly constitution, with effeminate features and pale skin, the brain being well developed; in short, they present the phenomena assign-

ed to the nervous temperament; and it may be remarked, that the frequency of insanity in persons of this organization, has not a little contributed to establish the opinion that this disease is only an exaggeration or perversion of the action of the brain, purely functional.

The practice of Bicêtre, in fine, may teach us that in insanity, (as in many other diseases, the nature of which, either inflammatory or nervous, is said to be much better known,) the antiphlogistic and antispasmodic methods of treatment have numerous points of coincidence; and that, consequently, the success of a plan of treatment, of one kind or other, does not permit us to presume upon the absolute nature of this singular disease. If the advantages derived from medical treatment appear to lead to this result, there would be still, in the relation of cases of mania, a considerable number which could not be so classed. Such are those in which moral treatment alone has been employed. Affectionate remonstrances, solitude, labour demanding little intellectual exertion, and in a different direction to the tendency of the disease, are therapeutic means which bear little analogy either to bleedings, opiates, &c. However, solitude for a diseased brain may be compared to rigid diet in the phlegmasiæ, or to repose in diseased joints, &c. &c. The same may be said respecting the admission of certain ideas, or a selection of moral impressions. In a disease of the digestive organs, you select some particular articles of food, observes M. Ferrus; well, in the same manner, in insanity we permit only certain sensations to reach the brain, by directing the attention of the patient towards sentiments, or towards intellectual operations, opposed to those which are diseased. It is, therefore, of much less consequence, continues this gentleman, to know the nature of madness, than to understand what modifications of the animal or intellectual faculties constitute it.

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From the London Medical Gazette.

#### HERNIA IN INFANTS.

*To the Editor of the London Medical Gazette.*

SIR,—On inspection of M. Dupuytren's case of strangulated hernia, published in a recent number of the Medical Gazette,\* it seems, at first sight, scarcely possible to doubt that it would have been reducible, had faintness been produced. I cannot boast of having managed this disease at so early a period of life as twenty days; but I (as many others doubtless have) have met with strangulated hernia in infants as young as six months. The patients were bled to faintness, and the hernia became readily reducible, or returned of itself. It is matter of course, that where the strangulation has taken place, the constant crying of the child will render its return next to impossible.

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\* Jour. For. Med. Vol. I. p. 555.

The act of fainting, in most instances, is favourable; and a state of insensibility, both in adults and infants, is also favourable to the efforts of the surgeon, by checking the efforts of the patient.

Nothing is more easy than to criticise the treatment of a case one has not seen. I think it would, indeed, be well if it were a maxim among us at all times, not to condemn the former treatment of cases, unless the treatment were conducted under our own eye: it is next to impossible to decide on the merits of former practice from the accounts of our patients; nor will their present state very commonly give us a very certain criterion by which we may judge of their former condition. It should be a part of medical morals, never to give any opinion excepting on the present circumstances, of which alone we have a sufficient cognizance to come to any thing like a right judgment.

My chief object for troubling you on this occasion is, lest a tyro, having seen that strangulated hernia in an infant has been treated by so great a man as M. Dupuytren, solely by the application of leeches, might be induced to consider their application sufficient (which, *perhaps*, in this instance, it might have been;) while I think all practitioners will agree with me, that bleeding by a full stream is as much the remedy in this case for infants as for adults; and, so far as the unfrequency of the occurrence will admit of my forming a judgment, in proportion a much more effectual remedy. Indeed, I can scarcely conceive the possibility of its failing, if the remedy is resorted to during the bleeding. Should bleeding to faintness not succeed, I am sure the operation, considering the remarkable irritability of the infant constitution, should be resorted to instantly; or, at all events, very early. In this disease, the experience of the late Mr. Cline's life can never be too frequently brought forward: "I have known," said he, "many die of performing the operation too late—not one by its being performed too early." Pupils should be reminded, that if blood cannot be obtained in stream from the arm, which it may in most instances (and I own I think in acute diseases it is the better practice,) it may always be obtained from the jugular vein.

I am further led to offer this criticism, that I may call the attention of practitioners to an accident I have met with a few times, and which may possibly have been overlooked, viz. the occasional intrusion of small portions of intestine or omentum at the navel, where the vacancy is only partially filled up. This I have found to be a frequent cause of the tormine to which infants are especially liable during the month. I was led to observe this circumstance, which I am even confident occurs much more frequently than could be supposed, by the following accident:—I was called to a child in the month, of whom I had assisted the mother in the delivery, and in whose respiration I had found no fault; it had shrieked out, and fainted suddenly away then for the third time; it was in all respects healthy. The

child had soon recovered, but the last fainting fit had lasted longer than the former attacks. I ordered the child to be stripped (a necessary step always, if a practitioner would really understand the diseases of infants,) and perceived that while it cried, the naval protruded somewhat more than it does naturally. On pressing the part, I felt a slight gurgling under my finger. A cork pad was placed over the navel, secured by an adhesive bandage, and the symptom never returned. In this instance, it would seem that temporary pressure of the intestine produced the faintness.

This circumstance has led me to examine the abdomen in children about this age, where they have been suffering from tormina. I have given instant and permanent relief, again and again, by the use of this bandage. I strongly suspect that many a diarrhoea occurring in the month, where the infant is unfed, is attributable to this cause, viz. the occasional pinching of the intestine, and a scarcely perceptible vacancy: its frequent intrusion produces slight inflammation, which nature relieves by instituting a purging. It is easily perceived how pain should first be created by the accident, how the consequent irritation should produce fits of crying, and the crying and the pain should at length become cause and effect alternately.

Of one thing the reader of this communication may be assured, that I have relieved several diarrhoeas occurring during the earliest periods of infancy, by the application of the cork pad and adhesive bandage, without any other remedy, where the infant has been unfed, and the mother in health.

One does not readily see why diarrhoea should occur, unless from such a cause, which appears to me sufficient to account for the disorder; that it does not continue, the subsequent filling up of the vacancy will account for.

Should this trifling communication, Mr. Editor, assist in relieving the troublesome sufferings in infants, or prevent the early introduction of medicaments (often opiates) into the lying-in chamber, I shall feel I have been serviceable; and be much obliged by your insertion of it in your publication.

I have the honour to be, your obedient servant,

P. F.

From the Medico-Chirurgical Review.

**A PRACTICAL ESSAY ON STRICTURE OF THE RECTUM**, *illustrated by Cases, showing the Connexion of that Disease with Affections of the Urinary Organs and the Uterus—with Piles and various Constitutional Complaints.* By FREDERICK SALMON, Surgeon to the General Dispensary, Aldersgate Street, and formerly House-Surgeon to St. Bartholomew's Hospital. Octavo, pp. 188. London, 1828.

It is no doubt true, that strictures in the urethra are infinitely more frequent than strictures in the rectum. But when these last

do occur, (which, by the bye, is by no means a rare case,) they are a great deal more distressing to the patient, and detrimental to the constitution, than the former. The stomach and bowels are organs of higher importance in the animal economy than the kidneys; and, consequently, whatever disturbs their complicated functions, whether in the process of digestion, or in the evacuation of the fecal remains, produces more serious and more numerous sympathetic disorders in the constitution at large, than any mechanical difficulty which the bladder may experience in the evacuation of the renal secretion. We are not unaware, indeed, that the irritation of an urethral stricture will occasion, in some people, considerable disturbance of constitution; but we do maintain, that these sympathetic disturbances are more distressing than dangerous—and that, in the worst cases, they are not to be compared with the effects of stricture in the rectum.

The object of Mr. Salmon is to prove, that stricture of the rectum is a very common disease, inducing other important affections—and that surgery furnishes us with means adequate to its removal or alleviation, if judiciously exercised. It is not our intention to enter into a minute analysis of Mr. Salmon's book in this place; but merely to touch lightly on some of the principal features of the work.

Under the head of "CAUSES and KINDS" of stricture in recto, Mr. S. examines the opinions of different surgeons. Mr. Bell considers the stricture as owing to a morbid change in the inner membrane of the intestine—not unfrequently about the inner edge of the sphincter ani. Mr. Copeland thinks the complaint may be produced by whatever excites inflammation or irritation in the inner membrane of the canal. Mr. White, of Bath, thinks the predisposing cause is the narrowing of the gut about the termination of the sigmoid flexure of the colon—which narrowing, he seems to look upon as sometimes an original malformation. Mr. Salmon is disposed to agree, in some measure, with Mr. White, on this subject; and thinks it illustrates a fact he has repeatedly noticed—that of several children in the same family being afflicted with stricture.

"Any cause, however, tending to produce local irritation in the rectum, existing for a continued period, may give rise to contraction: thus, habitual costiveness is among the most frequent causes of the complaint.

"Another, and I believe a common, cause of stricture, will be found in the administration of large doses of drastic purgative medicines, a practice peculiarly tending to irritate the bowels, and, by promoting increased contractile action, to impair their natural functions. Patients have informed me, that the first symptoms of the disease followed immediately upon taking violent aperients.

"Indigestion may give rise to stricture, the acrid state of undigested matter irritating the inner coat, and, in this manner, causing improper action of the intestine."

A peculiar constriction of the anus is some-

times the cause of stricture. Mr. S. has rarely found this constriction unattended with obstruction very high up in the rectum. In females, an occasional and afflicting source of stricture is an enlarged and tender condition of the uterus, of which we have a melancholy case now under our care. Every time that a motion is procured, by nature or art, the sufferings are dreadful, and can only be allayed by the introduction of laudanum and starch into the rectum. In these unhappy cases, little or nothing can be done by surgery. The only means of mitigating the pains, are, by emollient injections, to bring away the fæces in a soft state, and afterwards throwing up anodynes.

Strictures may occur from enlargement of the prostate gland—from piles, tumours, or excrescences—in short, from any cause, constitutional or mechanical, creating irritation in the rectum, and, thus, ultimately inducing spasmodic action of its muscular coat. In process of time, the complaint changes from spasmodic constriction into permanent stricture. Our author, indeed, thinks that the greater number of strictures are, at first, simply spasmodic, but that, in time, depositions take place between the coats of the bowel, that assist in preventing the natural action of the part, ending ultimately in induration and thickening, so that all trace of the natural structure of the gut is lost.

The third chapter of the work is on the SYMPTOMS of Stricture. Insidious as are the signs of this disease, in its early stage, yet Mr. Salmon thinks they may be detected by close examination. We give the following extract from his symptomatology; observing, however, that many of the symptoms here laid down do frequently occur, independently of any stricture in the rectum.

"In its commencement, trifling irregularity of the bowels occurs, the motions being deficient in quantity, sometimes passed in small pellets, at others flattened like tape, or having the appearance of worms. Instead of the bowels being every day fully and freely emptied of their contents, a day, sometimes two, will now and then intervene without any evacuation occurring; or, what is more common, patients following their customary habits, will, at a particular period of the day, void, as they suppose, an adequate motion from their bowels; yet were they to examine the quantity, they would find it next to nothing. A sense of soreness is experienced at the anus, at the verge of which, after the discharge of the evacuations, the skin, from extreme rigidity of the part, frequently gives way, forming several small cracks or fissures distressingly annoying. Occasional pains occur in the loins and lower part of the back, which sometimes extend into the groins, particularly the left, the hips and thighs, imparting a sensation similar to cramp.

"Sooner or later, decided costiveness supervenes; opening medicines are resorted to, which, affording temporary relief, satisfy the patient and his medical friend. This state of

things will sometimes exist for a very extended period—for months, nay, even for years, people will from day to day administer opening medicines, increasing the quantity in proportion as their constant use and the progressive advancement of the disorder render them necessary, till at last, weary of the trouble and inconvenience, they relinquish them. Very speedily this irregularity is followed by a difficulty of passing the contents of the bowels; after going to stool, a sensation is experienced as if the rectum was not completely emptied; persons feel a disposition, yet have not the capability to pass more relief, and endeavour by violent straining to force out the contents of the gut, which proves of little avail, serving only to produce a discharge of blood, and a prolapsed state of the bowel. As the disorder advances, these symptoms progressively increase, till at last, many days together will pass without any relief occurring, though the patient will be tormented by frequent calls, and the most painful yet ineffectual efforts.”

In other instances, (of which we shall presently give a remarkable example) accumulations take place in the colon, distending it to an enormous size; the whole alimentary canal becomes disordered; and inflammation of the bowels ensues, too often terminating in death. The various sympathetic disorders that are consequent on stricture in the rectum, cannot here be portrayed. The stomach, the kidneys, the bladder; in short, the general health, all suffer from this terrible disease.

*Treatment.*—Mr. Salmon divides this into constitutional and local. In the former, however, he includes leeching the anus, cupping the perineum, &c. The main object is attention to the bowels.

“No use of instruments can possibly establish a recovery, unless the bowels are brought to a free and comfortable relief daily; to accomplish this, two points should constantly be observed—first, that we do not load and annoy the stomach by the too plentiful administration of food—secondly, that we do not irritate the bowels by an injudicious use of purgative medicines. By adopting the plan of diet herein after advised, we shall avoid the first evil, but the administration of medicine is of equal importance. Here I cannot too strongly deprecate the common every day’s practice of giving violent doses of purgative medicine; not only is no benefit derived from such treatment, but serious injury is frequently induced. When adopted in diseases of the rectum such must be the result, since large quantities of feculent matter are driven from the small intestines into the colon, already distended, as a consequence of stricture in the rectum, and highly irritated, and thus arise frequent and distressingly painful efforts to pass motions, the straining to accomplish which, may, in extreme instances, induce inflammation, or even rupture of the intestine at the sigmoid flexure.”

Here Mr. Salmon introduces the case of a lady, who died from the effects of violent purgative medicines. In a severe attack of

constipation, peritonitis supervened, for which drastic purgatives were exhibited. In straining at stool, she felt something give way internally, and quickly expired. On dissection, it was found that a cherry stone had lodged in the sigmoid flexure of the colon, where the passage was nearly obliterated by a stricture, close to which the gut had given way, and extravasation was the result. Castor oil, or the electuary of senna with sulphur, are no doubt, the best aperients, in cases of stricture. The use of injections are here of great importance; but the way in which they are employed often frustrates the intention of the practitioner. Half or three parts of a pint of thin gruel with a little castor or common oil, should be thrown gently up, and the patient should keep in bed for an hour or two afterwards, so that it may be retained in the bowels, and thus soften the fæces before they pass the stricture. It is certainly a matter of regret, as our author observes, that the fastidious feelings of some, and the want of consistent recommendation in others, should prevent the adoption of this salutary and harmless measure. We copy our neighbours in many of their frivolities and inconsistencies, yet disdain to adopt those habits and customs that are both healthy and cleanly.

On the subject of diet, Mr. Salmon makes many judicious observations, and shows himself a warm advocate of the Abernethian doctrines.

*The Bougie.*—Mr. Salmon thinks that the constitutional treatment, however careful, can be only palliative, without the bougie. The following directions are judicious, and they are too little attended to by surgeons in general, who introduce the bougie with little or no preparation.

“One or two hours previous to the examination of the rectum, an injection is to be administered of tepid poppy water, containing forty or fifty drops of laudanum, which will tranquillize the bowel, and remove any lodgement of fæces. The patient should also be requested to make water immediately previous to the introduction of the instrument. The rectum is first to be examined with the finger, to ascertain that there is no kind of obstruction near the orifice.

“The patient, if a male, leaning over the back of a chair, or the side of a bed,\* should draw aside the nates fairly to expose the orifice of the bowel. A full sized bougie, not less than eleven inches in length, thoroughly softened, and well oiled, adapted to the shape of the passage through which it is to be passed, is to be introduced, with the convexity of the first curve towards the sacrum, in which way it is to be passed upwards and backwards about two inches† through the third portion

“\* In females the examination being made beneath the bed clothes, is conducted without the slightest exposure.”

“† The last curve of the rectum is so trifling, that it matters not whether we introduce the

of the bowel, provided it gives no pain, for the introduction will commonly produce an uneasy sensation; we continue to propel the bougie in the same direction, about three or three and a half inches higher, or through the second portion of the rectum; the point of the instrument will now bear directly upon the hollow of the sacrum, and the but end towards the left side of the body. With a view, therefore, of avoiding the sacrum, and of accommodating the instrument to the great curve of the rectum, we change its position, by describing the segment of a circle from left to right, with the but end, turning it upwards, at the same time continuing to propel the instrument. Having described this segment, we shall have carried the bougie full four inches farther, or to what may be considered the extent of the rectum. But it is yet to be introduced into the sigmoid flexure; we therefore triflingly depress the but of the instrument, at the same time propelling it upwards, till the whole is fairly within the sphincter—this accomplished, we may be satisfied."

The patient generally complains of pain, both in the rectum and over the surface of the abdomen, when the instrument has passed about five or six inches up. If obstruction be encountered, trifling pressure is to be maintained, for a minute or two, and then, if the pain increase, and the instrument remain stationary, it is to be withdrawn—a smaller size introduced—and so on, from larger to smaller, till we ascertain the bougie that passes with trifling pain or difficulty into the sigmoid flexure. The instrument should remain in the bowel ten or fifteen minutes, provided it produces no considerable irritation; at the expiration of which time it is to be removed, and allowed to harden in the shape which it had assumed in the intestine. The first introductions generally produce some irritation, and a nisis to relieve the bowels; but the rectum gets accustomed to the bougie in a short time. The operation should be repeated at intervals of three, four, or five days, gradually increasing the size of the bougie, and lengthening the period of its remaining in the bowel.

In the foregoing directions, it will be seen that Mr. Salmon differs from most preceding writers, as to the frequency of repeating the operation. Mr. Bell, after stating the constitutional treatment, directs a daily introduction of the bougie. Mr. Copeland gives similar instructions, but recommends the bougie to be left in for a longer period. Mr. White says the instrument should not remain longer in than half an hour at first—but, after some introductions, he lets it stay in the rectum eight or ten hours at a time. In general he uses the bougie daily. Against this diurnal introduction, the following objections are urged,

"The action resulting from the introduction of the convex or concave part of the instrument towards the sacrum, but by passing it with the convexity backwards, we avoid the necessity of altering the position of the bougie in passing it through the second curve of the bowel."

tion of the bougie is twofold—dilatation and absorption. Now it must be obvious, that to produce these effects, it is necessary that a certain degree of pressure be maintained upon the strictured portion of the gut. It is by dilatation that the simple spasmodic stricture is removed, and by dilatation and absorption combined, that the permanent obstruction will be materially alleviated, if not totally cured. Our object should certainly be to dilate the passage as speedily as possible; nevertheless it should be borne in mind, that the introduction of the instrument causes a specific action or irritation, by which we overcome the unhealthy function of the part; this action, or irritation, should be allowed totally to subside before we again introduce the bougie. If, however, we daily pass bougies, suffering them to remain in the bowel for eight and ten hours at a time, I would ask, what time do we allow for the subsidence of the irritation we have created? nay, do we not encounter it at every use of the instrument, and thus rather promote than lessen the disease?"

In the early period of his practice, Mr. S. followed the directions of the writers above mentioned—experience induced him to prefer the plan which we have noticed.

The structure of the bougie is considered to be a matter of great importace. He thinks Mr. White's bougie is not sufficiently firm. It yields too readily, and consequently does not make pressure enough on the strictured parts.

"The instrument I have been accustomed to use, is composed of fine linen cloth, very heavily coated with wax, and a certain portion of diachylon plaster, mixed with a small quantity of lampblack. From immersion in very hot water, some minutes previously to being used, it is rendered soft and pliable to any extent, nevertheless retaining one regular and smooth surface. When it is introduced into the bowel, instead of becoming softer, it hardens to a degree sufficient to afford considerable resistance to the action of the stricture."

As to gum-elastic bougies, when the stricture is remote from the orifice, it is next to impossible to introduce them. Metallic ones are still worse.

The introduction of the bougie is followed by cramps in the thighs and legs—numbness—shiverings—sickness, &c. not very unlike those phenomena which follow the introduction of a bougie into the urethra. These disappear after a few operations with the instrument.

"Of all the annoyances attendant upon the introduction of the instrument, none is more troublesome to the patient and the surgeon than the powerful action and the irritation of the sphincter muscle: which may be considerably lessened by taking care to pass the bougie completely through both the external and internal sphincters, first affixing a tape through its loop, a precaution which should never be neglected; for the bowels will sometimes draw up the bougie, so as to take it completely out

of the reach of the finger; this happened in case the seventh, hereinafter narrated. The patient, exceedingly alarmed, sent for me; but, prior to my arrival, he had voided the bougie, rolled up like a ball. It caused the most distressing pain in its passage through the sphincter, but no subsequent serious inconvenience resulted from the accident."

It is curious that patients under the use of the bougie are more susceptible of cold than at other times. In some cases, the bowels will soon become regular after the use of the bougie—in others, no material benefit will result for many weeks—in some, the bowels never become regular, however much the health may be improved.

In a very short chapter on carcinomatous disease of the rectum, Mr. S. advises, of course, that we should avoid producing irritation by any attempt at introducing bougies. All we can do, in these unhappy cases, is to soothe pain, and keep the bowels gently open.

A considerable number of valuable cases are introduced into the work by Mr. Salmon; but we shall only have room to notice one, which is very curious and interesting. We must abridge it very much.

*Case.*—Mr. —, aged 41, complained, on the 31st of October, 1821, of pains in various parts of the body, especially the loins, knees, and ankles. He had an ulcerated throat, and his digestion was greatly deranged. The least quantity of food produced pain in his stomach. He had undergone courses of mercury, under the idea that he had syphilis, and he was now taking four grains of calomel daily. The mercury was discontinued—proper medicines were prescribed—and, on the 24th Dec, we find him reporting himself quite well, and setting out on a journey. On the 13th May, 1822, Mr. Salmon saw him, when he informed Mr. S. that, till within a fortnight, he had been in perfect health. He now laboured under constipation and irregularity of the bowels—hæmorrhage occasionally from the rectum—pain in the region of the stomach—scanty laceritious urine—night-sweats—cough. The pulse was tranquil. He had been a free liver, and even now indulged in wine. He got a little better, and went to Brighton, on returning from which place, in July, 1822, he presented a very altered appearance. He was emaciated in the face and limbs, while the abdomen was enlarged and tender, but without distinct fluctuation. He suffered much when his bowels were moved. It was apprehended that his liver was diseased, and mercury, squills, &c. were prescribed. On the 27th July, fluctuation was evident, and he was generally anasarctous. He was seized with violent pain in the abdomen, severe purging, and profuse hæmorrhage from the bowels. All things went on badly, and, on the 2d September, it was determined to tap the abdomen. On examination, a hard protuberance was felt under the margin of the ribs, on the left side. It was considered by Mr. S. and a physician to be an enlarged liver. On the 10th September he was supposed to be dying. He was insen-

sible—pulse intermitting. Brandy recruited him, and, on the 11th, he was much better, having had a diarrhœa, accompanied with much hæmorrhage, and immense *prolapsus ani*. On the 1st October, Mr. S. examined the rectum, and a firm stricture was discovered a few inches up. Some oil and water gruel were injected, when a large quantity of horribly offensive fæces came away. The next day a still larger collection was discharged, the fæces resembling sheep's dung. In short, there had been a complete Augean stable here, in the form of an immensely distended colon. On the 22d Oct. we find the patient greatly improved, by the use of injections and bougies. By the 15th November, he was able to go out, and take exercise—his belly was reduced—his bowels acted regularly, but the motions were still followed by blood. On the 13th December, we find him residing in a tavern in London, indulging in every kind of excess. The swelling of the legs had returned, and the distention of the colon was evident—the bowels were very irregular and the evacuation trifling. We must pass over a great variety of vicissitudes which this miserable and imprudent patient experienced, between December, 1822, and June, 1823, when he suddenly expired, while on the close-stool. The following examination will elucidate this strange eventful history.

"Upon opening the abdomen, I discovered one of the most extraordinary specimens of disease I have ever witnessed; no vestige of the stomach, liver, or small intestines could be seen, but one immense tumour, having an irregular surface, in some points perfectly hard, in others of a softer consistence. Upon examination, this proved to be the colon, distended with hardened fæces; so immensely large was it, that the transverse arch of the bowel extended within a trifling distance of the *pubis*, and the ascending and descending portions nearly coalesced; from these several points coagulated lymph had been thrown out, which had become organized, connecting the different portions of the gut, which was, as it were, glued in one mass: at the superior part, the colon firmly adhered to the great arch of the stomach, thrusting this organ, together with the liver, under the ribs; it had also contracted firm adhesions to the liver and diaphragm; at the sigmoid flexure it was united in the same manner to the superior aperture of the *pelvis*, and its lateral portions to the abdominal *parietes*. After a laborious dissection, I succeeded in detaching it from its different adhesions, and emptied it of the fæces with which it was distended; this I could only do by cutting it through, above the sigmoid flexure and *caput coli*; at the former of which points, the bowel was so completely obstructed, that even water would not pass through it. The intestine, notwithstanding its distention, was very considerably thickened through its whole extent. This appeared principally to have resulted from deposition between the muscular and mucous coats. In the rectum there were two obstructions, one about four inches from the

anus, and a second at about seven inches; and at the sigmoid flexure I could scarcely find any passage at all. The appearance of the obstruction at the sigmoid flexure was not the same as that in the rectum; the former being perfectly circular, whereas the latter was more like the elongation of the semilunar folds of the bowel, the general structure of which was materially thickened through the whole of the gut; the colon was likewise violently inflamed throughout the inner surface, there being here and there patches of ulceration; the small intestines were glued together in one mass, feeling like hard cords, and were diseased through their whole extent. The liver was harder than usual, yet could not be said to be diseased, though it was of a remarkably small size; the gall-bladder was distended with healthy bile, and the stomach contained a considerable quantity of the same fluid. The spleen, pancreas, kidneys, and bladder, were perfectly sound and healthy, as were the lungs and heart, nor was there any unusual quantity of fluid in the pericardium; the cavity of the abdomen contained about three quarts."

The above very curious and interesting case shows how great a mass of feculent matter will lodge in the bowels, while we are daily administering purgative medicines! It also shows that a distention of the colon may be mistaken for enlargement of the liver, or other organ in the abdomen. From this distention arose the difficulty of breathing, the irritation of the bladder, and the numerous indescribable symptoms under which this wretched patient so long laboured.

We must now close our analysis of Mr. Salmon's little work, which is indicative of sound judgment, liberality of sentiment, and a fair portion of practical observation.

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From the *Lancet*.

**TETANUS OCCURRING FROM THE APPLICATION OF A BLISTER, successfully treated by Opium and Electricity.**  
By J. TAYLOR, Esq.

A widow lady, 30 years of age, the mother of one child, had been subject for three years to repeated attacks of uterine pains, requested my advice in March 1827, for a pain in the left side, about the region of the heart. A blister was applied, aperients prescribed, and in the course of four or five days no traces of pain remained; the sore produced by the blister had completely healed, but the integuments remained more than usually tender. On the following Sunday, six days after the application of the blister, she ventured, contrary to my wishes, to walk about a mile to church at a quick and hurried pace, which greatly fatigued her, and on attempting to sing, she felt, to use her own words, "her mouth suddenly close with a violent spasm." Alarmed at such an unexpected attack, she immediately returned home; and when seen by me about an hour afterwards, the symp-

toms were as follow:—Mouth firmly closed; great rigidity in the muscles of the jaws; slight tension of the abdomen; countenance not distorted; slight headach, nausea, thirst; and she complained of debility and stiffness in the neck. The irritation caused by the blister had apparently subsided; pulse 88, soft. In this state, perhaps it was difficult satisfactorily to determine, whether the symptoms were those of Hysteria, or proceeded from the application of the blister; but, reasoning from analogy, I adopted the first opinion, in consequence of having previously attended her sister in several attacks of hysteria, with all the above mentioned symptoms, although the locked jaw never continued longer than a quarter of an hour. Acting on this opinion, an aperient was prescribed, and after its operation, antispasmodics: (these she was enabled to swallow, from having had two teeth extracted a few months before;) which were continued for 24 hours without the slightest mitigation, or even any perceptible effect being produced. The teeth were still firmly in contact, the muscles of the jaw rigid and painful, with great increase of abdominal tension; headach and dimness of sight, watchfulness, and constipation; pulse 89 to 100, and small.

It was now evident, that these symptoms were no longer to be considered hysterical, and as there had been no contusion or wound of the extremities, I could not but regard the irritation of the blister, or some other exciting cause, as the source of those symptoms which, although but trifling at the commencement, were now clearly those of tetanus; consequently 16 ounces of blood were taken from the arm, 10 leeches applied to the temples, æther to the shaven head; an injection of  $\mathfrak{zj}$ . ol. terebinth;  $\mathfrak{zss}$ . sap. moll., and a pint of warm water; and a draught of  $\mathfrak{zj}$ . tr. opii every four hours, which was gradually increased to  $\mathfrak{zij}$ ., and ultimately to  $\mathfrak{ziii}$ ., together with teberinth. injection, and the ext. belladon. to the sides of the face; still the symptoms increased with alarming rapidity; and as the tr. opii had failed to produce any relief, it was discontinued, and the crude opium introduced per anum, commencing with  $\mathfrak{Ej}$ . and gradually increasing it to  $\mathfrak{zj}$ . every four hours, with the terebinth. injection, and cold ablution to the head and neck. Notwithstanding this active treatment, the symptoms continued to increase, with violent and frequent convulsions, the body being bent like a bow, and resting only on the heels and occiput; alternately, delirium and rational tranquillity. At this critical period I determined to try the effect of electricity; accordingly an apparatus was procured and placed in the room, from which a small Leyden jar was filled with the fluid, and the patient holding it in her hands, applied the knob to her chin, lips, cheeks, and forehead, thus receiving the electric fluid in the form of sparks: the application of the second and succeeding jars, was productive of a sensation of heat along the gums, which was soon fol-

lowed by complete relaxation of the muscles of the face, and also of the abdomen, accompanied with cold perspiration, faintness, and other symptoms of exhaustion. A glass of wine was given, a suppository of ℥j. crude opium was introduced, and sleep was procured for the first time since the attack, (eight days.) In the morning, however, these beneficial effects were obscured by a recurrence of spasm more violent than before, which not only resisted the effect of electricity, but even threatened to preclude its further application by a speedy and violent dissolution; however, the paroxysms becoming less violent, and the intervals longer, its application was continued, but without effect, the jaws still remaining firmly closed. But as its effects had been so decidedly beneficial in the first instance, particularly while the system was under the influence of opium, I was still determined to persevere; accordingly ℥j. of crude opium was again introduced per anum, and after an interval of two hours, electricity was re-applied, and with the same happy result as at first. The next and succeeding days served only to convince me, that although the conjoined effects of these two remedies were sufficiently adapted to overcome this formidable disease, the separate application of either was totally useless. This plan was therefore pursued daily for a week afterwards, and the patient ultimately recovered.

It is, perhaps, worthy of remark, that during the use of electricity, the catamenia, which had ceased for three years previously, again returned, but soon afterwards disappeared.

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From the Edinburgh Medical and Surgical Journal.

**CASE OF TUBERCULAR TUMOUR OF THE BRAIN TERMINATING IN HYDROCEPHALOUS EFFUSION.** By ROBERT EVANS, M.D. M.R.C. Surgeons, London.

May 22, 1826. J. N. aged 11 years, was a young gentleman of great acquirements and classical knowledge for his age, the son of a learned clergyman. He was reported to have had a discharge of blood from the bowels, with a protrusion of the rectum, till he was eight years old; and for the last six years this discharge took place occasionally while at stool. This ceased latterly for several months; and since that period he complained of a severe pain of the head, at times more acute, with slight giddiness, muscular debility and languor, listlessness, and disinclination to make his usual exertion. The bowels were costive, with deficient secretion of bile. The head was naturally very large.

He was ordered alterative doses of blue pill, with aperient medicine; the head shaved, and an evaporating lotion applied; leeches to the anus, and if not relieved by them, to the temples also, and to abstain from animal food.

By this treatment for some time he was

much benefited, and took horse exercise. This was a truce of short duration only; for in a little time (18th June, 1826) he complained of more decided symptoms of cerebral disease, as severe headach, great throbbing of the cerebral and carotid arteries, giddiness, double vision, irritability of stomach, and at times full vomiting; stupor, lassitude, and considerable dilatation of the pupils; and a sense of heat in the head, though without much change of colour in the face. The appetite was bad; pulse 60, and small; no thirst; no heat of skin, though restless; urine a little cloudy; bowels costive, and feet cold. He expresses the feeling in his head to be a throbbing, bursting sensation, forcing open the skull. A brother of this patient died some years ago with similar symptoms, and at the same age; but no dissection took place.

18th June, 1826.—For the above symptoms the head was shaved and blistered; and, though the pulse did not indicate it, xvij ounces of blood were drawn from the arm; ten leeches were applied to the temples, and cold to the head. The nape of the neck was blistered, and the surface was to be kept discharging with savin serate. Brisk purgatives were exhibited, with submuriate of mercury every fourth hour till the mouth became affected, combined with squill and digitalis in fit doses, and low diet. The feet were immersed in warm water. After venesection the pulse rose twenty beats, with perfect relief to the head and stomach.

22d June.—Complains very much of the head; vision double; pulse 88; dejections semifluid, dark coloured, and glossy on the surface; head to be well and often rubbed with tartar emetic ointment; venesection to ℥xvj., when he fainted; calomel and James's powder each two grains twice daily; diuretics continued.

26th June.—Much better, and medicines continued.

28th June.—Is better; pulse 70, irregular; dejections yellow; head easier; vision was better, but to-day is again double. No effect from the diuretics.

3d July.—A sense of fulness in the head; double vision, and a mist before the eyes obscuring objects; immense flow of urine; mouth very little affected; pulse 80. Twelve grains of strong mercurial ointment were ordered to be rubbed into the arm-pits night and morning; calomel and James's powder to be continued; weak chicken-broth and bread, which agree with his stomach.

10th July.—Was better, but to-day is very ill, nearly insensible. The head appears much enlarged, and the sutures feel as if separated. The mercurial fætor is great, and the gums are red, but not sore; the pupils greatly dilated; he voided his urine once insensibly; requires aperient medicine; pulse 60.

14th July.—The two days previous he made still a larger quantity of urine, is more sensible, and felt no pain since the 10th. The head seems smaller, and the sutures are not felt so sensibly separated. Sleeps quietly and sound;

has more appetite and desire for drink; complains of cramp in legs and hands, with numbness and sleepiness, as he calls it, particularly in the right, but which are sometimes relieved by friction. He is almost blind, pupils dilated, and iris like a narrow ring. The mercurial ointment, calomel, and James's powder continued, to have beef infusion, chicken-broth, albumen of egg beat up with milk and water, and ripe fruit.

20th July.—Is free from pain; makes much water; no return of spasm; sleeps well at night; mouth not much affected. Continue medicines. A seton inserted in neck.

28th July.—Continues free from pain; is much affected by the mercury; no alteration in sight. As the action of the heart is much affected, the digitalis is omitted. Is cheerful, and joins in conversation; takes nourishment. Is ordered a mild bitter as a tonic to stomach; frictions with stimulating liniment to spine, legs, and thighs; bowels regulated by colocyath pills. The reported protrusion of rectum is now found to be a highly vascular fleshy tumour, with a long flat neck, which was removed by ligature.

This degree of convalescence continued to the 7th August, when he experienced a renewal of all his former sufferings; pain in the temples and forehead, chiefly in the evening; giddiness, and an almost constant involuntary rolling of the eyes; pulse 108 to 110. Mercury was again used, and antimonial ointment to the head; diuretics, &c. as before.

In the present attack the pain was so violent as to induce severe and general convulsion, and apparent apoplexy, ending in profound sleep, and returning with short intervals of ease; also most excruciating pain in the loins, groins, and front of thighs, with vomiting of acid matters. His distress was so great that he prayed to be relieved from his bodily sufferings. He is emaciated to a skeleton; blind, and unable to make the slightest exertion.

From this state he was again relieved, and continued more sensible, with good appetite, regular bowels, and sound sleep, and without pain or convulsion; but the eyes were constantly rolling and turned downwards, and a feeling as if his body was constantly sliding down from his chair.

29th September.—No permanent benefit having been obtained from the treatment pursued, and various other remedies not mentioned, and pain returning at intervals, frequent convulsions, and tremors of the legs, with inability to raise or move them, though possessing sensation, he was merely attended to with nourishing diet, medicine, and enemata to regulate the bowels. Though at intervals free from pain, at others it was most excruciating, with frequent convulsions. He lingered till 26th June, 1827, when death released him from his sufferings. He was sensible to questions, and his mental faculties were clear almost to the last.

He was scarce able to raise his hands to his head; and the fæces and urine were passed unconsciously for several weeks previous to

death. When near dissolution, and scarce able to speak from weakness, and when addressed by his parent on the subject of eternity, his intellectual functions were shown to be unclouded to the last moment by the clearness of his replies. A medical student, a relation, was the greater part of his illness constantly with him.

*Dissection twenty-four hours after death.*—A clergyman and the medical gentleman mentioned were present. The body was so emaciated as to resemble a skeleton covered with skin.

*Head.*—On the upper and anterior part of the right frontal bone was a tumour about the size of a hazel-nut, soft and compressible, but not so large as during life. When the skull-cap was removed, we found in it seven perforations two and half lines wide, and several smaller ones, some on the upper part of the frontal bones near the coronal suture, and in the line of the sagittal on the parietal bones. These perforations were not from caries, but absorption from internal pressure, and were merely closed by a fine delicate membrane, not stronger than the arachnoid seen in the base of the skull, and covered by the common integuments. The whole internal surface of the skull, particularly in the direction of the sinuses, was rough, and studded with numerous spiculæ, not from osseous deposition, but remaining from bony absorption; and generally the substance of the skull very thin and nearly transparent, except a patch at the upper portion of the parietal bones, which was morbidly thick and dark-coloured. The dura mater was perforated by the tumours, which corresponded to the openings in the skull, and its vessels tinged with dark blood, though the sinuses were empty when slit open; and it adhered firmly for a small space to the upper portion of the right anterior lobe of the brain, and in other respects healthy. The falx was healthy, but adhering also firmly to that part of brain similar to dura mater. The pia mater loaded with blood, and easily removed from among the convolutions; and its removal gave vent to a small quantity of limpid fluid.

The convolutions and intergyral spaces seemed in a sound state, and in usual proportion all over the surface, except on the right anterior lobe, which was a mass of apparently disorganized substance, with a surface unequally projecting, like a mass of tubercles agglutinated together, and hard to the feel. On exposing what is termed the *centrum ovale*, the interior of the brain resembled a bag of fluid buoying up the *corpus callosum*, and distending the surrounding substance. The left lateral ventricle contained a quantity of fluid clear as rock water. The *plexus choroides* was small, and of a pale colour.

The right ventricle was enormously distended with a brownish-coloured fluid, that coagulated on being exposed to the air. The floor and side of this ventricle next the *corpus callosum* were lined by a layer of apparently organized lymph, closely adhering to the subjacent surface, about a line thick, highly vascu-

lar, and smooth surface, and when cut into resembled the cortical substance of the brain. The *plexus choroides* was larger. The outer wall of the right ventricle, and indeed the whole of the anterior lobe, was a mass of tubercles, of various hues, and from the size of a hazel-nut to a hen's egg, of a light dove-gray and yellow colour, highly vascular, except the centre of the general mass, which was friable, and of a whitish yellow colour, like a custard. Some of the exterior part cut like semi-cartilage. Some of these tubercles lying in the ventricle were easily separated from the general mass, and were friable in the centre; and in one there was a small elliptical cavity lined with a fine glossy membrane. There was not a vestige of medullary substance in the right anterior lobe nor pia mater. There were some small tubercles on the outer surface of the left anterior lobe, but not dipping beneath the cortical substance, corresponding to the absorbed portions of the skull, of a pulpy consistence and whitish yellow colour. These perforated the dura mater. It was thought there could not be less than sixteen ounces of fluid in the head. It could not be collected.

The cerebellum was healthy. Limpid fluid was effused between the dura mater and upper portion of *medulla spinalis*, but its quantity could not be ascertained.

*Abdomen.*—Intestines distended with air; hard faeces in rectum and colon; stomach empty and small; veins of omentum filled with dark blood; gall-bladder distended with dark-green bile; liver and other viscera healthy.

*Thorax.*—When opened, the lungs were found not nearly to fill the chest, but crepitous all over their substance. No fluid was contained in the pleura or pericardium, which was in other respects sound. Heart generally large; right auricle very large; and both auricle and ventricle contained black fluid blood; the walls of the latter soft, flabby, and two lines in thickness; its capacity large; and was judged four times larger than the left; valves very thin. Left auricle before cut into was about the size of a goose egg, and hard; its capacity very small, and its walls of the base six lines in thickness. The margin of the valves at the aortic aperture, for a line in breadth, was very closely studded with morbid growths, semi-cartilaginous, from the smallest point to a pin's head. *Carnea columnæ* very large, and nearly filling the cavity of the ventricle; left auricle very small and shriveled; coronary veins filled with blood; large vessels healthy. There was a growth of a solid bone, ending in a point, near three inches in length, growing from the internal condyle of the *os femoris*, point standing upwards.

From the Nouvelle Bibliotheque Medicale.

# MEMOIRE SUR UN NOUVEAU MOYEN DE HATER L'EXPULSION DU PLACENTA. Par M. le Docteur DUPARCQUE.

Some years ago, Professor Mojon of Genoa, proposed the injection of cold water into the

vessels of the placenta, in uterine hemorrhage supervening immediately after delivery: the same physician has recently suggested a similar employment of a solution of the chloruret of lime, to remove the fetor arising from the putrefaction of the placenta, in cases of retention of that organ. I am not aware that this plan has hitherto received the sanction of experience.\*

The following facts are therefore not without interest in this point of view, while at the same time they show the applicability of the proposed method, to other cases of parturition.

1st. Madame L—, æt. 20, of small stature and lymphatic temperament, was attacked with the pains of parturition on the evening of the 26th March; they were principally confined to the cervix uteri, and, notwithstanding their sharpness and frequency, were long in effecting its dilatation, so that the expulsion of the child did not take place till the evening of the following day, and was then rather the result of the voluntary efforts of the patient, than of uterine contraction. Half an hour afterwards, she complained of a sense of faintness, became pale, pulse small, and the uterus formed a soft tumour extending to the umbilicus,—symptoms evincing inertness of the organ, and internal hemorrhage. The immediate extraction of the placenta was indicated, but the os uteri had contracted, and the umbilical chord appearing too weak to support the necessary force, it was deemed a favourable case for the employment of the plan recommended by M. Mojon.

The chord was divided above the ligature, which had been previously applied, and the tube of a syringe, containing five ounces of water of the temperature of 55°, being closely adapted to one of the veins; about two-thirds were slowly injected, when the patient complaining of pain in the region of the kidneys, the operation was suspended. The pain however quickly ceased, and the remainder of the water being thrown up, immediately induced a recurrence of the pain, and uterine contractions, which continued uninterruptedly till the expulsion of the placenta and a large mass of coagulated blood,—the whole not occupying more than from five to six seconds.

2d. Another fact which has lately fallen under my observation, tends to confirm the efficacy of this plan in accelerating labour. A woman, at the termination of her fourth pregnancy, was attacked by an abundant hemorrhage, occasioned by the insertion of the placenta over the os uteri. The existence of the patient being endangered. I immediately proceeded to the delivery, which was effected by turning the first child; another was expelled by the unassisted efforts of the uterus, which, exhausted by the distention it had

\* The Journal of Foreign Medicine, vol. I. p. 90, contains a case in which the above plan was successfully adopted by Dr. Taroni of Milan.

undergone, and participating in the general debility, fell into a state of inertness, while the hemorrhage continued to the imminent danger of the patient. Six ounces of cold water were injected into the umbilical chord of the second child, with the effect of exciting uterine contractions, which, together with gentle traction of the chord, produced the expulsion of the placenta; the manual assistance was rendered necessary from the bulk of the united placenta, one of which was still farther increased in size by the injection.

*Remarks.*—The action of cold water injected into the vessels of the placenta, and thus brought into more or less immediate contact with the uterus, has a strong analogy to that of the secale cornutum. In both instances, pain, energetic contractions of the uterus, and involuntary expulsive efforts rapidly supervene, and continue without intermission till the delivery of the child in the one case, and the placenta and its dependencies in the other. These effects are more quickly produced by the injection, than by the ergot; several minutes, and occasionally, a quarter of an hour, intervening after the administration of the latter, while they instantaneously follow the entrance of the water into the placental vessels.

The injection of cold water may therefore be considered as an excellent means of inducing the detachment of the placenta, in cases of retention of this viscus, and particularly when any circumstance occurs, rendering its immediate expulsion necessary. It is preferable to tractions exerted upon the chord, which from a variety of causes are often ineffectual, and also to the introduction of the hand into the cavity of the uterus, an operation which is always painful, and not altogether without danger.

The absolute safety of the injection is not the least argument in its favour; and it should even be had recourse to immediately after the birth of the child, when from any circumstance, we may be led to apprehend inertness of the uterus, and the frightful symptoms which are its ordinary consequences; in women, for example, whose previous labours have been protracted and difficult, complicated with hemorrhage, &c.

The effects arising from the injection of cold water, being equally prompt and efficacious, may it not be productive of some advantage in certain cases of preternatural labour? such, for instance, as the following:—

1st. *Presentation of the Umbilical Chord.*—In this case, the life of the child can be preserved only by accelerating delivery, and the chance of success is proportionate to the rapidity with which this is effected; but how much delay and difficulty, often insurmountable, does it not present! The application of the forceps, or the turning of the child, cannot always be effected with the requisite celerity, and is moreover not unattended with danger both to the mother and child. Let us suppose, and the case is not of unfrequent occurrence, that the waters of the amnios

have drawn along, in their escape, the umbilical chord, and that immediately afterwards, the uterus has fallen into a state of inertness, the os tincæ being soft and dilated, and the presentation favourable; would it not be better to terminate the labour in a few seconds, by the injection of cold water into the vessels of the placenta, than to lose time in attempting to return the chord, waiting the effect of uterine contractions, applying the forceps, or performing the operation of turning?

The momentary suspension of the circulation between the child and placenta, by the ligature and division of the chord, will be evidently less injurious to the child, than the more prolonged interruption necessarily occasioned by the adoption of either of the other methods. I have always observed, that in presentations of the chord, the child is born almost exanguious, and have therefore long since adopted the practice of applying a ligature before proceeding to the artificial termination of the labour. It is true, that by this means, a state of asphyxia is induced, but from this state the child can generally be resuscitated, which is almost impossible, when this precaution having been neglected, the child is born exanguious.\* The ligature of the chord, therefore, cannot be adduced as an objection to the plan proposed. It should subsequently be divided on the placental side, and two or three ounces of cold water injected, and repeated at an interval of a few minutes, till the effect be produced.

The greatest difficulty will be, in deciding upon the portion of the chord which belongs to the placenta. This, however, may be determined by the pulsation of the artery, and the direction of the current of blood when the chord is divided.

2d. *Natural and artificial Presentation of the Inferior Extremities.*—It frequently happens in these cases, that after the delivery of the trunk, the head remains engaged in the superior strait, by reason of feebleness or absence of uterine contraction. It is necessary to have witnessed such a case, in order to appreciate correctly the difficulties attending it; the most skilful manœuvres are very often ineffectual, while it happens, that after a variety of fatiguing and useless efforts on the part of the practitioner, a strong contraction of the uterus, spontaneously effects its expulsion; not, however, until after the death of the child. Is not this a case, in which the in-

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\* The umbilical veins being very flaccid, a slight degree of compression is sufficient to arrest the circulation through them; the arteries, on the other hand, are more resisting, and consequently do not yield so readily to any obstacle. It will, therefore, occasionally happen, in presentations of the chord, that the circulation from the fœtus to the placenta will continue, while that in the reverse direction will be interrupted, and the consequence is, that the fœtus will be more or less completely drained of blood.

jection of the placenta might be advantageously resorted to?

Having witnessed the extraordinary effect of the injection of cold water into the placental vessels, I do not doubt that it will be found as effectual in the cases just indicated, as in simple retention of the placenta, and hope therefore that physicians may be induced to submit it to the test of experience.

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From the London Medical Repository and Review.

**CASE OF UTERINE HEMORRHAGE, in which Internal Compression of the Abdominal Aorta, and of the great Arterial Trunks, was adopted with Success.**

TO THE EDITORS OF THE LONDON MEDICAL REPOSITORY.

Gentlemen,

In the September number of your Journal, 1825, you were so obliging as to insert a case of presentation of the placenta, in which internal compression of the abdominal aorta was resorted to, and although unsuccessful, convinced me it was practicable. I have several times felt through the parietes of the uterus the aortic pulsation, and have compressed it firmly against the left side of the spinal column; and in the above case the intermission and great slowness of pulse were distinctly perceptible. In executing this compression, I experienced at first some difficulty, in consequence of introducing my elbow into the vagina, which prevented my revolving my arm, and, in that way, turning its palmar aspect towards the sacrum, which it is essential to do. For in this manner, a more equable and firm pressure can be given for a longer period than where the situation is constrained. I would suggest also (but it is without experience as to the fact) the use of ol. terebinthinæ, which, being smeared over the hand, would evaporate into the uterine cavity, and possibly might assist in promoting contraction of its vessels, as it is known to do in other hemorrhages. Since the period of my first case, in which this treatment was employed, I have met with one which terminated favourably. I introduce it to show the whole plan in a connected manner, for it is very satisfactory to know that no means have been left undone which could, in any way, promote the principle in practice or secure a fortunate result.

Mrs. J—, a spare but healthy woman, was delivered of her fourth child in the absence of her medical attendant, by which she was much alarmed, and, in consequence, sent for me, about two hours after the birth of the child. The hemorrhage had been extremely profuse; the blood had passed through the bed and boarded floor into the lower room, and there appeared a full quart of coagulum in the bed itself. Her state was exceedingly alarming; the voice was lost; she spoke in whispers; the lips and gums were quite pale;

no pulse distinguishable at the wrist; extremities cold; much restlessness. From these symptoms I immediately foresaw the utmost danger, and I apprised the family of my apprehensions. On examination, I found that hour-glass contraction had taken place, most completely indeed. The placenta was retained in the upper cavity of the uterus, and the intermediate space through which the funis passed, not larger than would admit the apex of the cone formed by the thumb and four fingers of the right hand. I gradually dilated this, by expanding the several fingers, and with some difficulty, extracted the placenta through the opening. The hemorrhage now became increased, with the most alarming degree of faintness; the respiration was carried on by long and slow inspirations; the external senses, as well as deglutition, were almost entirely abolished; a sort of convulsive rising of the larynx took place, and to every by-stander the patient appeared dying. I determined, therefore, without loss of time, to put in full force the following method, (fortunately there were rollers at hand:—) I banded up the arms and legs as tightly as possible, placing beneath a ball of worsted, cotton, or a pad directly upon the brachial and femoral arteries. Again introducing my hand into the uterus, which by this time had nearly recovered its proper figure, I made firm pressure upon the abdominal aorta, just above its bifurcation, with the points of my four fingers, adapting them to the convexity of the bone. At the same time, pressure was made by hand externally. I kept my fingers with great force upon the artery for about ten minutes. There was from the first a gradual improvement. Uterine pain now came on and forced the hand and a quantity of coagulum from the cavity. My own anxiety was now removed, and the woman answered my inquiries audibly. Nevertheless I did not think it prudent to release the limbs from the bandages for several hours, when they were felt very disagreeable, and the heat was restored. Cold and pressure was continued to the hypogastric region, and small doses of tinct. opii were given to abate pain; by which treatment she rapidly improved and became convalescent.

I am, Gentlemen,

Yours, &c.

WALTER JAMES.

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From the London Medical Repository and Review.

**I. THE ELEMENTS OF PHYSIOLOGY.** By FRED. BLUMENBACH, M.D. F.R.S., Professor of Medicine in the University of Gottingen. Translated from the Latin of the Fourth and last Edition, and supplied with copious Notes. By JOHN ELLIOTSON, M.D., Cantab; Fellow of the Royal College of Physicians, &c. &c. Longman and Co. London, 1828.

**II. A DISQUISITION ON THE NATURE AND PROPERTIES OF LIVING ANIMALS.** *With an Inquiry how far our Knowledge of Anatomy and Physiology is*

*consistent with the belief of a Soul and a Future State; and on the Intellectual Difference between Man and Brutes.* By GEORGE WARREN, Surgeon, pp. 144. Longman and Co. London, 1828.

Blumenbach's Elements of Physiology are so well known to the profession, as to render it quite unnecessary to give an analysis of them here; but the copious and interesting notes attached to this edition, call for some notice on our part. It is seldom that an opportunity is afforded us of reviewing works on physiology, we therefore hail the present with some degree of pleasure, especially when our remarks have to apply to the production of two such learned and scientific characters as the author and translator of the present volume. However valuable practical experience may be in the treatment of diseases, and however important to the profession and to the public at large, that all the facts connected with particular maladies should be faithfully recorded; still, unless this experience be associated with a knowledge of physiological principles, and unless the facts can be accounted for in a manner compatible with the recognised functions of the different organs, the healing art can never make much progress. Independently of the usefulness of the study of physiology as regards medicine, there is perhaps no science capable of affording the student more real pleasure, or of adapting his mind for forming more liberal ideas of human nature. Astronomy, adorned with all her sublimity, can elevate his views no higher than the stars of heaven; but physiology, although confined within a more limited boundary, leads him, nevertheless, to examine the attributes of a power associated with the nature of man, whose flights frequently extend beyond the range described by the eye of the astronomer. The properties of the human mind come as much within the range of physiology as those of the body; and he alone who understands the relative situations and the functions of the various organs constituting the animal machine, can expect to add any thing useful to the science of moral philosophy.

Every physiologist finds it necessary to take into consideration the properties of LIFE before he can proceed with his subject, but it is curious to notice the different views which authors appear to hold respecting the causes which give rise to vital phenomena. It is unnecessary to inquire at present into the different opinions which have been entertained by philosophers at different ages relative to the nature of life, or to the powers which have been more particularly attributed to it under the different appellations which it has received. It is sufficient to notice, for our present purpose, that all those who have treated of life, under whatever appellation, may be ranked in one or other of the three following classes: *first*, those who have considered life specially to consist in the function of each organ, and, generally, in the sum total of all the functions; *second*, those who have maintained that life

consists in some subtle, mobile fluid, or other principle pervading the whole system, and directing the operation of every organ; *third*, life has been considered by another class as a mere *power*, either emanating immediately from the Deity, or presiding in some other inexplicable manner over the functions of the various seats. The definitions of life under these forms have been further variously modified and reduced into diverse orders. Many of those who confound life with the organic functions, do not attempt to explain in what manner, or by what power, these functions are carried on. They rest contented with attributing the operation to some secret powers, either connected with the chemical combinations of the materials which constitute the structure, or to some other hidden properties not worth inquiring into. "The essential nature of life," says Dr. Elliotson, "is an impenetrable mystery, and no more a subject for philosophical inquiry, than the essential nature of attraction or of heat." We may also say, that the *essential nature* of oxygen, is a mystery quite as impenetrable as that of life. The essential nature of light, electricity, carbonate of lime, and of all the most subtle, as well as the grossest forms of matter, is equally as mysterious; but do the *properties* of these substances, for all that, become less a subject for philosophical inquiry? If life consists of a subtle fluid pervading the structure, let us inquire what the properties of this fluid are; how it acts in the formation of the different tissues; by what kind of properties it promotes the growth and nutrition of these tissues; how its modifications differ in the different organs, so as to adapt each for a particular function; what is the nature of the changes which take place in it while under the influence of disease; whether it be liable to a change of form, like other modifications of matter; and, lastly, what are the properties which render it liable to decay, and to cease to exist as a vital agent? These inquiries do not relate to the *essential nature* of life; they relate to properties, which, if such a thing as life exist, are essentially connected with it; and these properties, although merely objects of inference, come as much within the range of philosophical inquiry, as those of oxygen, carbon, electricity, or any other form of existence. Again, if life be a mere nominal effect, depending upon causes connected with the chemical combinations of the gross materials of which the textures are composed, let us inquire what the peculiarity of these combinations are in the different tissues; in what way they are affected by disease; in what way they contribute to the growth, nutrition, and the preservation of identity of the different organs; what way their forces become weakened in old age, and, ultimately, destroyed in death? Whatever view we take of the nature of life, there are numerous inquiries suggested concerning its properties besides those which are applicable to its phenomena, manifested in the performance of the animal functions.

It is curious to notice the inconsistency of

the arguments of those who maintain that life has only a nominal existence. Dr. Elliotson says, that "the *organic functions* depend on *life*, in the proper acceptation of the word. The word *life*, should be regarded, like the word *attraction*, or *repulsion*, as merely an expression of a fact. In this point of view, it may be as easily defined as any other expression. By *life*, we *generally* mean the *power of organized matter*, to preserve its particles in such chemical relations, as to prevent other chemical relations from inducing disorganization; or even to increase or decrease by internal appropriation and separation; to produce peculiar matters for its own purposes, &c." So the organic functions depend on *life*; *life* depends upon a certain *power*, which *power* depends upon *organized matter*. We will ask, where is the organized matter to be found on which the life of an egg depends? There must be some power here, to draw together the organic particles, in order to form the organized matter. Upon what does this power depend? Does it not depend on *life*? If not, upon what else? for there is here no organized matter. Let us propose the same questions respecting the human ovum. What gives a certain quantity of fluid, secreted in the ovaria and testes, the peculiar property of developing a heart, a liver, a brain, muscles, intestines, &c.? There is here also no organized matter, but are these effects not dependent on *life*? Can any organic function be pointed out where vital properties are more clearly manifested than in the organic formation of the structure? yet this process, as in the ova of birds, as well as in the human ovum, is carried on independently of any *organized matter*, further than that, in the latter, this modification of matter conveys materials for some other power to act upon. It would be absurd to maintain that an egg, from which, by the mere application of a certain degree of caloric, a chick may be produced, does not possess *life*; and it is for those who attribute *life* to the inherent properties connected with the chemical combinations of the structure, to show how a substance, possessing nothing resembling organization, can preserve its *life*, and by what sort of process this structure is converted into organized matter, according to their view of vitality. We do not deny that *life* acts by means of properties analogous to those which govern the affinities of tangible matter; but that there is, in living substances, a principle *not depending on* the organized form of these substances, yet *connected with* the structure, when arranged, appears to us quite positive, so far as inference can render any thing positive; because the structure upon which *life* is said to depend, could never be originally formed without such a principle. By principle, we here mean a particular modification of matter, possessing properties peculiar to itself; like every other modification. We do not inquire into the "essential nature" of this modification more than into that of matter in a visible and tangible form, in our chemical researches; for such an inquiry would be useless, inasmuch as we

know no more what the essential nature of matter is in one form than in another. Its properties constitute our only subject of examination, and we find these different in each modification or form. In one form, it presents all the characters of muscular fibre; in another, it exhibits those of cerebral substance; but neither of these will indicate any thing like vital properties, although to all appearances perfect in condition, without the addition of another modification, possessing properties different from any of those essentially connected with matter arranged in the form of these tissues.

It appears extraordinary at first view, that none of those who argue in favour of *life* being a part and parcel of the structure, or dependent on this structure, attempt to explain the manner in which the structure itself is produced; but when we consider that to account for its production would be impossible, unless it be admitted that *a thing can produce itself*, our wonder ceases that this part of the business of *life* is passed over unnoticed by these physiologists. Dr. Elliotson observes, that "by attributing *life*, the power of attraction, &c. to subtle and mobile fluids, we not only do not advance a single step, for we have still to explain what these fluids are, and how *they* obtain *their* powers, just as we had before in regard to common matter; but we make the additional mysteries of their being united with ordinary matter, and so united, that *life* appears a power possessed by *it*." Now the first remark which we shall make on this passage, is, that no analogy can be traced between *life* and *attraction*. Attraction is a property connected with matter universally, in every state or condition; whereas *life* is associated with it in very few conditions. Attraction is never lost when matter changes its form from one state to another, but bodies endued with *life* lose their vital properties frequently even before they change their form. With regard to "what these fluids are, and how *they* obtain *their* powers," the question is easily settled. Respecting what they are, it is enough to say, that they are modifications of matter; and they obtain their *powers* from their properties, like every other modification of it. As these properties are peculiar to these forms, their *powers* must, of course, be also peculiar. Whence does oxygen, carbon, and all other substances obtain their powers but from their properties? and the properties of each of these are as peculiar to it, as those of the fluids in question are to these fluids, if they may be called by that term. By ordinary matter is, we suppose, meant gross or tangible forms of matter. If this be the case, we make no mystery by supposing a more subtle form of it to become united with grosser forms. We find an analogy to this in the union of the electric fluid with different bodies. The phenomena which these bodies manifest in the electrified state is never attributed to any properties inherent in the bodies themselves, because they are known not to possess such properties in their natural state. In the same way, any

properties which the structure is found to possess when not in alliance with life, are inherent in it, and belong to it; but if, by the addition of life, it can do that which it could not before, the properties which enable it to do so are attributable to life, and not to the structure itself. As for life, by its union with ordinary matter appearing a power possessed by this matter, we have only to remark that electricity, in the same manner, when united to bodies, *appears* as if a property inherent in these bodies; but we are not to take this *appearance* as a sufficient test of their being so, because it can be demonstrated, in another way, that they are not. If it be asked what advantage we gain by admitting that life consists in a principle *sui generis*, we reply, that we gain the advantage of being able to account for the formation of the structure, for its growth, nutrition, and the reproduction of some parts of it; for the phenomena of disease, and for death, produced under various circumstances, when the structure appears to retain all its integrity. We shall not stop at present to show these advantages to be real, but some remarks upon this part of the subject may be referred to in the late numbers of this Journal.

We fully agree with the learned translator of this volume, in the following opinion:—"That fluids, as well as solids, are susceptible of life, I cannot doubt. There is no reason why they should not be so, although a person who has not thought upon the subject, may be as unable to conceive the circumstance, as a West Indian to conceive that water may, by cold, become solid. It is impossible to deny that the male or female, or both, or united, genital fluids are alive, because, from their union, or the one influenced by the other, a living being is produced, which partakes of the vital qualities of each parent." We say that we fully agree with Dr. Elliotson in the above opinion, but how he can reconcile this remark with that part wherein he says that life is nothing more than the power of organized matter to preserve its particles in chemical relations, &c., we cannot tell; for nothing like organization exists in the fluids; yet were there no vital properties evolved in union with the seminal fluid, where are they to come from to the embryo? There is no resemblance between the seminal fluid and muscular fibres, yet each possesses life, which they could hardly be supposed to do if life depended upon the chemical combination of the structural materials. There is as little resemblance between the texture of the brain and that of vegetables. In the chemical world we never find substances so dissimilar in their internal characters possess properties so analogous as those of living beings of every description. We are not going one link further than is necessary, in attributing life to some other principle than the organized structure, for as this structure cannot produce itself, something else must produce it; and this something, which must necessarily be different from the structure, we infer to be life. We, moreover, infer this to be a principle or modi-

fication of matter, possessing properties peculiar to itself, by means of which it is capable, probably by affinity, of arranging materials into the form of organized tissues, and of producing in unison with these tissues, all those phenomena observable in living beings.

According to this view of the properties of life we may account for all the phenomena manifested by it; the different tissues must be regarded as depending upon the union of vital and material properties. Every organ is made up of tissues possessing separately very different properties, but all these contribute to produce one effect. The phenomena of nature universally, so far as our knowledge extends, are carried on under the influence of opposing forces, and the functions of living objects appear to form no exception to this rule. If we take the function of secretion as an example, we find the necessity for a particular form of structure for the production of each fluid; for nervous influence; for blood; absorbent vessels, &c., the properties of each of which are very dissimilar from those of any of the others, yet, by their union, they tend to bring about one determinate end.

A particular phenomenon connected with the function of life, is *sympathy*. No satisfactory doctrine of sympathy has been as yet promulgated; for although this phenomenon has generally been traced through the medium of the nerves, yet it frequently subsists between parts not immediately connected by nervous communication. As an instance of this, we may notice titillation of the nose producing sneezing, by calling the diaphragm into action; the translation of gout from the toe to the stomach, or to the cerebral membranes, may be also noticed as an example. In fact, sympathy can neither be traced to nervous communication nor to continuity of surface in many cases; it must, therefore, be sought for in some other powers associated with living parts. Before we can point out these powers we must refer to what has been advanced above, and in former numbers of this journal, but in order to render these remarks understood, it may be here noticed that life is considered a heterogeneous principle, differing in modification in every tissue, having, consequently, different properties in each, and that upon this heterogeneity depends the different forms of structure of which the body is composed. It is obvious that tissues which are most nearly allied in structure, are endued with most analogous properties, and it is between such tissue that we most commonly observe the phenomena of sympathy take place. We may mention as examples, the metastasis of rheumatism from one joint to another; of gout; of affections of the mucous and serous membranes of distant seats sympathising with each other, and partaking of the same disease. This may be accounted for upon the principle that similar properties have similar affinities, and that they are, consequently, liable to be acted upon in the animal system by similar causes. Whether these causes derive their source from the outward world, or from ope-

rations going on internally, and changing, by a chain of causes and effects, those relations naturally subsisting between the different tissues, or between life and structure, and thereby giving origin to a new form of matter, capable of acting further, by a similar process, as a morbid cause to particular forms of tissue. Blumenbach notices this sympathy dependent on *analogy of structure and function*, but he does not attempt to explain the cause of it. Although, as he observes, "the primary and most extensive cause of sympathy must be referred to the *nerves*, and indeed chiefly to the *sensorial reaction*," still numerous instances present themselves where the phenomenon cannot be attributed to this cause; where the sympathy subsists between seats having no nervous connexion, except through the medium of the nervous centre, and where, often, there is no reason to infer this centre to exert any influence in producing it.

Dr. Copland, in his interesting notes to the last edition of Richerand's Physiology, divides sympathies into the *reflex* and *direct*, the former being chiefly referrible to the cerebral nerves and to the reaction of the sensorium, and the latter to the ganglial system of nerves. No classification will, perhaps, appear more correct than this, when we consider the difference of function which these two systems of nerves have to perform—that the former presides over the animal functions, and that the latter governs the operations of life in the organic department. From the intimate relation of the two sets of nerves, we frequently observe instances where the sympathy partakes partly of the character of the *reflex* and partly of the *direct*, and this is more particularly the case in that modification of sympathy depending on morbid action.

Bichât divides the properties of the living body into *sensibility* and *contractility*, and each of these is subdivided into *animal* and *organic*. Animal sensibility is accompanied, or rather followed, by mental perception; and animal contractility is the result of this perception, conveyed by the act of volition to the voluntary muscles. Organic sensibility is attended by no perception, and is followed by contraction quite independent of the will. When the definition of a term is given it can lead to no misunderstanding, otherwise that of *organic sensibility* does not appear to us quite expressive of the cause which gives rise to contraction in parts not under the influence of the will. The action of the heart, which is generally adduced as an instance of contraction, dependent on organic sensibility, caused by the stimulus of the blood, is, perhaps, as bad an example as could be produced, inasmuch as this organ will continue to act when not a drop of blood exists in it. The heart will be found to contract and dilate its cavities for some minutes after being totally removed from the body; its action cannot, therefore, be attributed to the *sensibility* of any impression made upon it. It, in fact, takes place from inherent properties connected with the life and structure of the organ itself, independently

of the presence and impression of the blood on the lining of its cavities. How far all the other involuntary organs will contract from inherent properties is a question which we are not fully capable of answering, but that some parts of the intestinal tube, particularly the rectum, will do so, we have had opportunities of witnessing. Before a part can be said to act from sensibility, it is necessary that it should receive the impression of some cause which it is to be sensible of, for it cannot show itself *positively* sensible of *nothing*. We have, perhaps, no term that will strictly express the cause of contraction in these *self-acting* tissues, unless *irritability* will do so, but the idea here must not be associated with that of the properties of disease. Dr. Elliotson uses the term *excitability*, but this is liable to the same objections as sensibility with regard to the properties of the involuntary organs; for although these organs are *excitable*, still they will contract without being *excited* by any evident cause.

Organic functions are common to all living beings, and may be traced down from man to the most simple form of vegetable life. It may be asked, how far these functions are necessarily dependent on nervous influence? Dr. Copland, in the notes already referred to, attributes all the properties manifested by the organic life to the influence of the ganglial or great sympathetic system of nerves. This system, in the higher order of animals, combines with the cerebro-spinal, and is distributed to every part of the body; but he considers more particularly that nervous filaments proceed from the cardiac plexus, and follow the course of the arteries to their very extremities, forming a kind of sheath round these vessels, and imparting vital properties to them, as well as to the blood itself moving through them. This view will appear more correct when we consider that, as we descend in the scale of animated beings, the cerebral and cerebro-spinal nerves become gradually less perfect, and that in the lowest order these nerves are altogether wanting, whereas the ganglial are found to exist either in filaments, or in nervous globules, from the highest to the lowest degree of vital perfection. An objection might be raised here, because vegetables, although endowed with life, still possess no ganglial or other nerves. But such an objection will be found invalid when we consider that life must be viewed according to the class of beings with which it is allied. The properties of organic life may be allied with the ganglial nerves in beings supplied with these nerves; and although vegetables are devoid of nervous filaments, still analogous may be here more generally and homogeneously diffused throughout the textures of the body. In like manner, the procreative function is carried on in the higher orders of animals by means of certain organs, namely, the testes and ovaria; whereas, in lower grades, properties destined to produce a similar end, are generally distributed through the system, and the embryo grows from the surface of the parent, or the

parent itself, when divided, will multiply itself into almost any number of animals, each enjoying the same perfection and integrity of parts as the original. Again, in animals devoid of kidneys, the blood undergoes as perfect a depuration as in those that possess these organs, although the latter would soon cease to live if deprived of them. In fact, life appears so differently modified in different classes of living beings, as well as in the various tissues belonging to each class, as to confer properties belonging essentially to one organ, or one system of organs, in one class, on the textures generally, or on some particular tissues of which these textures are composed, in another class.

Admitting, however, the ganglial nerves to preside over the functions of organic life, still we cannot admit that they are the only, or the primary source of life. Neither the seminal fluid nor the secretion of the ovarian cells can possess any thing like nervous arrangement, yet these fluids, by properties resulting from their union, are capable of forming this arrangement, as well as that of the other textures. That they are not the only source of life is probable from the circumstance that parts which are scantily supplied by them are as much alive as parts which receive a more liberal quantity of their influence. The life of the brain cannot be denied, yet it is supplied with no ganglial nerves. The ganglial nerves appear to bear the same relation to the automatic organs as the cerebro-spinal bear to the voluntary organs. They form with them a cause capable of producing a certain effect, which consists in the organic movements; in the same manner as the other nerves contribute to bring about voluntary movement in their relation to muscular fibres. Each set is endowed with vital properties peculiar to itself, for the reason that any other properties, in relation to the same modification of structure, could no more produce that effect, than *four* and *two* could produce *seven*, or any other odd number. But, if the structure be different in relation to properties also different, it may tend to produce the very same effect, in the way as *five* and *three* will make eight, as well as *four* and *four*.

We shall now proceed to consider the animal functions more particularly, and these lead us to the moral department of our nature. To these we owe the sources of all our pains and pleasures, and all our social relations with the external world. By means of these functions the eye associates with colour, the ear with sound, the tongue with those properties of bodies which constitute taste, and the same with the other senses. This power of the senses, of forming relations with the external world, Bichât calls *animal sensibility*. The power of the brain, by which it takes cognizance of this relation, may be termed *perceptibility*. It is doubtful how far what Bichât calls animal sensibility does actually exist. We have no proof of its existence; we have no proof that light affects the optic nerve, or that sound affects the auditory nerve, independently of the brain. If either of these nerves be

divided, the organ of sense which it supplies shows no longer any sensibility, though the natural stimulus be applied to it. Preternatural stimuli, such as the galvanic fluid, will, however, excite the cerebro-spinal nerves when their continuity with the brain is destroyed; but the nerves which are affected here are probably those of motion only. Query, will the galvanic fluid run from the extremities of the nerves *towards* the brain, as well as in a direction *from* the brain towards the extremities of the nerves? After death, when the muscles still manifest properties of contractility under the influence of the galvanic fluid, will the galvanic current flow in the contrary direction, and excite the *perceptible* properties of the brain? If it can do this, why does not the brain, like the muscles under such circumstances, perform its function, and display the phenomena of thought and volition, if it be the natural function of this organ to do so? Mr. Warren, the author of one of the works whose titles stand at the head of this article, states that—

“In parts endued with sensibility, the nerves are pervaded by electric fluid, and the circulating blood does excite to action this fluid at the extremities of the nerves, and thereby occasions an electric vibration throughout the whole course, up to that part of the brain or spinal marrow in which is seated the perceiving power. Such a state constitutes sensibility, for any further impression falling upon the part occasions a sufficient further motion, to impress upon the perceptibility of the animal a sensitive idea. It is to be understood that the nerves are conductors of vibrations, and not that they do themselves actually vibrate.

“In support, it is to be observed, that experiment proves the nerves to be conductors of electricity, and they are enveloped in a condensed membranous coat, which is a non-conductor of electricity; so far the natural arrangement appears conformable to the proposed truth. The next question which naturally arises is, whether an electric shock be capable of producing that variety of sensation which the animal is capable of experiencing. Here the proposition has the required support, since an electric shock passed through the skin, gives the sensation of an impression of contact through the eye of light, through the ear of sound, through the tongue of taste, and through the nostril of smell.”

If we can find no stronger proof in support of the opinion that sensation is dependent on electricity, than that it gives a sensation of contact when the shock is passed through the skin, of light through the eye, &c. we may say, with the same degree of propriety, that a *piece of rotten wood* is a conductor of sensation, for it has all these properties in relation to our senses. If electricity be the conductor of sensation (we mean the *immediate* conductor, by its motion through the nerves,) how is it that the sensation conveyed through the different organs produces such a different impression on the brain? We are not aware

that there exists more than *one* modification of the electric fluid, and if this be the case, we can see no reason, according to this electric theory, why the olfactory nerve should not be as susceptible of colour as the optic, or why the optic should not convey sound to the brain as well as the auditory; or why sound should not excite the electricity of the nerve of taste as well as it does the nerve of the seventh pair. We can no more comprehend how the nerves can conduct vibrations without vibrating themselves; for something must vibrate before there can be a vibration, and if the nerves do not vibrate what is it that does?

We shall find the sum total of Mr. Warren's arguments in the following paragraph:—

"The opinions afforded by the foregoing reasoning are—that in every living animal there are certain faculties or attributes, to which, when considered abstractedly, may be appropriated the term 'anima' or 'soul'—that life consists of a relation between *such attributes* and the physical laws of the material world—that the body is the medium or instrument by which such relation is accomplished—that sensibility, muscular contractility, the organic movements, and animal combinations, depend upon the agency of electric fluid—that *the ulterior use of food-taking is the supply of electric fluid*—that the rapid circulation in animals is always in accordance with their degree of sensibility—that in the operation between the arterial and nervous systems, as well as muscular contraction and organic movements, heat is evolved—and *that the use of the lungs is to cool the body.*"

Mr. Warren does not attempt to account for the formation and growth of the structure. As the use of the food, according to his view, is the production of electricity merely, which is to act as the medium of relation between the soul and the material world, we suppose that the body derives its growth solely from the *air* which it inhales, for we know of no other materials than air or food which it can convert into that purpose. If electricity is life, as Mr. Warren and many others maintain, why, in the name of fortune, should we ever die, when we have the means of supplying ourselves to our heart's content with this principle? He who identifies electricity with life must be a very narrow-minded philosopher indeed. Because, forsooth, electricity can excite the muscles into action, and can substitute the properties of life in one or two more things, electricity must, therefore, be life; or, which is the same thing, life must be electricity. We may as well assert that spirits of wine, because it finds its level, is limpid, will moisten substances, &c., is absolutely nothing more nor less than water, for does water not possess the very same properties? Electricity is life, because it appears in the form of light to the eye; so does fire the same; electricity is life because it imparts the sense of sound to the ear, so will also the collision of two bits of iron; electricity is life, because it gives an impression of taste to the tongue; so will citric acid; it is life because the nose can swell it; so it

can a rose; it produces a sense of contact with the skin—will not every other substance do the same thing? and, as we said before, are not all these properties found connected with rotten wood, or rotten fish? But it is said that electricity excites the muscles into action; will not a bit of salt, or the prick of a needle do the same thing? Will it keep the body alive, let it be ever so intense, after all the blood is drained from the vessels? Will it keep it alive after the spinal marrow is divided in the neck? because the electric fluid will run through just the same when the two ends are brought into contact. Will it re-establish the relations of the brain with the external world after natural life has become extinct? If electricity cannot do these things, and a great many more, we see no reason to identify it with life. But the most extraordinary property of this electric life is that it becomes extinct when another quantity of the same principle is added to it. The identity of lightning with electricity is universally acknowledged, yet lightning has, somehow, the property of destroying life without inflicting any mechanical violence on the structure. It is rather strange that it should do this if the two principles be the same. The volume or sum total of any other substance increases according to the quantity added to it, but it appears to be quite the contrary with life in this case. In fact, the absurdities of identifying life with electricity are almost without end, besides that the electric fluid could not, by any known property connected with it, build up the various tissues, and arrange them into their natural and relative situations. It is absurd to suppose that electricity, or *any other homogeneous principle*, could produce such a diversity of effects as are observable in a living body. What resemblance is there between the contraction of a muscle and the secretion of urine? Or between the contraction of the urinary bladder, and sensation, or volition? Electricity, in relation to inanimate bodies, never manifests such dissimilar properties, however dissimilar those of the bodies themselves may be.

The organs of the animal functions, like those of the organic functions, must possess, each, vital properties peculiar to itself—properties depending upon a certain modification of vital principle. The terms *organic* and *animal*, can only be considered in a relative sense; for the life of the brain, or of the cerebral nerves, is as much organic to these tissues, as those of the liver are to that organ, or those of the ganglionic nerves are to those nerves. The brain is preserved, nourished, and the corpuscles of which it is made up, are renewed under the influence of its own peculiar modification of life, in the same way as the heart, kidney, or any other organ is by a different modification; in a similar way, also, its functions are performed by the united properties of this form of life, and those of the cerebral mass, whose particles have been previously arranged by the affinities of the former properties. The life of all the organs act by analogies, but not identical, properties. That

of the nerves of sense is adapted for sensation, and even here life is modified according to the kind of impression which the organ is intended to receive, for we know that the olfactory nerve has no relation to light, nor has the auditory any relation to odours.

Having offered these remarks, we shall now inquire into the office of life in relation to the brain, and we cannot explain our views more clearly than in Dr. Elliotson's own words.

"The animal functions demonstrate mind. This is seated in the brain, to which the spinal marrow, nerves, and voluntary muscles are subservient. Mind is the functional power of the living brain. As I cannot conceive life any more than the power of attraction, unless possessed by matter, so I cannot conceive mind unless possessed by a brain, or by some nervous organ, whatever name we may choose to give it, endowed with life. I speak of terrestrial or animal mind; with angelic and divine nature we have nothing to do, and of them we know, in the same respects, nothing. To call the human mind positively a ray of the divinity, (*Divinæ particula aureæ, Ex ipso Deo decerptus, Ex universa mente delibatus*) appears to me absolute nonsense. Brutes are as really endowed with mind—with a consciousness of personality, with feelings, desire, and will, as man. Every child is conscious that it thinks with its head, and common language designates this part as the seat of mind. Observation shows that superiority of mind in the animal creation is exactly commensurate with superiority of brain; that activity of mind and of brain are coequal; and that as long as the brain is endowed with life, and remains uninjured, it, like all other organs can perform its functions, and mind continues; but, as in all other organs, when its life ceases, its power to perform its function ceases, and the mind ceases; when disease or mechanical injury affects it, the mind is affected—inflammation of the stomach causes vomiting, of the brain, delirium; a blow upon the loins causes nephritis or hæmaturia; a blow upon the head stuns; if originally constituted defective, the mind is defective; if fully developed and properly acted upon, the mind is vigorous; accordingly as it varies with age, in quality and bulk, is the mind also varied—the mind of the child is weak and very excitable; of the adult vigorous and firm; and of the old man weak and dull, exactly like the body; and the character of the mind of an individual agrees with the character of his body, being equally excitable, languid, or torpid, evidently because the brain is of the same character as the rest of the body to which it belongs:—the female mind exceeds the male in excitability as much as her body; the qualities of the mind are also hereditary, which they could not be, unless they were, like our other qualities, corporeal conditions; and the mind is often disordered upon the disappearance of a bodily complaint, just as other organs, besides the brain, are affected under similar circumstances. The retrocession of an eruption may affect the lungs, causing asthma, the bowels, causing enteritis, or the brain,

causing insanity;—phthisis and insanity sometimes alternate with each other; just like affections of other organs; the laws of the mind are precisely those of the functions of all other organs;—a certain degree of excitement strengthens it, too much exhausts it, physical agents affect it, and some specifically, as is the case with other functions, for example, narcotics. The argument of Bishop Butler, that the soul is immortal and independent of matter, because in fatal diseases the mind often remains vigorous to the last, is perfectly groundless, for any function will remain vigorous to the last, if the organ which performs it is not the seat of the disease, nor much connected by sympathy, or in other modes, with the organ which is the seat of the disease;—the stomach often calls regularly for food and digests it vigorously, while the lungs are almost completely consumed by ulceration. All the cases that are adduced to prove the little dependence of the mind upon the brain, are adduced in opposition to the myriads of others that daily occur in the usual course of nature, and are evidently regarded as extraordinary by those who bring them forward. An exact parallel to each, may be found in the affections of every other organ, and each admits of so easy an explanation that it may be always truly said 'Exceptio probat regulam.'"

It will be said that the above passage savours strongly of materialism. The object of every rational being ought to be to arrive at truth, without any regard to popular opinion or prejudice. If the physical facts are as stated, all the cry of materialism in the world cannot alter them. They only show that, as the Deity is omnipotent, he can display his power through the medium of matter as well as through that of spiritual existence. If any one rests his hope of a future existence solely on the belief that the mind is immortal, he perhaps places it upon an insecure foundation. If the ground pointed out by Dr. Elliotson in the following passage is not safe enough to support our belief, we fear that we cannot have a safer shown us, either from scripture, or from the power of reason.

"In contending that the mind is a power of the living brain, and the exercise of it the functions of that organ, I contend for merely a physical fact, and no Christian who has just conceptions of the Author of Nature will hesitate to look boldly at Nature as she is, lest he should discover facts opposite to the pronouncement of his revelation; for the word and the works of the Almighty cannot contradict each other. Lord Bacon accordingly, in a very memorable part of his writings, directs the physical inquirer to be uninfluenced by religious opinions, as the more independently truth is pursued, the sooner will it be gained; and the sooner will the real meaning of the divine statement of natural things, and the conformity of this to physical fact, be established.

"The assertion, however, that the mind is a power of the living brain, is not an assertion that it is material, for a power or property of matter cannot be matter.

“Neither is it an assertion that this power cannot be a something immortal, subtle, immaterial, diffused through and connected with the brain. A physical inquirer has to do with only what he observes. He finds this power, but attempts not to explain it. He simply says the living brain has this power, medullary matter though it be. Seeing that the brain thinks, and feels, and wills, as clearly as that the liver has the power of producing bile, and does produce it, and a salt the power of assuming a certain form, and does crystallize, he leaves others at liberty to fancy an hypothesis of its power being a subtle, immaterial, immortal substance, exactly as they fancy life to be subtle fluid, or, perhaps, though very extraordinarily, the same subtle fluid (if subtlety is immateriality and immortality,) elucidating the subject no more than in the case of life, and equally increasing the number of its difficulties; as though we were not *created* beings or not altogether ignorant what matter is, or of what it is capable and incapable; as though matter exhibited nothing but extension, impenetrability, attraction, and inertness; and as though an Almighty could not, if it seemed good to him, have endowed it, as he most evidently has, with the super-addition of life, and even of feeling and will.

“Nor does this assertion imply that the resurrection from the dead is impossible or even improbable. The physical inquirer finding the mind a power of the brain, and abstaining from hypothesis, must conclude, that, in the present order of things, when the brain ceases to live, the power necessarily ceases—that in the language of scripture, dust we are, and unto dust we all return,—that our being is utterly extinguished, and we go back to the insensibility of the earth whence we are taken. Our consciousness of personality can afford no reason for imagining ourselves immortal and distinct from earth, more than brutes; for this the fly possesses equally with the philosopher about whose head it buzzes. The moral government of the world, the sublime reach of our acuteness, the great improvableness of our characters—

‘———this pleasing hope, this fond desire,  
This longing after immortality,  
———This secret dread and inward horror  
Of falling into naught,’

have been thought to completely harmonise with a life hereafter, but certainly fall so short of proof, as to have left the wisest of antiquity—Solomon, Socrates, Cicero, &c.—in uncertainty, when they saw how death reduces us to our pristine elements. The hope of immortality inspired by such reflections, assisted by the desire of explaining every thing in some way or other, first, I apprehend, made men attempt to find, in the imagined ethereal essence of the soul, a reason for our not totally perishing as our senses would lead us to suppose. But, because we refuse to listen to a mere hypothesis respecting spirit, we are not *necessarily* to deny the resurrection. For if a divine revelation pronounce that there

shall be *another order of things* in which the mind shall exist again, we ought firmly to believe it, because neither our experience nor our reason can inform us what will be hereafter, and we must be senseless to start objections on a point beyond the penetration of our faculties. The scripture so pronounces,—not that we are naturally immortal, but that ‘in Adam (by nature) all die,’—have our being utterly extinguished, and in another order of things—when the fashion of this world shall have passed away and time shall be no more, that in Christ (by the free, additional, gift of God, granted through the obedience of Christ, but, consequently, *by a miracle*, not by our nature,)—we shall all again be made alive. St. Paul declares the resurrection to be a ‘*mystery*.’ it must in truth be a *miracle*, and therefore the inquiry ‘how can these things be,’ altogether fruitless. The miracle of Christ’s resurrection, to which the scriptures refer us as the foundation of the hope of a future state, would not have been necessary to convince us of a necessary truth, discoverable by sense and reason. That the promises of the New Testament are the proper and only foundation of our hopes of immortality was the opinion of the late Regius Professor of Divinity in the University of Cambridge, whose powerful intellect and sincere love of truth render his opinions weightier than the decrees of councils. “I have no hope of a future existence,” says he, “except that which is grounded on the truth of Christianity.” While those are wrong who think there can be any thing like an argument against a future life in another order of things, if declared by a revelation, it is strange that others should think it necessary to attempt rendering the pronunciations of scripture more probable, and that by an hypothesis, which is at best but the remains of unenlightened times, and should require any assurance besides that of the gospel, which they read, “has brought life and immortality to light.” They should reflect that the belief of an immaterial substance removes no imagined difficulty, as it is the peculiar doctrine of scripture, in distinction to that of all the heathen philosophers and people, that the resurrection will be positively of *body*—that in our *flesh* we shall see God, and that therefore our minds must appear as much a property of body hereafter as at present.

“Thus only, the christian doctrine of a future state is reasonable. The heathen doctrine was grounded on the supposed inherent immortality of a supposed substance distinct from the body. The christian doctrine teaches the resurrection of what we obviously are—bodies, and that through a miracle of the Almighty.”

Blumenbach classifies the faculties of the mind into *perception, attention, memory, imagination, abstraction, judgment, and reason*, in the first or *intellectual* order. There is another order relating to *appetency*. This consists of *desire, aversion, and volition*. This is, perhaps, as imperfect an arrangement as could well be contrived. Dr. Copland gives a

more extended arrangement in the notes already mentioned. This contains all the mental faculties as they are manifested in their compound form, but as the classification is founded upon purely metaphysical principles, if the brain be really the organ of the mind, it is liable to all the objections applicable to similar arrangements. Whether the phrenological system of Drs. Gall and Spurzheim be perfect or not, it appears to us that the principle on which their doctrines rests, is the only one founded on nature, as relates to the faculties of the mind; for unless these faculties can be traced to some physical organ, it is not likely that their seat can ever be discovered by metaphysical researches. So much has been said on this subject, as to render it fruitless to attempt to adduce any new argument either in favour or against the doctrine. Its truth or fallacy must be decided by facts alone; and so far as these have been hitherto adduced, every one divested of prejudice, and who takes the trouble to examine them, must confess that they tend to confirm the truth of it. Dr. Elliotson has given a long note in favour of the phrenological classification of the mental faculties, and he shows that the experiments of Majendie, Fontana, Fleurens, Rolando, and others, do not tend to invalidate that arrangement.

We shall next proceed to the doctor's notes on the motion of the blood, and here the principal subject of discussion is the thoracic vacuum, so much insisted on by Dr. Carson, of Liverpool, and several other physiologists. Dr. Carson, supposing that the weight of the blood, the length of its course, the various obstacles from angles, friction, &c., with which it meets in the vessels, renders it necessary that some other power than that of the left ventricle of the heart should assist in the circulation, has assigned that power to a vacuum which he supposes to take place in the chest during inspiration. As a further assistance still, there is, moreover, a vacuum supposed to take place in the cavities of the organ itself during each diastole. It is useless to build any theory upon a supposititious fact, when that fact can be either proved or disproved by a simple experiment. To show that the propelling power of the left ventricle, or, at least, some power situated between the left auricle and the right auricle, in the line of circulation, is sufficient to carry the blood round to the latter cavity, without any suction power to attract it in this direction, let a ligature be placed round the root of the inferior cava, close to the heart; then let an incision be made into the vessel, close to that ligature, on the further side of it from the heart, and it will be seen that the blood will not only run out by the force from behind, but it will be thrown to some distance from the orifice. This will happen let the animal be held in any position we like. This experiment we have several times performed on rabbits, and we never noticed any lack of *vis a tergo* to make the animal bleed to death in a few seconds. This fact, alone, proves that

there is no necessity for a suction power to carry the blood round, which is one step towards proving that such a power does not act on it, for nature seldom does things in vain. In the next place, is there a vacuum produced by the expansion of the right auricle? and if there be, can it have any influence on the blood in the veins? Before the first question can be answered, it is requisite to decide whether or not the auricle dilates itself, or is dilated by the blood being driven into it? We must keep in mind that the right auricle is scarcely anything more than a membranous receptacle, or sinus, possessing but few muscular fibres. Admitting, therefore, that it expands by the elasticity of this structure, (which is the only inherent power of dilation that it can possess, for there is no mechanical contrivance connected with it for that purpose,) the force with which such a structure could do so, would be very trifling; not enough, indeed, to overcome the pressure of the atmosphere on it communicated through the lungs and diaphragm; for it must be remembered that the auricle expands during expiration, when it is acknowledged that no vacuum exists in the thorax. Now, we have frequently taken hold of the heart during its regular action, at least, as regular as it could act immediately after opening the chest, and from the very small degree of pressure exerted by the ventricles even against the hand, we are fully satisfied that the dilating force is next thing to nothing. It amounts to no more than a mere relaxation of the fibres, caused by the elasticity of the structure, after this elasticity has been reduced below its medium, by the vital contractility of the muscular fibres. If the dilating force of the ventricles is so small in degree, that of the auricles must be, at any rate, ten, if not twenty times, smaller, if we may infer from the quality and thinness of their walls. Indeed, when the auricle dilates, it hardly produces a sensible pressure against the hand, when no blood is allowed to flow into it, as when the two cavæ have been secured by ligatures; still it has enough force to dilate a little, but not nearly to the extent that it expands, or rather is expanded, when the blood is allowed to flow regularly through it. When a ligature is tied round the vena cava inferior, close to the heart, the force from behind is sufficient to dilate the vessel below that ligature very considerably beyond its common diameter, and as the right auricle partakes a good deal of the qualities of veins, the same force will expand that cavity much beyond the point it can attain by its own inherent power of dilation when void of blood. Any one, who will take the trouble to do so, may satisfy himself of the truth of these facts, and few experiments are less complicated, or attended with less difficulty.

It would appear then, that the suction power of the right auricle, if anything, is next to nothing, and that the blood will flow into that cavity, and would flow beyond it by the mere propelling force of the left ventricle. But it has been said that the right ventricle adds to

this suction power; for, as maintained, the right ventricle, in dilating, forms a vacuum, and draws the blood from the auricle; consequently the column from the veins follows, and supplies the place of this in the auricle. Those who suppose they can discover this power should consider, that, when the *dilatation* of the *ventricle* takes place, the *auricle contracts*; the column from the veins cannot, therefore, enter the latter cavity while the supposed suction power of the former exists. During the dilatation of the right ventricle, the blood in the *cava*, far from being drawn into the auricle at this time, regurgitates, from the contraction of this cavity, and produces a regular pulsation in these veins; and here the pulse is sensible to the eye, owing to the highly elastic property of the veins in the transverse direction, whereas it is not visible in the arteries, the root of the aorta excepted. It is doubtful, however, whether this venous pulsation is produced by *regurgitation*, in the strict sense of the word, from the auricle; for the venous diastole is more probably produced by the force from behind propelling the blood towards the auricle at the very instant that this cavity is shut, distending, consequently, the venous trunks a little beyond the medium of their elasticity; and as soon as the systole of the auricle is over, the coats of the venous trunks resume their medium of caliber, and with the assistance of the regular and continued *vis a tergo*, press the column into the auricle, so as to dilate it in the same manner as the veins were dilated the instant before. The auricle at the same time, being now in a relaxed state, the contractile influence having ceased for an instant, has a tendency to expand a little, from the inherent elastic properties connected with its structure.

Now, with respect to the thoracic vacuum. We shall not at present inquire whether or not a vacuum does take place in the chest during inspiration, but will admit the fact, and see what influence it can have over the motion of the blood in the veins. In man, the veins connected with the superior cava would, most of them, during by far the greater part of his life, empty themselves by the force of gravitation, so that very little more power is necessary here, from the left ventricle, than is sufficient to carry the blood to the capillaries. This is certainly done against the force of gravity, but no thoracic vacuum is required to return the blood from this part of the venous system. Such a vacuum, therefore, if it have any effect, is more particularly required by the *inferior cava* and its branches. Now the atmosphere presses in all directions, with a force equal to about fifteen pounds on every square inch. There are very few human bodies whose surface is more than 3,500 square inches. Taking the surface at this measurement, and allowing that 15 pounds presses upon every square inch, the whole weight of atmosphere supported by the body, will be 52,500 pounds. Now the next question is, how many square inches are the whole

surface of the air-cells of the lungs equal to? Lieberkühn makes the surface of these cells equal to 1,500 square feet, or 216,000 square inches, being nearly *sixty-two* times greater than the surface of the whole body, bearing, therefore, a column of atmosphere weighing sixty-two times greater than the surface of the skin does. Lieberkühn's calculation is, probably, however, considerably exaggerated respecting the extent of surface possessed by the air-cells. When we consider the minuteness of the air-cells, and that they are so numerous as to be able to contain 200 cubic inches, or more, of atmospheric air during inspiration, there is some reason to infer that their surface is equal to, if not greater than, that of the skin, and that they consequently bear a weight equal to that which presses on the surface of the body. We will allow it to be equal only, but the probability is, that it is greater. Now as soon as a vacuum is formed in the chest, the pressure of the atmosphere on the surface of the air-cells expands these cells *at the very instant* that the ribs are raised, or that the diaphragm descends, and the vacuum is filled up. In fact, we cannot expand the chest without taking in air, for if we close the mouth and nostrils, and attempt to raise the ribs, the diaphragm is pressed up involuntarily by the abdominal muscles, and a vacuum is prevented from taking place. If, as we said before, the quantity of pressure be allowed as equal on the surface of the lungs and on that of the skin, there is still the weight of a column of blood, reaching from the extremities to the heart, to be raised against its own gravity by the pressure on the latter surface. If, therefore, we take the *absolute* pressure on both surfaces to account, the blood in the lower cava must necessarily stand still upon its own gravity, unless moved by some other power. But the fact is, that, although the pressure of the atmosphere be equal to 52,500 pounds on the surface of the body, still, supposing that a *perfect vacuum* did take place in the chest, the propelling pressure of the blood in the cava, where it enters the chest, *will be only as fifteen pounds to the square inch, according to the diameter of the caliber of this vessel*; and as a transverse section of the inferior cava, when quite full, would not present a surface equal to three-fourths of a square inch, a weight here equal to about eleven pounds and a quarter, even in a perfect vacuum, would balance all the pressure of the atmosphere on the surface of the body. But of these eleven pounds and a quarter, we have still to deduct the weight of the column of blood. Let us suppose the body to contain ten pounds of blood, and that it is equally distributed in the arteries and veins. There is generally more in the veins, but we will take it as equal. Out of the five pounds contained in the veins, we will suppose three and a quarter (which, if any thing, is less than the exact proportion) to be in those connected with the *inferior cava*. Now, when these three pounds and a quarter are deducted from the eleven pounds and a quarter, it leaves a balance of eight

pounds in favour of the pressure of the atmosphere against three-fourths of a square inch of vacuum (for we still suppose the thorax to be in a state of vacuum.) As the pressure of the atmosphere on the surface of the air-cells in the lungs, is equal to fifteen pounds to the square inch, it imparts of course a weight equal to eleven pounds and a quarter to every three-fourths of a square inch. When we now deduct the eight pounds (the weight to which the pressure in the inferior cava is equal) out of these eleven pounds and a quarter, (the weight on equal surface in the lungs) it leaves a balance of three pounds in favour of the pressure in the lungs, over that on the blood in the cava. It follows then, that the vacuum produced in the chest, during inspiration, is filled instantaneously by the pressure of the atmosphere in the lungs, with a force equal to eleven pounds and a quarter, whereas the force with which the pressure on the surface of the body drives the blood towards it, through the vena cava, is only equal to eight. In other words, the proportions are as 3 to 0 in favour of the vacuum being filled by air, rather than by the blood from the veins, even if these vessels did not possess any property of resisting the pressure of the atmosphere by the elasticity of their coats. We have here also only taken the simple weight of the column of blood into calculation, whereas its friction against the vessels and the angles, and the counter-streams which the current meets, ought likewise to be taken into account.

Our limits will not permit us to follow this subject further at present, otherwise we could show, both from experiments and from deductions founded on natural laws, that the influence of this vacuum on the motion of the blood in the veins is a thing next to, if not a perfect, nullity. It should be considered in this inquiry, that the blood, like every other fluid, will find its level in the vessels; that, as these vessels adapt their caliber to the quantity they contain, so as to preserve themselves always full, the fluid will rise in the veins to the same height as in the arteries, namely, up to the heart, by the same laws as it does in any other vessels which communicate, or in subterraneous canals; and that, consequently, the force required of the left ventricle to move the blood through the vessels situated below the level of the heart, is not necessarily much more than sufficient to overcome the inertia of the column, and the obstruction which its motion meets with from the angles and curves of the passages.

We cannot follow Dr. Elliotson through his very interesting and instructive notes on the chemical changes which take place in the blood in the lungs; on the source of animal heat; on growth, nutrition, absorption, secretion, conception; on the difference of the sexes; on the genital functions, and numerous other very interesting subjects. His remarks on the varieties of mankind, are also both entertaining and interesting. He considers it probable that all the human race are descend-

ants of two individuals, and that the varieties now observed on different parts of the globe, have been caused by accidental circumstances; by the nature of the soil, climate, food, &c.

"In favour of the opinion that we are all brothers, it may be urged—1. The universal simplicity of Nature's causes would induce us to imagine that, as, if the varieties among us are accidental, two individuals were evidently sufficient for the production of the rest of mankind, no more than two were originally created. Nor should I deduce a contrary presumptive argument from the length of time during which immense portions of the earth must have thus remained unpeopled. One of Nature's objects seems the existence of as much successive life as possible, whether animal or vegetable, throughout the globe. For this purpose, every species of animal and vegetable possesses an unlimited power of propagation, capable of filling the whole world, were opportunity afforded it. The opportunities of exertion are indeed very scanty, compared with the power: climate, soil, situation, may be unfavourable; one vegetable, one animal, stands in the way of another; even the impediments to the increase of some, act through them as impediments to others. The incessant tendency of the power of multiplication to exert itself, seizes every opportunity the moment it is presented, and thus, though every living object has a fixed term of existence, and may be carried off much earlier by innumerable circumstances, all nature constantly teems with life. The slow increase of mankind could not interfere with this apparent object of nature; the deficiency of our race must have invariably been fully compensated by the opportunities which it afforded for the multiplication of other existences: for that man alone was not designed to enjoy the earth, is shown by the vast tracts of land still but thinly peopled. The infinitely rare opportunities afforded for the maturity of the intellectual and moral powers born with every human being, may afford still greater surprise than the extent of country unoccupied by man.

"2. *Analogical and direct facts* lead to a conclusion that none of the differences among mankind are so great as to require the belief of their originality.

"Animated beings have a general tendency to produce offspring resembling themselves, both in mental and corporeal qualities.

'Fortes creantur fortibus et bonis;  
Est in juvenis, est in equis patrum  
Virtus: nec imbellem feroces  
Progenerant aquilæ columbam.'

"An exception occasionally occurs, much more frequently, we are told, in the domestic than the wild state,—the offspring differs in some particular from the parents; and by the force of the general tendency transmits to its offspring its own peculiarity. By selecting

such examples, a breed peculiar in colour, figure, the form of some one part, or in some mental quality, may be produced. Thus, by killing all the black individuals which appear among our sheep, and breeding from the white only, our flocks are white; while, by an opposite practice pursued in some countries, they are black: thus a ram accidentally produced on a farm in Connecticut, with elbow-shaped fore-legs, and a great shortness and weakness of joint indeed in all four extremities, was selected for propagation, and the *argali* breed, unable to climb over fences, is now established: thus some breeds of hares have horns like the roebuck: the Dorking fowl has two hind claws; and fowls, in short, are bred in every conceivable variety. Individuals, distinguished from others by no greater differences than those which thus spring up accidentally, cannot be supposed to belong to a separate species. Upon the comparison of these differences, depends the analogical argument first employed by Blumenbach. Finding the ferret (*mustela furo*) to differ from the pole-cat (*m. putorius*) by the redness of its eyes, he concludes it is merely a variety of the same species, because instances of this deviation are known to occur accidentally in other animals; but he concludes the African elephant is of a species distinct from the Asiatic, because the invariable difference of their molar teeth is a description which naturalists have never found accidental. Now there exist among mankind no differences greater than what happen occasionally in separate species [varieties?] of brutes.

"The colours of the animals around us, horses, cows, dogs, cats, rabbits, fowls, are extremely various—black, white, brown, grey, variegated.

"The hair of the wild Siberian sheep is close in summer, but rough and curled in winter; sheep in Thibet are covered with the finest wool, in Ethiopia with coarse stiff hair; the bristles of the hog in Normandy are too soft for the manufacture of brushes; goats, rabbits, and cats of Angouri, in Anatolia, have very long hair, white as snow and soft as silk.

"The head of the domestic pig differs as much from that of the wild animal, as the negro from the European in this respect; so the head of the Neapolitan horse, denominated ram's head on account of its shape, from that of the Hungarian animal remarkable for its shortness, and the extent of its lower jaw; the cranium of fowls at Padua is dilated like a shell, and perforated by an immense number of small holes; cattle and sheep in some parts of our own country have horns, in others not; in Sicily sheep have enormous horns; and in some instances this animal has so many, as to have acquired the epithet polyceratous.

"The form of other parts is no less various. In Normandy, pigs have hind legs much longer than the fore; at the Cape of Good Hope, cows have much shorter legs than in England; the difference between the Arabian, Syrian, and German horses, is sufficiently

known; the hoofs of the pig may be undivided, bisulcous, or trisulcous.

"These are regarded by naturalists as but accidental varieties, yet they equal or surpass the varieties existing among mankind. We are consequently led by analogy to conclude, that the differences of nations are not original but acquired, and impose no necessity for believing that more than one stock was at first created.

"3. *Direct facts* harmonise with this conclusion. All races run insensibly one into another, and therefore innumerable intermediate examples occur where the distinction between two varieties is lost. Again, no peculiarity exists in any variety which does not show itself occasionally in another. Many instances of these facts have been already related. The difficulty of regarding the negro as of the same stock with ourselves vanishes, on viewing these circumstances, and on reflecting that he and ourselves are two extremes, one of which may have sprung from the other by means of several intermediate deviations, although experience may not justify us in supposing any single deviation of sufficient magnitude. Lastly, both the males and females of all the varieties breed together readily and in perpetuity—an assertion which cannot be made in regard to any different species of brutes."

The following account, given by Volney, and confirmed by Mr. Crompton, a friend of Dr. Elliotson's, of the power of climate upon the generative function, is very curious and singular.

"During five hundred and fifty years that there have been Mamlouks in Egypt, not one of them has left subsisting issue; there does not exist one single family of them in the second generation; all their children perish in the first or second descent. Almost the same thing happens to the Turks; and it is observed that they can only secure the continuance of their families, by marrying women who are natives, which the Mamlouks have always disdained. Let the naturalist explain why men, well formed, and married to healthy women, are unable to naturalize on the banks of the Nile, a race born at the foot of Mount Caucasus! and let it be remembered, at the same time, that the plants of Europe in that country are equally unable to continue their species! Some may refuse to believe this extraordinary fact, but it is not on that account less certain; nor does it appear to be new. The ancients have made observations of the same nature: thus, when Hippocrates asserts, that among the Scythians and Egyptians, all the individuals resemble each other, though they are like no other nations; when he adds, that in the countries inhabited by these two races of men, the climate, seasons, elements, and soil possess an uniformity nowhere else to be found, does he not recognize that kind of exclusion of which I speak? When such countries impress so peculiar a character on every thing native, is it not a

reason why they should reject whatever is foreign? It seems, then, that the only means of naturalizing animals and plants, would be to contract an affinity with the climate, by alliance with the native species; and this, as I have before said, the Mamlouks have constantly refused. The means, therefore, by which they are perpetuated and multiplied, are the same by which they were first established; that is to say, when they die, they are replaced by slaves brought from their original country.”

We wish our limits had permitted us to notice more of the subjects treated by Dr. Elliotson in these notes, but we are compelled to stop short against our inclination, as we have neither time nor room to proceed further.

With respect to Mr. Warren's work, it is particularly interesting, and it proves the author to possess an ingenious mind, although we think many of his views are founded in error.

From the London Medical Gazette.

**SINGULAR CASE OF DISEASE, occupying the whole extent of the Abdomen on one side, and the Pelvis, external to the Peritoneum.**  
By S. D. BROUGHTON, Esq. Surgeon to the 2d Life Guards, &c. &c.

W. B. æt. 29, a musician in the band of the 2d Life Guards, had, during more than a year past, gradually fallen away in condition, and suffered latterly repeated attacks of violent pain, referred to the back and hip of the left side, not higher up than the loins. His appearance was pallid and unhealthy, and his bowels costive and irregular; but the appetite natural. Frequently he was much annoyed with distention from wind, which was generally removed by the introduction of a clyster pipe. Relief was always obtained by purgatives and anodynes. His emaciation and debility of late obliged him to be confined to the hospital, where he had frequently received temporary relief; but getting better, he was allowed to return to his lodgings, deriving benefit from the open air. During the spasmodic attacks, the heart and arteries would beat high and rapidly, and pain be complained of in the lower bowels on the left side, and loins. On examination, the belly was always soft and yielding, and without tenderness on pressure; nor was any enlargement detected, but some occasional distention from wind. The stools were always of a natural appearance; the tongue clean, but rather dry; and the skin moist. The urine was sometimes turbid, not high coloured, and occasionally contained a predominance of acid.

Between the attacks of pain he was comfortable and cheerful, though looking as if labouring under disease. The quantity of urine passed during the twenty-four hours was apparently much as usual. Latterly there was at times pain referred to the left groin, but no affection of the glands or other disease

was perceivable to the touch. He continued to enjoy his meals of broth, &c. and to digest his light diet. During the attacks of pain, the respiration would be hurried.

The fifteenth of February was the date of his last admission into the regimental hospital; when, the heart and arteries acting strongly, a pint of blood was taken from the left arm, which exhibited a buffy coat, and cupping. Leeches were applied to the side next day: constant attention was necessary to the state of the bowels, and medicine easily produced liquid stools. He was relieved, though the beating of the heart and arteries continued, and so as to be perceptible in the neck, the groins, and the limbs, and felt also in the loins. The pulse never much exceeded from ninety to one hundred, and gave an impression to the finger and to the hand, (when placed over the region of the heart,) similar to that which occurs in *pericarditis*, but without pain beyond the lower part of the abdomen and loins. The constitutional debility increased, and the pulsation continued, with periodical attacks of pain, which were always removed by leeches, laxatives, and moderate anodynes, with occasional blisters to the seat of pain. From the day of his admission sixty minims of the *liquor antimonii tartaratis* were given every four hours in saline mixture; and rhubarb and carbonate of soda twice a day. Subsequently the antimonial wine was reduced to forty minims, and fifteen of the tincture of digitalis added, as some nausea was excited by the antimony. The pulsation continued with diminished force and quickness latterly, but the application of leeches was repeated occasionally whenever pain came on, and with laxatives and anodynes afforded great relief.

On the twenty-first of March his pulse had fallen to forty-eight, though the pulsation was still strong and visible as before, and he breathed in rather a hurried and laborious manner. The night before he had experienced a violent paroxysm of pain, with increased frequency in the pulse, and in the morning was much relieved from anodynes, &c. as usual. The digitalis and antimony were omitted. He appeared to be much debilitated, though free from pain, except a little soreness, which was soon removed by small doses of æther and laudanum in camphor julep, with extract of hyoscyamus, and the pulse rose to seventy-four, and lost its former character in a great degree, giving an impression of more natural action. On the morning of the twenty-second, at seven o'clock, he had another attack of pain and quickness of the pulse, from which the usual remedies recovered him, and about half past eight he expired, without any urgent symptom, collected in mind, and perfectly quiet and sensible.

*Sectio Cadaveris.*—Upon examining the body about twenty-four hours after death, the following appearances presented themselves. In the thoracic and abdominal cavities, there were found about three or four pints of bloody serum. The pericardium contained a small quantity of pure serum; the lungs appeared

perfectly healthy; the heart was larger, and its parts were more strongly developed than usual. The thoracic membranes exhibited no signs of inflammation. The abdominal viscera showed no appearance of diseased structure or inflammation; but the cavity of the abdomen, on the left side, was occupied, from the diaphragm to the bottom of the pelvis, with a mass incumbent on the spine, and forcing the liver, stomach, &c. upwards; which mass seemed, at first sight, to be composed of coagulum, covered with a shining white membrane.

On removing the viscera, in order to get at the attachments of this mass, it was found to adhere strongly to the diaphragm, the whole extent of the left side, the great arch of the stomach, the pancreas, part of the colon, the cæcum, and the rectum. When removed, it measured nearly twenty inches in length, and was narrower in the centre than at either end. The left kidney was enclosed in its upper portion, and partly adhered to its membranous coat. The kidney itself was perfectly healthy in structure, but somewhat large. The cavity of the pelvis was so much occupied by this mass, that the rectum was closely pressed upon, and the bladder contracted to a very small oblong form; in its internal structure, as well as that of the intestinal canal, appearing free from inflammation, and healthy. Three of the dorsal vertebræ, against which the mass rested, were rough and irregular on their surfaces towards the left side, as if absorption of bony matter had taken place. Where the bifurcation of the aorta takes place, there was thickening of its coats for the space of about two or three inches, so as to contract its bore, and condensation of cellular substance appeared externally. Upon examining the main blood-vessels throughout, no other diseased appearance was observable; and their internal coats were free from redness.

When the shining white coat of the mass was divided, its contents every where displayed a continuity of dark coloured coagulum, which, on being washed, presented the usual evidence of fibrinous matter.

From the *Lancet*.

**A CASE OF HEMORRHAGE, IN WHICH THE PLACENTA WAS ATTACHED OVER THE OS UTERI. RUPTURE OF THE UTERUS BY ITS OWN ACTION.** By JOHN GREENING, M. D.

Mrs. ———, æt. 40, residing at White Lady Aston, near Worcester, requested my immediate attendance, June 4th, 1827. On my arrival, she informed me, she had had flooding come on whilst she was taking her breakfast, to a degree which had caused great consternation to herself and family, but that it had nearly ceased since she had been in bed. She was five months gone with child; had not been quite so well lately, having been obliged to part with her servant, which caused her to

exert herself more than common. Complains of headach; pulse full and strong; had been troubled with palpitation of the heart; bowels regular; no labour-pains, but a slight discharge continues. Ordered to be kept quiet, and repose of body in a horizontal position; light covering; cool air admitted into the room; cloths, dipped in vinegar water, to be kept constantly applied to the abdomen; to take a little cold gruel or lemonade. Mittr. sanguis ad ℥xii.; R. Infus. rosæ, ℥vi.; magnes. sulphat. ℥ss. Mx. capt. coch. iij. 4tis horis.

5. Better; discharge less. R. Habeat olei ricini, ℥ss. tertia, qq., hora repetend donec plene responderit alvus.

To keep her bed, and pay attention to her diet; to avoid over exertion; and if the palpitation returns, to lose blood. This was her tenth pregnancy; in the last, Mr. Carden, surgeon of this city, attended her of a seven months child; in the previous labour I was sent for; that was a preternatural case, the scapula presenting, and a portion of the funis; she was not quite gone her full time; she had a good recovery. When last at market, a person accidentally pushed her down; she did not find herself hurt by the fall, and returned home very well.

7. Repet. med. Ol. ricini.

10. Much better, but very low and weak; no discharge; complains of slight pains in the bowels. R. P. rhei, ℥ij.; magnesia, ℥i.; conf. opii, ℥i.; aquæ menthæ, ℥vi.; st. lavendulæ comp. ℥ij. Ft. mist. cujus capiat coch. mag. iij. ter in die.

July 7. I was again sent for about ten o'clock P. M.; she had been exerting herself, and a return of the hemorrhage came on about seven o'clock; pulse tranquil; bowels open; free from labour pains. Upon examination, there was a quantity of coagulum in the vagina, which was removed, but others soon formed; the os uteri open to the size of a half-penny; a portion of the placenta could be felt on the anterior part of the cervix uteri; the coagulum which had now formed I left in the vagina, and remained with her till two A. M.; vinegar cloths were kept constantly applied, and the discharge stopped. R. Infus. rosæ acid. ℥ij.; magnes. sulph. ℥iv. Ft. mistura cujus capiat partem tertiam quarta quaque hora.

8. Nine o'clock A. M., Mr. ——— called at my house, and informed me a little coagulum was thrown off about every quarter of an hour, but very trifling in quantity; refreshed by sleep; to be kept quiet. Admitt. liberime aer gelid.

9. Better.

14. Had no discharge since Monday. Habeat ol. ricini, ℥iv. h. s.

21. Has had a trifling return of the discharge; complains of palpitation. V.S. ℥vi.

28. Discharge increased; repeat the vinegar cloths, and to be kept quiet.

29. I saw her about nine o'clock, P. M.; a large coagulum had come away; there was a great change since I last saw her; strength reduced; pulse feeble and small; ninety-five

in a minute; face pallid; she said the discharge had not entirely stopped; all this week she had kept her bed, and used the cold application. Upon examination, I found a coagulum lying in the vagina, the os uteri dilated to the size of a dollar, the placenta attached over it. I stated her danger, and requested the nurse to be sent for, that I might proceed to delivery; it alarmed her much, and she would not accede to it, for she never heard of a woman being delivered without labour pains. I told her such cases did occur, and advised her to consent, if her life was to be saved. A friend of her's had called upon Mr. Carden, a week ago, for his opinion; he desired that she should keep her bed, and use the same means which I had adopted; it was her particular wish I should see him, before any thing was done; and he agreed to go with me at six o'clock. On our arrival, we found her much the same as when I left; the discharge still continuing, Mr. Carden examined her, and said it was a very clear case, that the placenta was actually protruding, and her life was not safe a minute; a large gush of blood might ensue, and death be the result, before any assistance could be given her. Cautiously introducing my hand through the placenta and membranes, I brought down the feet; when the child was about half born, she complained of most excruciating pains in her back. We now thought the uterus was beginning to act, and he advised me to wait a little; the pulse was at this time rather fuller; she took a little brandy and water occasionally; and after a short lapse of time the child was born, but dead; the funis encircled round its neck. We now applied our hands to the abdomen, and being of opinion, that the uterus was very well contracted, we removed the placenta, which was lying loose in the vagina; there was no hemorrhage; her pulse became fuller, and she felt, as she said, comfortable. However, before I had finished washing my hands, she complained of being faint; her countenance became pale; pulse feeble; a discharge of blood and wind took place; the abdomen was greatly distended; and when I applied my hand to it, a great expulsion of blood and wind again took place; the uterus contracted under the hand; the faintness continued; brandy was administered. Mr. Carden came up stairs, and was much surprised to find such a change, and advised me to introduce my hand; the discharge was not very profuse; there were no coagula; the uterus did not contract, although I irritated it with my fingers, and cautiously introducing my hand still farther, I found my fingers among the intestines. Never having met with a case of ruptured uterus before, I was at a loss to pass any correct judgment upon the case, and requested Mr. Carden to examine; he soon told me it was an extensive rupture, and the intestines were coming down, which he pushed up; death soon put an end to her sufferings; the uterus was so thin, it was like a piece of paper; an examination of the body could not be obtained.

From the Medico-Chirurgical Review.

HEPATIC DISEASE. Drs. GRAVES and STOKES.

[Meath Hospital, Dublin.]

In the following case, related by Drs. Graves and Stokes, from the Meath Hospital, some observations will be found, corroborating certain opinions, which we gave in our review of Dr. Bright's work, in the second Fasciculus of this number, page 319.

*Case.*—J. McClusky, aged 11 years, of scrofulous habit, labours under considerable swelling of the abdomen, with fluctuation and tympanitis. "The right hypochondrium appears rather full; upon examination, the liver can be felt extending across the left hypochondrium, and as far down as the umbilicus, presenting a defined edge." He had œdema of the left leg—no cough or dyspœnia—good appetite—much thirst—urine copious, light-coloured—and depositing albumen in abundance when heated. Bowels are regular, the pulse 125, tongue clean and moist, sleeps well. The complaint was of twelve months' standing. One grain of calomel was ordered to be taken every night, and some mercurial frictions to the right hypochondrium. On the eighth day of this treatment, the mouth was affected, and the calomel was omitted. Having caught cold, a smart ophthalmia occurred, which required leeching, &c. Afterwards, as the swelling of the abdomen continued, the calomel was renewed, in two grain doses, and half a drachm of the spir. æth. nitr. was given twice a day. The belly became reduced in size—the urine more copious, but still coagulable by heat. In a short time afterwards, he was discharged, the abdomen being nearly reduced to its natural size, and the appetite good. The following extract from the observations appended to this case, we deem worthy of insertion.

"It is not easy to determine the nature of the hepatic tumour which was so very considerable in this boy. It was slow in its increase, and not attended with well marked symptoms of chronic inflammation of the liver. There was no tenderness on pressure, nor any pain or uneasiness in the right hypochondrium. There was no evident derangement of the biliary secretion, and his appetite, sleep, strength, and nutrition, were scarcely impaired. On the other hand, the œdema of the left leg, the commencing ascitis and tympanitis, the albuminous urine, accelerated pulse and increased thirst, all united to prove that the constitution had begun to suffer in consequence of the diseased state of the liver. The alterative doses of mercury, which were cautiously exhibited, were evidently productive of much benefit. The mixture with spirit of nitrous æther seemed useful in relieving the tympanitis. *In this, and many other cases, where considerable organic alteration had taken place in the liver, we have observed an apparently healthy secretion of the bile. Thus we have found bile of an healthy colour and consistence in the gall bladder, when the substance of the liver was tuberculated throughout. Naturally coloured al-*

*vine discharges, therefore, furnish no proof that extensive organic disease of the liver does not exist."*

"Concerning the albuminous state of the urine, we may remark, that it is no proof of an inflammatory condition of the constitution, it merely indicates considerable disorder of the function of assimilation. In health, a certain portion of animal matter is contained in the urine, in the form of that highly animalized substance, urea. This may be increased so much in quantity above the healthy standard as to constitute a disease. When the assimilative powers are more deranged, the animal matter of the urine ceases to assume the more highly animalized form of the urea, and is voided in the form of albumen, which contains much less nitrogen than urea. In a state of the system still further depraved, it passes off in the form of sugar, which contains no nitrogen, and is the least highly animalized. In diabetes, it is probable, that the urea is voided in increased quantity at first; as the disease proceeds the animal matter is voided in the shape of albumen, and afterwards of sugar. When diabetic patients are getting better, then the contrary seems to take place; and when the sugar diminishes the albumen increases or reappears, and afterwards is replaced by the more healthy secretion of urea. Dr. Prout was the first to establish the existence of these three different species, or rather stages of diabetes. In dropsy the appearance of albumen in the urine is a bad sign, as indicating a depraved assimilation and a source of debility. We have established, by numerous experiments, that when there is much albumen, there is scarcely any urea in the urine, and *vice versa*, or more generally that the proportion of urea is inversely as that of the albumen. How far the treatment suited to diabetes may be also applicable to cases of chronic dropsy with albuminous urine, and unattended by organic disease, experience alone can decide."

The passage which we have marked in italics, is that to which we wish to draw the attention of Dr. Bright, and the public. But the whole of the observations of our intelligent authors are deserving of notice.

From the Medico-Chirurgical Review.

#### **PATHOLOGY OF FEVER. DRS. GRAVES and STOKES.**

We believe that none, even of our continental brethren, have taken greater pains to investigate the causes, the seat and the consequences of fever, than the two physicians above named. Their sentiments, therefore, on the pathology of this wide spread disease must be interesting to the profession at large. These sentiments may be gathered from some observations appended to a case of fever (among many others) treated in the Meath Hospital.

"The epigastric tenderness, with nausea and vomiting, so common in this fever, seemed to be caused by inflammation of the mucous membrane of the stomach. In most of the fatal cases this was found of a dark red colour and

very soft, a condition evidently produced by violent inflammation. In others the redness was not so extensive, dark, or continuous; although we acknowledge, that in the present fever the above symptoms, depending on inflammation of the stomach, are very frequent, yet we have seen some cases where there was no evidence of any local inflammation whatsoever; and in others again, have observed that some other organ, as the *brain* or *lungs*, was the seat of inflammation, while the stomach was free. We cannot subscribe therefore to the opinion, which supposes a local inflammation to be the root of all fevers, or to that which attributes their origin solely to inflammation of the mucous membrane of the stomach and alimentary canal. In our dissections we have in some cases found the brain inflamed, in more cases the lungs, and still more some part of the digestive organs. We do not recollect to have found in one instance, out of very numerous dissections, a fatal case of fever which did not exhibit some serious local lesion of an inflammatory nature; so that while we deny, from our observation of cases during life, that fever necessarily implies local inflammation, dissection has convinced us that the occurrence of local inflammation during fever, is the general cause of its fatal termination. Let us here observe that the latter inference is by no means contradictory of the former; for in the fatal cases, accurate observation always detected, *during life*, the seat of the inflammation; so that in those cases which terminated favourably, and where no such symptoms existed, our inference that no local inflammation had been present, receives additional strength from our post mortem examinations, for there we always found inflammation where we expected to find it; that is to say, we were always able to pronounce on its situation, so far as to tell, before the body was opened, *in which cavity the inflammation would be found*. The post mortem examinations have been always conducted by ourselves with the greatest care, and we generally spend between two and three hours in the examination of the body, being convinced that nothing has contributed to retard the advancement of medicine so much as superficial post mortem examinations. In examining the abdomen we first note the general appearances of the intestines, and then take out the whole intestinal tube, which we slit up with an *enterotome* at its mesenteric attachment; this is done in order to avoid dividing any of the follicular patches or ulcerations in the small intestines, which are always situated at some distance from the mesenteric attachment. During this process we examine the contents of the alimentary canal; and afterwards having first carefully washed the mucous membrane, we remark its appearance throughout its whole extent from the stomach to the rectum. The morbid anatomy of the brain, the lungs, and the intestinal canal, has, within these very few years, received so many important additions, so much light has been thrown on this subject by more accurate investigations, that we would hesitate much in

drawing any conclusions from the dissections of fever subjects recorded before this period. Indeed, we could prove that in most of even modern works on the pathology of fever, morbid appearances have been frequently mistaken, and more especially that many things, both in the brain, lungs, and alimentary canal, have been set down as morbid, which really are not so; consequently, conclusions, not at all justified by the state of the parts, have been drawn. Thus we hear of sanguineous congestion in the head, and morbid vascularity of the brain, intestines, &c. where the very accounts given contain internal evidence, that these supposed morbid appearances had either no claim to that appellation, or resulted from changes which took place immediately before or after death. In fact, we look upon the morbid anatomy of fever as a subject which requires to be investigated almost *de novo*. Our assertion, that we have hitherto found evident lesions of vital organs in all the fever subjects we have dissected, is, we are aware, opposed to the recorded experience of many authors, who relate numerous cases in which no morbid alteration of any consequence could be detected. We question, however, very much the accuracy of such dissections, for, as has been well observed by Rostan, nothing is easier than to find nothing. We doubt whether such persons have injured medical science more than those who have found too much."

We recommend the above careful mode of *post mortem* researches to the attention of our brethren in this country—especially those who have the superintendence of public institutions, and who, consequently, have less difficulty to contend with, than those in private practice. We believe that very few cases of fever go on to a fatal termination, without the occurrence of organic changes—at least of inflammation in some of the great viscera. These may be the cause of death, but not of the fever.

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From the London Medical Gazette.

**CASE OF IMPERFORATE ANUS, WITH OPERATION.** By A. COPLAND HUTCHISON, F.R.S.E., Surgeon to the Westminster General Dispensary, &c.

*Mr. Editor,*

Dr. Locock did me the honour to consult me, on the 20th of November last, in the case of a female infant, named Mary Scanler, born with imperforate anus; this being the sixth instance of the kind that has happened in my practice. The usual mark, or hollow, in the situation of the natural anus was most distinct. Upon making pressure over the abdomen with my hand, I could not discover any particular fulness or tension of the parts; and, therefore, profiting by past experience, I proposed to delay the operation until the following day, by which we would ensure a greater distention of the rectum with meconium, when the operation should be performed. The

infant had vomited ever since its birth, and would not take the breast, so that it was necessarily fed with spoon meat.

On the 21st November, just sixty hours from its birth, I performed the operation, after the usual manner,\* at the Westminster General Dispensary, in the presence of Messrs. Jennett, Davis, and Wade, besides some pupils; when, at the distance of an inch and a half from the surface, we had the satisfaction of penetrating the intestine with a trocar, through the canula of which a quantity of meconium instantly flowed. The canula was secured in its situation for the night, and the child was carried home.

The vomiting, which for twenty-four hours previous to the operation had become sterco-raceous, now gradually ceased after the operation; the infant fed better, and continued for between three weeks and a month to thrive and grow; its motions were natural in colour, consistence, and quantity, during the whole of this period, and a tea spoonful of castor oil was administered only twice, more as a precautionary measure than as a matter of necessity.

From about the 14th of December, owing to some cause which we could not some time clearly understand, the child gradually lost flesh. Its motions, though natural, were scanty, and the artificial anus was easily permeable to a good sized bougie. In a few days we ascertained that the mother had placed the child out to be nursed by a person, who, it would seem, professed at the time to be suckling her own child, but which, a few days only before the child's death, was discovered not to be the case. The child died on the 28th of December, without evincing any symptoms of acute disease; and leaving an impression on my mind, that there had been gross neglect on the part of the nurse.

The next day I examined the cavities of the chest, abdomen, and pelvis, in the presence of the Messrs. Wade, when not any cause of death, or appearance of disease, could be traced. The parts concerned in the operation, with the whole contents of the pelvis, were carefully removed, and are now in the Museum of the College of Surgeons.

The whole of the viscera in this case were in their natural situation; the rectum turning up behind the uterus, and ending in a narrow cul de sac; the part cut into being at the bend where it approached nearest to the surface.

After the preparations had been macerated, and in spirits and water some days, the part cut through at the operation only measured one inch; half an inch, however, we must allow for absorption, and the natural approximation of the mucous membrane of the intestine to the surface, after a lapse of upwards of five weeks. The parts seemed healed, and in so far as the operation itself was concerned, it

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\* See *Practical Observations in Surgery, &c.*; second edition, by A. Copland Hutchison.

had completely succeeded. The sphincter muscle appeared to be wanting.

In this case, as well as in my former, the excitement of titillation, amounting sometimes to rather a rough movement of the finger in the natural situation of the anus, both before the operation was commenced and afterwards, was our best guide in making the various incisions with the scalpel. Without this expedient, and the introduction of a director into the vagina, to guard us against wounding the parts in that direction, I am certain that this operation would not have been so successfully performed. I can now, therefore, with more confidence than ever, speak of the advantages to be derived from attention being paid to these suggestions. In this last case, indeed, I was chiefly directed in cutting as the bowel protruded downwards, and only used the knife as the protrusions occurred; so that, after the first incision, the best stimulus or excitement to protrusion, is the scalpel, followed by the surgeon's finger thrust into the bottom of the wound, and moved upwards and sideways as before directed. The operator, however, must be very much on the alert in watching the various protrusions or bearings down of the gut, for its retirement from the surface is much more rapid than its approach or protrusion downwards.

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From the Medico-Chirurgical Review.

#### OPHTHALMIA PORRIGINOSA.

Under this term, Mr. Christian, consulting surgeon to the Liverpool Ophthalmic Infirmary, has treated of an affection which is usually denominated *scrofulous ophthalmia*. Mr. C. thinks himself authorized to give the disease this title, not only from its appearing in connexion with porrigo, but from its bearing certain features, sufficiently characteristic in themselves, to warrant such a distinction. The coincidence has been regarded as casual, and the ophthalmic disease treated on general principles. Not unfrequently, when the symptoms have run high, the porriginous eruption has been encouraged, with the hope of relieving the local affection. As Mr. Christian looks upon these two in the relation of cause and effect, he conceives that treatment of derivation, according to that principle, must be erroneous, if not injurious. The following description of the disease is worthy of record in this place.

"Porriginous ophthalmia is a disease of early life, affecting principally children, though sometimes seen in the adult subject. It is usually accompanied by an eruption of pustules on the face or head which go through the various stages of suppuration, ulceration, and desquamation: and if the eruption in its pustular form shall have disappeared before the inflammation of the eyes have commenced, still there will, almost always, be found some traces of the original disease, in the form either of scabs or fissures, situated behind the ears,

at the commisures of the palpebræ, or at the junction of the alæ nasi with the cheeks. It is worthy of remark, that when the fissure or chap is situated between the lips, attended with excoriation of the nostrils, the upper lip often swells, assuming the appearance of what is vulgarly termed the scrofulous lip, which may be one cause of the disease being referred to this origin.

"Sooner or later, however, the ophthalmia commences, and the eye in a short time presents a highly vascular state of the conjunctival membrane. The inflammation appears in different degrees of intensity, in different parts of the albuginea; and the vessels, which are much enlarged, are seen to run in clusters, towards certain parts of the cornea, whilst this transparent tunic contiguous to these vessels is more or less clouded. On minute inspection, a pustule or vesicle will frequently be discovered at the apex of each of these fasciculi, or bundles of red vessels; but, very often, depressions will be found to exist, instead of raised pustules, situated at the margin of the cornea, or on the intermediate surface. On some occasions, the cornea will be perfectly transparent, and free from either pustule or ulcer, whilst the albuginea, with its vessels fully distended with blood, will present one or more yellowish spots, apparently elevated above the surrounding vascular superficies. These are so many ulcers, which by their extreme irritability keep up, if not give rise to, the inflammatory excitement in these parts. Similar ulcers are sometimes found on the lining membrane of the palpebræ. Wherever they are situated, they render the motions of the eyelids very painful; so that the eyes are generally kept fast closed, and their inspection, in consequence, becomes a matter of extreme difficulty. The sight being affected, only in proportion to the degree of opacity, and as this is but inconsiderable in some instances, the vision remains perfect; but the sight will necessarily be more or less impaired, according to the extent and density of the opacity of the cornea. Very often black spots may be observed arising from the attenuation of the cornea, occasioned by the ulcerative process, which sometimes perforates this tunic, and causes the incarceration of a portion of the iris. The discharge which issues from the eye, consists principally of tears, mixed more or less with a sanious fluid which discolours the linen applied to the parts, and is often considerable in quantity. Although the pain attendant upon this affection of the eyes is not very great, yet from the great irritability of these parts, the patient not only carefully shuns the light, but desires to lie with the face downwards, whilst the hands are almost unceasingly applied to the forehead."

The disease often assumes a chronic form, and lasts for months, leaving one or more opaque spots on the cornea, the consequence of effused lymph. The conjunctiva palpebralis also becomes changed in structure, and a morbid sensibility is generated, which is

very difficult of removal. In such cases the disease will continue long after its cause is removed, and then requires a specific treatment. The author next goes on to prove, or at least to support his doctrine, that the affection of the eye is an association with, or consequence of, the cutaneous eruption. For the arguments and observations on this head we must refer to the original paper. His principle being admitted, (and we do not see any just cause for denying it,) the indications of cure will hinge on the removal of the eruption, and the extinction of the ulcerative process.

"Whatever external appearance of active inflammation may exist, provided it be of the specific character, usually attendant on porrigo, and occurring in conjunction with pustules or ulcers of the globe, bleeding either locally or generally is seldom necessary; except, indeed, the inflammation shall have extended to the internal tunics of the eye characterized by pyrexia, severe pain of the eye, forehead, &c. Blistering, likewise, which constitutes so valuable a remedy where a derivative is required in these cases, is not only useless, but generally tends to aggravate the eruptive disease, and thereby proves an additional source of irritation. The cooling sedative lotions in such general use are for the most part unavailing here: the object kept in view in the treatment of this particular affection, being not so much the alleviation of pain and irritation, as the production of a new action, in parts already under the influence of a specific disease.

"Corresponding with these views, it has been found by experience, conducted upon rather an extensive scale, in a public institution, where a large proportion of the cases are of this class, that the mercurial applications constitute the best remedies. A weak solution of the oxymuriate of mercury, composed of one quarter of a grain to the ounce of water, forms a very useful application: or if there be much discharge from the eyelids, and especially if accompanied by excoriation of the parts around the eye, the mixture of calomel and lime water, known by the name of black wash, will be found to be one of the best local remedies. The unguentum hydrargyri nitratis mitius, or the unguentum hydrargyri præcipitati albi, affords an excellent dressing for the eruption, or ulcers about the face and ears, which require to be attended to; a small portion of the former, or the red precipitate ointment diluted, is to be introduced within the palpebræ at bed time.

"Previously, however, to the use of these latter means, a weak solution of the argentum nitratum, in the proportion of two grains to the ounce of distilled water, should be dropt on the surface of the globe, and this ought to be repeated every second day, as long as the ulcers continued.\* It has already been re-

marked, that the morbid action has been kept up, in the fine textures of the eye by the presence of an ulcer, or fissure at the corner of the eyelids, which will be frequently observed to bleed, whenever the palpebræ are forcibly separated. These fissures must be touched, every second day, with a saturated solution of the nitrate of silver; and during the intermediate days, with the weak ointment of nitrate of mercury.

"The vascularity together with the irritability will generally disappear with the healing of the ulcers, a strong proof that the specific action of these vessels is overcome; but should a degree of morbid sensibility still remain, a collyrium consisting of four grains of the sulphate of zinc, to an equal number of ounces of water, combined with a drachm of the vinum opii, dropped into the eye, once or twice a day, will be found highly useful."

On the constitutional treatment, Dr. Willan's work will be consulted with advantage, and also the lectures of Mr. Lawrence, as published in the 10th volume of the *Lancet*.

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From the *Lancet*.

#### LARGE POLYPUS, GROWING FROM THE WHOLE CIRCUMFERENCE OF THE OS UTERI.

[St. Bartholomew's Hospital.]

Sarah Lloyd, aged 41, a stout healthy looking woman, was admitted into the Hospital, March 12, 1828, under the care of Mr. Lawrence. Six months ago, she was delivered by the forceps of a still born, full sized child. During delivery, the accoucheur, who is an intelligent practitioner in the country, observed for the first time a polypus in the vagina, so large that he at first mistook it for the head of the child.

The tumour has since gradually increased, being attended with a sensation of weight and bearing down, and with mucous discharge, sometimes mixed with blood. There has not been much pain, and the constitution has been but little affected. The catamenial discharge has ceased.

At the time of admission, a tumour nearly as large as a child's head, protruded from the vagina, into which it could be completely returned by moderate pressure. It was a firm and nearly insensible mass, of a somewhat pyriform shape, with a surface for the most part smooth, but at some points a little knotted, deep red, and plentifully bedewed with mucus. On passing the finger up the vagina, the neck of the tumour was felt as large as the wrist; its texture was firm, and the patient complain-

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argentum nitrat. in the proportion of two grains to the ounce of distilled water, (dropt into the eye twice a day,) a safe, not painful, and very powerful remedy in inflammation of the conjunctiva."—*Extract from a letter to the Editor.*

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\* We are informed by Dr. Maule, of Marlborough, that "he has found a solution of the

ed of pain when it was pressed. In the recumbent position, the tumour remained in the vagina; but it protruded externally, and occasioned increased pain when the patient rose and walked about. There was copious discharge of thin mucus from the vagina; but no hemorrhage had ever taken place. The faces and urine were voided with difficulty; the general health was not impaired. Mr. Lawrence requested Dr. Gooch to see the patient, and after the most careful examination, neither of these gentlemen could feel any portion of the *os tincæ*. They both concurred in opinion as to the propriety of removing the tumour by ligature.

The patient was ordered to confine herself to the horizontal posture, and the bowels were kept open by the occasional use of saline aperients. On the 25th of March, a strong silk ligature was passed around the root of the tumour, by means of a double canula, and drawn tightly; it caused a little pain, which increased towards the evening, when a dose of laudanum was given with relief. The ligature was drawn tight every day; it broke on the 28th, and was immediately replaced by a stronger one. The surface of the tumour assumed a dark livid, and then a black sloughy appearance, and produced a copious foetid discharge. There was severe pain shooting up the loins, heat of skin, thirst, restlessness, furred tongue, and nausea. The hypogastric region and abdomen were free from pain and tenderness; the bowels, which were costive, were freely moved by the use of mild clysters, and the pain was much alleviated by small and repeated doses of Dover's powder. A lotion of the chloruret of soda applied to the tumour completely corrected the fœtor. On the seventh day after the application of the ligature, violent hemorrhage suddenly came on from the vagina, and a very large quantity of blood was lost, proving nearly fatal. Cold cloths were applied externally, and a solution of alum was thrown up the vagina. Brandy, wine, and ammonia, were freely administered, and the heat of body was restored. She continued in an extremely feeble state for about 48 hours; she then recovered considerably. The pain and sense of bearing down, which she likened to labour pains, continued; she was occasionally sick, and the bowels were moved with difficulty; there was, however, no tension, pain, or tenderness of the abdomen. The tumour grew more livid, and its surface presented an ash-coloured gangrenous aspect, its bulk was much reduced, and there was a considerable discharge from it.

On the third of April, Mr. Lawrence thought it advisable to remove a portion of the tumour, but as he feared it was not completely deprived of its vitality, he first made a slight incision at its lower part; a quantity of dark coloured blood immediately flowed out, and confirmed his suspicions. Alum lotion was applied, and the bleeding soon stopped. The poor woman grew more feeble, and sank at last on the 8th of April, fifteen days after the application of the ligature.

#### *Examination twenty hours after death.*

On laying open the cavity of the abdomen, not the least evidence of peritoneal inflammation could be discovered. The symphysis pubis was divided, and the uterus and its appendages taken out. The uterus was of its natural size; its neck was elongated, and formed a continuous surface with the root of the tumour. Viewed externally no portion of the *os tincæ* could be discovered. The ulceration which had been produced by the ligature had not extended very deep through the dense substance composing the neck of the tumour. Through the peritoneum, which is reflected from the posterior surface of the uterus to the rectum, was a ragged opening, about an inch in length, and opposite to this, and in the back part of the uterus, was a small superficial ulceration. The surrounding peritoneum, and that lining the cavity of the pelvis, were healthy. The cavity of the uterus was found to communicate externally by a small opening at the inferior part of the tumour, and on making a perpendicular incision through the centre of the uterus and tumour, the root of the latter was found attached to the whole circumference of the *os uteri*, and completely incorporated with the cervix. The tumour consisted of a hard compact substance, intersected by membranous septa, presenting a whitish coloured aspect.

From the Edinburgh Medical and Surgical Journal.

**OBSERVATIONS ON THE MORTALITY AND PHYSICAL MANAGEMENT OF CHILDREN.** By JOHN ROBERTSON, Member of the Royal College of Surgeons, Edinburgh; one of the Surgeons to the Manchester Lying-in Infirmary, &c. 1827. 12mo.

The pursuit of medical statistics is in this country unhappily attended with many difficulties, and is too apt to lead to unsatisfactory or uncertain results. Our public mortality bills, which must form the basis of all general researches, and to which all special investigations must bear a reference, are, if not so inaccurate as generally represented, at all events miserably defective. For want of a few additional particulars, which it would have been easy to collect along with the rest, the attention of the statistical inquirer must be confined in a great measure to the general facts of the science, and it is exceedingly difficult, if not impossible, for him to pursue it into its remoter corners, and to make those more special researches, by which chiefly we are to discover where the public health is deficient, and how it may be improved.

It certainly reflects disgrace on Great Britain, and no little discredit on our Houses of Parliament, that matters are in this respect allowed to remain so long in the same state. Every succeeding year teaches us to lament that our ancestors were ignorant of the great objects to be attained by extending the mor-

tality bills. Yet with our eyes open we neglect the lesson, and seem disposed to revenge the loss we have sustained from our predecessors, by transmitting it to our successors unrepaid. The faint attempt at improvement in the statute of 1812 will hardly relieve the Legislature from the charge of indifference. We fear it deserves too truly the reproaches heaped upon it by Mr. Robertson. "The deviser of that act (says he) deserves little praise for his exertions. His plan is meagre and defective in the extreme. If the mode of parochial registration which had been so long followed required revisal, it should have been entered upon with full intelligence as to the nature of the task, and a sincere desire as far as possible to remedy every defect; qualifications of which no trace appears in the act in question." It is to be hoped, that amidst the extensive inquiries which are now making into various departments of the internal policy of the kingdom, this most important topic will not remain much longer unnoticed.

Meanwhile, by the activity of private individuals, and the public spirit of a few municipalities, the defects in the general system have been supplied for some local districts; and comprehensive tables have been kept, the examination of which has led to the discovery of some striking facts. These facts, however, are often lessened in value for want of comparative statements, and still more frequently for want of an analysis of their causes;—an inquiry, into which it is impossible to enter without tables of still greater comprehensiveness than any now existing.

Among the discoveries which have thus been brought to light, there is none more pleasing to the physician and the philanthropist than the almost inconceivable amelioration which the progress of physic and political improvement together have effected in the mortality of children. And we apprehend there are few discoveries in medical statistics which, when deeply examined, will lead to so many useful practical hints for directing us to farther advancement. This subject has engaged the attention of several writers, and in recent times particularly of Dr. Watt, Sir G. Blane, Mr. Mylne, and Dr. Casper. In Mr. Robertson's work it is handled in a very clear and satisfactory manner. His object is to arrive at an accurate numerical statement of the general fact, to trace it as far as possible to its causes, to correct certain errors committed by others in the latter investigation, to show how much our efforts at future improvement should have reference to the physical education of children, and to lay down the rules by which their physical education should be conducted. It consists, therefore, of two parts; in the former or statistical part, he treats of the mortality of children and its causes; in the latter, of their physical education as regards their health. In the former he shows that he possesses no small share of the talent of the statistical philosopher; in the latter his instructions are judicious, popular, and with a few trifling exceptions correct. We shall give a

short analysis of his statistical inquiries. The practical part of his treatise is too concise to admit of abridgment.

After a few remarks on the nature of mortality bills, and the defects of those kept in Britain, he gives a number of local tables from various parts of the country, and proceeds to determine the relative mortality among children in different places. From this inquiry it results that the mortality is much greater in cities than in small towns, and still greater than in the country. The tables, however, by which he endeavours to establish the numerical relation between them, are not exactly adequate to the end in view. He makes it appear that from the tables of London, Liverpool, Glasgow, and Manchester, the deaths under ten years of age are, on an average of the whole cities, 51.39 per cent. of the total mortality;—that in small towns, such as Chester, Carlisle, Warrington, and Northampton, whose population varies from eight to twenty thousand, the average per centage is 46.97;—that in village parishes it is 45.9;—and that in agricultural parishes it is so low as 35.4. Two objections lie against these results. In the first place, the tables from which the data are taken were constructed at very different periods, some only a few years since; others so long ago as the middle of last century; between which periods it will presently be seen that a vast change has taken place in infantile mortality. And secondly, as the total mortality is much greater in some places than in others, it follows that the mode of counting by the ratio of the infantile to the total deaths, does not convey an accurate idea of the number of deaths during infancy, in proportion to the population. This objection has been foreseen by the author; and the following example he has given of a result corrected on account of it, is an excellent illustration of the magnitude of the errors which the ordinary way of computing relative mortalities may introduce. For thirty years after 1782, in every hundred deaths at Glasgow, 54.75 were under ten. For twenty years after 1746, in every hundred deaths in the parish of Ackworth, an agricultural parish in Yorkshire, 27.33 were under ten. Whence it would be natural at first sight to infer, that the mortality among children was twice as great in Glasgow as in the country districts of Yorkshire. But in reality this is not a correct account of the ratio; for the general mortality was much greater in Glasgow than in Yorkshire. The numerical facts we shall state in a manner somewhat different from our author. In Glasgow the deaths annually were one in every forty inhabitants, in Ackworth one in fifty-six. Hence in Glasgow, among every thousand inhabitants, there died annually 25, of which 54.75 per cent. or 13.7, were under ten; while in Ackworth, among every thousand inhabitants, there died annually 17.8, of whom 27.33 per cent. or 4.8 were under ten. *Consequently for every child that died in the latter place, in a given number of inhabitants, three died in Glasgow.* This is an appalling fact. Probably, indeed, the

places chosen afford extreme results. But unhappily we are not in possession of data for comparative observations.

The next subject to which Mr. Robertson turns his attention is the causes of the great comparative mortality of children in towns over that in the country.

The causes he enumerates are those very obvious ones,—the foul air of crowded houses and apartments,—the consequent facility with which infectious diseases propagate themselves—poverty, and particularly in manufacturing towns, sudden alternations of ease and want—increased number of illegitimate births, the tendency of which to augment the mortality among children was very well shown in our account of the work of Dr. Casper. Another cause of which Mr. Robertson treats more specifically is the custom of the lower classes in towns of giving the youngest children pure spirits, an abominable practice, which in Manchester, it seems, the reluctant babes are gradually taught by making them in the first instance suck the finger dipped in the liquor. Another and a very important cause in the case of Manchester is the total want of vacant spaces throughout the town for public walks. It is to this cause that the author is disposed to assign the greater mortality among children in Manchester than in other towns of equal size, in which the general mortality is the same.

The next section treats of the improvement in infantile mortality which has taken place within the last forty or fifty years.

For proving this fact, and determining the improvement numerically, he has had recourse to the bills of London, Glasgow, Liverpool, Manchester, Warrington, Chester, Carlisle, and various country parishes in Lancashire and the adjoining counties. Few of these bills yield satisfactory results, as most of them are more or less defective. The following example, however, we shall select as being complete in all its parts, and made on a pretty extensive scale of years and population. In Warrington, a manufacturing town in Lancashire, of about thirteen thousand inhabitants, the average annual mortality for nine years subsequent to 1772 was one in 26.48, and of the deaths 55.12 per cent. were under ten: The average annual mortality for eight years after 1817 was one in 37.4, and of the deaths 44.65 were under ten. Consequently in every thousand inhabitants there died annually, of children under ten, 20.8 in the first period, and only 11.94 in the second. We shall presently find, from analogous data, that the improvement in Glasgow between the six years after 1782, and the six previous to 1812, was not much less, namely, from 20.3 to 13.58 in every thousand inhabitants.\*

In the next section Mr. Robertson proceeds to rectify an oversight which has been committed by Dr. Watt of Glasgow in his well-known observations on the Mortality of

Children. The subject of this section is one of very great importance.

Dr. Watt, as many of our readers must be aware, inferred from an examination of the Glasgow tables, that in that city the mortality among children had *not* improved between the two periods mentioned above; and that, as a manifest diminution had taken place by the abatement of small-pox, this improvement must have been compensated by a corresponding increase of deaths from other infantile diseases, more especially measles. In a late number of this Journal we have inserted a pointed contradiction of Watt's statements by Dr. Casper of Berlin. Dr. Casper found from the statistical tables of that metropolis, not only that the mortality among children was really and materially improved in the course of thirty years subsequent to 1790, but likewise that the decrease of deaths from small-pox did not account for the whole improvement; nay, that in proportion to the births there was an actual diminution in the number of deaths from measles. But Mr. Robertson has succeeded in contradicting the conclusions of Dr. Watt from the data which he himself has furnished. Dr. Watt's statement, as to the permanence of infantile mortality in Glasgow, was founded on the fact, that for six years subsequent to 1782 the deaths under ten years of age were 53.48 per cent. of the total deaths; while in the six years concluding with 1812 they were 55.43 per cent. Unfortunately, however, for the accuracy of his inference, Mr. Robertson shows that Dr. Watt did not take into account the great improvement which has taken place during the interval in the *total* mortality, and omitted to make the requisite correction, namely, by referring the infantile deaths to the population, instead of the general mortality. Let us now see what will be the result of that alteration. In the early period, the average annual mortality was one in 26.7, and of the deaths 53.48 per cent. were under ten: That is, among every thousand of the population there died annually 37.45, of whom 20.03 were under ten. In the latter period the annual mortality was one in 40.8, and of the deaths 55.43 per cent. were under ten. Hence in every thousand inhabitants 24.51 died annually, of whom 13.58 were under ten. So that in reality the deaths among children in a given number of inhabitants had decreased to two-thirds of what they were in the former period. We are not aware that this satisfactory correction has ever been made before.\*

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\* The mode of computation which refers the deaths in childhood to the population is much more correct than that which refers them to the general mortality. Nevertheless it is not quite exact. For when the mortality among children is improving in a greater proportion than the general mortality, which has for some time been the case every where, it follows that in recent times there must be a

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\* See Note \*, next column.

We shall now, however, take a wider view of the section in which the foregoing criticism is contained, and which treats generally of the alleged reciprocity between the deaths from small-pox and those from measles, scarlatina, and other infantile diseases. The author has been led to bestow some pains on the subject, as in Manchester the statements of Dr. Watt and others have had a very pernicious effect on the advancement of vaccination. His account of the state of vaccination in that city is indeed deplorable. From May 1815 to May 1823, the annual number vaccinated at the Infirmary and Lying-in Charity was 3161. From May 1824 to May 1826, although the paupers who repaired to these establishments for other purposes were much increased, the annual number of children vaccinated has fallen to 1309.

We are enabled to place Dr. Watt's statement of the increase of measles in Glasgow in a clear and satisfactory point of view from a document, which we regret Mr. Robertson does not seem to have had it in his power to consult. The increase observed by Dr. Watt in thirty years subsequent to 1782 was a twelve-fold ratio; and even after making the proper correction on account of the error mentioned above, and referring the deaths to the population, there will remain an increase in the ratio of one to seven. This great rise does not correspond with the increase elsewhere: In London, for example, a much wider field of observation, the increase between 1791 and 1820 has been only in the proportion of two to five. We were therefore led to conjecture that the great apparent increase at Glasgow must have been owing to the extraordinary prevalence of epidemics towards the close of the period included in Dr. Watt's table. And accordingly this will appear pretty evident from the table itself; for while the annual average of deaths from measles for ten years before 1813 was 166, the numbers in three of the six years which form the last period of the table, were 267, 304, and 787. But farther, on referring to the valuable statistical account of Glasgow published in 1820 by Mr. Cleland, it will be found, that the ratio of deaths from measles to the total deaths,—which ratio had risen, during the thirty years comprised in Dr. Watt's table, from 0.8 per cent. to 9.09,—was for four years subsequent to 1812 (the last year in that table,) only 4.19; for the next three years 3.0; or for the whole seven years 3.69. On consulting the table below, which we have calculated from the data furnished by Mr. Cleland from Dr. Watt's in-

greater proportion of infantile population in the same number of inhabitants than in more remote periods. Now it is properly to the infantile, not to the general population, that the infantile deaths should be referred. But it is by no means easy to ascertain, from present data, what the proportion of children in the population amounts to.

vestigations and his own inquiries, the reader will perceive, that while the yearly deaths from small-pox in Glasgow have decreased, during the forty years prior to 1820, from 63.2 in ten thousand inhabitants to 4.0, those from measles have increased from 3.0 to 7.9 only.\* Dr. Watt's estimate of the great increase of measles is therefore distinctly vitiated by the prevalence of epidemics towards the close of the period to which his inquiries were necessarily confined.

But although the decrease of small-pox is thus even in Glasgow very far from being compensated by the increase of measles, there is no doubt that the latter disease in proportion to the population, although not more common, as appears from Dr. Casper's inquiries, at Berlin, is materially so since the diffusion of vaccination both in London and Glasgow, and probably in Britain at large. Returning now, however, to our author, he maintains, that the increase, such as it is, arises from nothing else, than, on the one hand, from a greater number of children, who

\* The subjoined table comprehends five periods of six years each, and one period of seven years. The numbers in the per centage department differ from those in Mr. Robertson's table. We have verified their accuracy, however.

From 1783 to	Deaths from			Of 100 deaths, were from		In 10,000 Inhabitants there died yearly of	
	All Dis- eases.	Small- Pox.	Measles.	Small-Pox.	Measles.	Small-Pox.	Measles.
1788,	11544	1955	93	16.90	0.80	63.2	3.0
1794,	14174	2135	130	15.06	0.92		3.8
1800,	12950	1869	210	14.43	1.62	33.8	
1806,	13584	892	403	6.56	2.97	7.95	21.
1812,	15801	522	1437	3.30	9.09	4.0	7.9
1819,	22060	236	814	1.07	3.69		

used formerly to be carried off by small-pox, being now left to be affected by other infectious diseases; and, on the other hand, perhaps, from an increased fatality among the infected, which is owing, not, as some imagine, to the small-pox formerly cleansing and strengthening the constitution so as to enable it to resist future diseases, but simply to these diseases now finding a greater number of unsound constitutions to attack, which in former times were previously disposed of by the small-pox. And he denies altogether that any sound argument has ever been brought forward to prove, that the want of those mysterious changes, which are supposed to have been effected in the body by small-pox, does now either engender or leave undisturbed in any constitution a liability to be either more easily or more severely affected by measles and other disorders. On the contrary, "he is inclined to think," and we believe with great justice, "that the truth lies in an exactly opposite conclusion, and that one considerable cause of the decrease of mortality of late years in every part of the kingdom, is *the comparatively greater soundness of constitution enjoyed, since small-pox has given way.*"

In support of this opinion he mentions that consumption, which during last century had increased in a distressing ratio, appears to have begun to decrease at a date, after the general introduction of vaccination, corresponding with what would *a priori* be expected, considering the period of life at which the disease in question begins to prevail. This effect, if it has really taken place, cannot be expected to be yet very decided, because a sufficient time has not elapsed to bring a great number of the vaccinated to the age at which consumption is *most* prevalent. But still the improvement, judging from the London tables, is manifest, and certainly began about the period mentioned. At the close of last century the deaths from this disease had gradually increased during the previous hundred years from 15.4 to 26.3 per cent. of the total mortality. Between 1799 and 1808 they still continued to increase, being 27.8 per cent. But after that, namely down to 1808, they had decreased to 23.8; and between 1818 and 1825 to 22.2. As during the last thirty years there has been a great improvement in the general mortality in London, it is obvious, that, if the proper correction were made on this account, and the deaths from consumption taken in relation to the number of inhabitants, the improvement would be still more manifest. It is not easy to ascribe the decrease to an improvement in the political condition of the country, as many would be disposed to do; else why did consumption increase so much during last century?

The author might have further illustrated his opinion, that the absence of small-pox does not render the constitution more liable to other diseases, by the fact, that the mortality above the age of ten from all diseases is

lessened. We have already seen that the abatement of the mortality among children under ten is not nearly accounted for by the diminution of deaths from small-pox. It may now be added, that the improvement in the mortality under ten does not account for the improvement in the general mortality. Thus, taking the town of Warrington as an example: In the eight years subsequent to 1772, the total mortality was 37.7, and the infantile 20.8 in a thousand; while in the eight years previous to 1825, the total mortality was 27, and the infantile 12 in a thousand. Hence, for every thousand inhabitants, there died in the first period 16.9, in the second 15 above the age of ten; *being a diminution of nearly nine per cent. of such deaths.*

Let us now turn for a moment to the relative mortality among children of different ages, and to the relative change which has taken place among those of different ages in the course of the modern improvements in medicine and domestic life.

It is so well known that a vast proportion of the deaths among children occur during the first years of life, that it is unnecessary in this sketch to quote any of the fresh proofs to that effect which have been presented by Mr. Robertson. But it may be interesting to notice a few of the facts adduced by him to point out the very small mortality among children between five and fourteen. About the year 1780 the deaths between five and ten in Warrington, Chester, and Carlisle were, for every 10,000 inhabitants, only *fourteen and a third, thirteen, and twelve*, neglecting small fractions. In the Manchester Blue-coat Hospital, which contains eighty boys between six and fourteen years of age, there have died in twenty-six years only four, or one in 520 annually. In the Warrington Blue-coat School, among twenty-eight children, between eight and fourteen, none have died in twelve years. At the seminary for the children of the Society of Friends at Ackworth, containing 300 children between ten and fourteen, there have died in four years only three, being a mortality of one annually in 400.

To these statements it may be interesting to add some corresponding facts derived from the kindred institutions of this city. Heriot's Hospital is an institution for the maintenance and education of boys who are the children chiefly of the better class of operatives and lower class of shopkeepers. Certain chronic diseases disqualify for admission. The age of the children is from seven to fourteen. For the last seventeen years the average population has been 180, and the deaths have been thirteen, making an annual mortality of one in 235. Watson's Hospital is a similar institution for the children of deceased or decayed members of the Merchant Company, which consists of the superior class of shopkeepers and of merchants. From 1778 to 1811 the population was 55; from 1811 to 1820 it was 70; from 1820 to 1828 it has been 80. The deaths during these fifty years have been twenty-five; consequently the annual average

mortality is one in 123.4. The boys are from eight to fourteen years of age; and, as in Heriot's Hospital, certain chronic diseases disqualify. The nature of the Orphan Hospital needs no explanation. It contains boys and girls between the age of eight and fourteen. We have been unable to procure the particulars from which the estimate of the mortality is taken; but we have good authority for stating it during the eight years previous to 1823 so high as one in 37.5. The High School and Academy, the grammar schools of the city, are attended by boys from seven to fifteen years of age, who belong almost entirely to the middle ranks; and live with their parents or friends throughout the city. The population of the High School for the last six years has been on an average 680; that of the Academy since it was opened in October 1824 has been on an average 500. At the High School in six years *five* have died, being an annual mortality of one in 816: at the Academy in three years and-a-half *two* have died, being a proportion of one in 875. *The annual mortality of both institutions embracing a population of 1180, is only one out of 833.* These interesting facts are all derived from the most direct and authentic sources. It is evident that they would lead to much curious inquiry into the causes of the extraordinary difference of the mortality at different institutions.\*

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\* The very small mortality thus indicated among boys of the middle ranks of society in this city cannot be imputed to delicate children being educated at home, and consequently not included in the enumeration in the text. In point of fact, parents here, far from showing a propensity to pamper delicate children in their nurseries, generally send them to school with the rest; and it is consistent with the experience of our teachers, that their delicacy frequently wears off very soon under grammar school regimen. No species of physical education appears better calculated for ensuring health and hardiness. The boys have abundance of exercise of the most vigorous kind, and in all weathers; and at the same time they are constantly under the watchful eye of those most interested in their welfare.

The mortality in Watson's Hospital calls for one or two remarks. It is about double that of Heriot's Hospital, although the age of the boys, and general system of management, are as nearly as possible the same. But in fact, even this ratio is not the correct one. During the first ten years of the half century, namely the ten years after 1777, the mortality was one in 137.5; and in the last ten years it has been one in 71. May not this unfavourable ratio arise from the straitened circumstances of the parents previous to the admission of their children; and more particularly their sudden transition from ease and affluence to comparative poverty,—a transition which will of

It appears that the improvement in the total mortality and in infantile mortality, which has been produced in the course of the last thirty or forty years, has not affected much the mortality among children of the denomination we are now considering. In some places, indeed, one would be apt to suppose from the tables at first sight, that there was an increased mortality among them. Thus we find by calculation from the tables of Dr. Watt for Glasgow, and of Mr. Robertson for Warrington, that in the former place the deaths among children between five and ten for every 10,000 inhabitants had risen betwixt 1783 and 1812, from 12.8 to 13.7; and in Warrington, betwixt 1773 and 1825, from 14.3 to 19.3. This apparent augmentation, however, will be accounted for in the case of Warrington, and more than accounted for in the case of Glasgow, when it is considered that a ratio to the population is not altogether a fair way of calculating in the present instance. In fact, in late years, in consequence of the improved mortality among children under five, the proportion of children between five and ten to the total population has been very much increased. It is not easy to make the proper correction on this account; but it is plain that it will completely compensate for the apparent increased mortality in Warrington, and alter the apparent increase at Glasgow to a substantial diminution. Still it cannot be questioned that the improvement between the two periods of life alluded to has been small, compared with what has been effected for earlier ages.

We come now to consider one of the most interesting sections in the work, that namely which treats of the relative mortality from different diseases at different periods of childhood. Few tables on this subject have hitherto been published, and none of these are both extensive and accurate. Mr. Robertson has added to the stock one taken from the Register at the Rusholmeroad cemetery in Manches-

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course be much greater, and much more severely felt in their case, than by those of the class from which the population of Heriot's Hospital is supplied? Will not the increased degree of this wretchedness among the middle ranks since the peace account for the increased mortality among their children? The greater part of the deaths have arisen from diseases depending on general unsoundness of constitution.

The enormous mortality of the Orphan Hospital should lead to a strict inquiry into the physical management of the children, and salubrity of the situation where the building stands. An inquiry, in fact, was instituted in 1823, but we have not been able to learn whether it ever led to any precise conclusions as to the causes of the mortality. The situation is low; and in a quarter where the fogs which in spring and autumn infest the city are always peculiarly dense, and frequent.

ter. The register, he informs us, is kept with unusual care, and the part chosen by him extends through a period of four years after April 1821, including upwards of 2000 deaths under the age of ten. It would lead to too great details were we to examine this table with the fulness which would be required for discovering the results it yields. We must content ourselves with showing how it establishes one of the leading objects of the author's work, the importance of paying increased attention to the physical education of children. Of the 2056 deaths it appears that no fewer than 994 originate in diseases more or less directly connected with errors in physical education, and particularly in disorders of the bowels. "No facts," says our author, "can show more forcibly the importance which ought to be attached to the physical management of children. Upon it chiefly depends, under all circumstances, the healthy condition of the digestive organs; and when it is neglected or conducted in error, the foundation is laid for many definite as well as anomalous ailments."

There are two other subjects connected with the mortality of children which Mr. Robertson has investigated, the relative mortality of the sexes at different ages, and the relative mortality at different seasons of the year. Under both of these heads the reader will find some interesting discussion and information; but the conclusions at which he arrives are not very pointed.

The second part of the work treats of the physical education of children in regard to their health. This department of his subject is too concisely discussed to admit of a useful abridgment here. It consists of eleven sections,—1. On the structure, functions, and temperament of the body peculiar to infancy and childhood. 2. On the duties and qualifications of a wet nurse. 3. On the articles of aliment suitable for the early periods of life. 4. On the dieting of children in health and sickness. 5. On cleanliness, bathing, and the management of the skin. 6. On dress. 7. On air, temperature, and exposure. 8. On sleep. 9. On exercise and amusement. 10. On derangement of the alimentary canal, and on the management of the bowels. 11. On the hereditary transmission of peculiarities of structure, and the early management of such as are predisposed to particular diseases.

We have already observed that he appears to us to have given a very plain, judicious, and popular view of the physical management of children, avoiding equally the errors of those who attempt to ensure the health of infancy by hardening the body, and of those who think to accomplish the same thing by overnursing. Persons concerned in the physical education of children, parents, and others, will do well to consult our author's opinions and directions.

In the following particulars we cannot coincide with him. In the *first* place, he discards too absolutely wine and malt liquors as articles of diet for children. Given regularly they will certainly in general prove injurious; but their

occasional use has never appeared to us hurtful. Perhaps they do little good, given now and then to healthy children; but in laying down precepts on their physical education it is of some moment not to insist on what is matter of indifference,—in particular not to raise the voice against common practices, which are neither hurtful nor beneficial. *Secondly*, Mr. Robertson is too indiscriminate in his abuse of foundling hospitals. Their utility or hurtfulness is assuredly relative: in this country they would undoubtedly prove injurious to morals; in some continental states it is to be feared that their abolition, without improving the morality, would greatly increase the misery of the lower orders. *Thirdly*, We do not exactly comprehend his instructions never to use coke or charcoal as fuel in nurseries. If he means that they ought never to be burnt in choffers and away from the chimney, the injunction is correct, and we believe called for. But that coke and charcoal should not be used at all as fuel, on account of "the celerity with which they yield carbonic acid," appears to us a vague statement. Wood and coal yield carbonic acid quite as quickly, besides other vapours also, such as sulphurous acid and empyreuma, which when diluted are fully more pernicious than diluted carbonic acid. *Lastly*, He is unnecessarily severe on the use of gas for lighting apartments. His reasons for condemning it are that, when unburnt, carburetted hydrogen escapes, which is very deleterious, as Sir H. Davy's experiments show; and that when burnt long in a close apartment, the air becomes very offensive. On this subject we can speak with confidence, having had some personal experience regarding it. We take it that no person would submit to breathe for any length of time an atmosphere sensibly impregnated with unconsumed gas; nor if he did, do we conceive that any obvious harm would result. It has never, so far as we have been able to learn, been remarked, that coal-miners suffer materially from breathing a dilute atmosphere of light carburetted hydrogen; gas-workmen, who constantly breathe a dilute atmosphere of the mixed carburetted hydrogen gases, do so with impunity; and we may add, that on the occasion of some experiments, conducted along with a scientific friend, we spent the greater part of our working hours for three months in an atmosphere which all strangers felt disagreeable, without being able to observe any impression whatever on the health. As to the emanations from the consumed gas, these depend on the care with which the gas is manufactured and burnt. But good gas, burnt with ordinary care, although it does yield a little sulphurous acid, we know from the experience of several of our friends here, is very far from being the source of any annoyance in a dwelling-house.

We must close this analysis abruptly with expressing our sincere recommendation of Mr. Robertson's work, and our expectation of seeing him ere long employ the talent he has shown for medical statistics on wider, more varied, and more definite materials.

From the London Medical Gazette.

**SURGICAL OBSERVATIONS ON THE TREATMENT OF CHRONIC INFLAMMATION, in various Structures, particularly as exemplified in Diseases of the Joints.** By JOHN SCOTT, Surgeon to the London Ophthalmic Infirmary, and Assistant Surgeon to the London Hospital.

Mr. Scott of Bromley has long had an extensive reputation as a successful practitioner in various local diseases, difficult of cure, among which the most important are the diseases of the joints. His success has been attributed, partly to a greater attention to local treatment than is common in modern surgery, and partly to the care and dexterity with which he has applied it with his own hands. On these subjects, however, none but vague accounts had hitherto gone abroad until the appearance of the present volume, written by his son, Mr. John Scott, who has had ample opportunities of becoming acquainted with his father's opinions and remedies, and who has fully adopted them in his own practice. We shall proceed to condense or extract those parts of the work in which its chief novelty and value consist. It commences with an endeavour to ascertain the state of the blood-vessels in chronic inflammation, the local remedies by which this state may be removed, and the various local diseases, which however different in form and seat essentially consist in chronic inflammation, and admit of being cured by the same local treatment.

This state Mr. Scott thinks essentially consists in a dilated and feeble state of the venous circulation, accompanied by increased arterial action; the result of which is, that the blood-vessels are unable to propel their contents. This is especially the case in the lower extremities, where the depending position of the part is constantly favouring the influx of blood into it, and constantly retarding the efflux of blood from it.

"The true pathology of chronic ulceration in the lower extremities appears to be this. The ulcer is only the termination and effect of the chronic inflammation by which it is surrounded, and the former cannot be healed until the latter is removed. In the treatment, the direct object is not to heal the ulcer, but to cure the chronic inflammation; for if this can be effected, the ulcer heals spontaneously. The essential remedy for this state of things is mechanical support, which restores to the vessels the power of propelling their fluid along their canals."

Mr. Scott differs from Mr. Baynton both in his mode of applying mechanical support, and in his explanation of its action. Mr. Baynton thought that the absorbents of the leg were rendered inactive by being separated from the arteries by effused fluid, and that this effusion depended on relaxation of the common integuments. Mr. Scott thinks that the effusion into the limb is the result of chronic inflammation, not of deficient absorption.

"If a patient with an ulcer on his leg be confined to bed, the pain, redness, and tenderness will rapidly subside, and the ulcer will heal, in many cases, speedily; but as soon as he begins to use the limb, the inflammation will return, and again terminate in ulceration. If, however, the limb be subjected to mechanical support, the inflammation will subside, and the ulcer will heal as quickly as, and often quicker than if he were confined to bed. All ulcers, not of a specific nature, which occur in the upper extremity or the trunk of a healthy person are healed with facility; yet the only difference between the parts there situated and those of the lower extremity is in the course of the venous circulation. It is clear therefore that the obstinacy of ulcers in the lower extremities depends on the obstruction to the venous circulation, and this is corroborated by the fact that the means by which this obstruction is obviated immediately get rid of the obstinacy of the disease.

*Mechanical Support a Remedy for Chronic Inflammation.*—"Mechanical support is a remedy equally well adapted to ulcers on the lower extremity, whether they arise from a varicose state of the veins or not. It is capable also of affording great relief in many cases of chronic inflammation not so violent as to produce ulceration. In the former cases, it is not the ulceration that is the object of our solicitude, but the inflammatory action, which induces ulceration. The ulceration ceases as soon as the inflammation is arrested; and as this has been shown to depend on distention of the veins, which are no longer able to resist the gravitation of the blood, we have only to afford such an uniform support to the limb, as shall prevent the veins from yielding to the pressure of their contents. If we adopt the adhesive bandage with this view, it must be applied in a manner very different from that in which it is recommended by Mr. Baynton. He directs the "middle of the piece of plaster to be applied to the sound part of the limb, opposite to the inferior part of the ulcer, so that the lower edge of the plaster may be placed about an inch below the lower edge of the sore, and the ends drawn over the ulcer with as much gradual extension as the patient can well bear. Other slips are to be secured in the same way, each above and in contact with the other, until the whole surface of the sore and the limb are completely covered, at least one inch below, and two or three above, the diseased part."—"The force with which the ends are drawn over the limb must be gradually increased, and when the parts are restored to their natural ease and sensibility, which will soon happen, as much may be applied as the calico will bear, or the surgeon can exert."

"I could relate many instances in which this mode of applying the plaster bandage has been attended by great mischief. The pressure round the part of the leg encircled by the plaster and bandage is so much greater than at the lower part, where a roller only is

applied, that the venous circulation is so much impeded, as to cause considerable tumefaction of the foot and ankle. This produces extensive inflammation, which is propagated to the original seat of disease. Besides, in many instances, the inflammation of an ulcerated leg extends much more than an inch below the ulcer; so that, according to Mr. Baynton's directions, we are to apply to a portion only of the disease a remedy which, when so applied, aggravates the remainder; for I repeat that inflammation is the disease and ulceration only its consequence.

"Instead therefore of commencing the application of plasters an inch below the ulcer, it is necessary to afford equal support to the whole limb, in order effectually to bring about a uniform state of the circulation. The difference between using the adhesive bandage with this view, and with that of squeezing the parts that are swollen into their natural dimensions, will be obvious to every one. By applying the plasters to the whole limb in the manner I am about to describe, we at one relieve congestion, and the attendant inflammation. The pain subsides, the lymph and serum are secreted in a less abundant quantity, and absorption going on, the limb is less swelled on the following day. In this manner it is gradually reduced to its original size and figure.

"With regard to the method of fulfilling the foregoing indication, the emplastrum plumbi, P. L., spread on calico, is the best application, as it does not irritate the skin. It is most conveniently made use of when cut into slips of fifteen inches in length by two in breadth. The foot being placed at a right angle to the leg, one of the slips should be applied from the first bone of the great toe, along the inner edge of the foot, around the posterior part of the os calcis to the first bone of the little toe; the middle of another slip should then be placed under the bottom of the os calcis, and its ends extended perpendicularly up on each side of the leg; the third is to be applied along the foot, parallel to the first, and overlapping the half of it; the fourth should be placed parallel to the second, overlapping the half of it, and extending perpendicularly up the sides of the leg. In this manner they should be applied alternately along the foot and up the leg, the one holding and, as it were, antagonizing the other in the motions of the foot until the whole limb is covered from the toes to the knee. Subsequently to this a calico bandage is applied in the usual manner, first alternately around the foot and ankle, and then up the leg as high as the knee. It is necessary to be particularly careful that the plasters and bandage be applied in such a manner that their superior and inferior edges are accurately placed in apposition to the skin, otherwise they will exert an unequal pressure, which is highly injurious. The whole should be applied with only that degree of tightness which is perfectly agreeable to the feelings of the patient, and not with a view of compressing the parts into a

smaller space. In this manner every vessel in the limb will be uniformly and effectually supported.

"In respect to the time at which it will be necessary to renew the applications, that must be regulated by the quantity of the discharge; for when applied in the manner that has been described, they will remain for weeks, or even for months, without altering their position in the least.

"By adopting this mode of treatment, an ulcer on the lower extremity is placed precisely under the same circumstances in respect to the circulation, as one that has its seat on the trunk, or on the upper extremity; and will heal with equal facility."

*Mercury locally applied, a Remedy for Chronic Inflammation.*—"It is now universally acknowledged that mercury has the power of subduing inflammation; that when the whole vascular system is placed under the influence of this remedy, inflammatory action subsides.

"Mercury, however, when locally applied, has the same power of subduing chronic inflammation as when internally administered, and this without producing its constitutional effect; hence we may get all the benefit without any injurious effect. Whether mercury employed in this way really possesses the power I attribute to it is a question of fact which can be determined only by experience; but it appears to me that there is ample proof of the correctness of this opinion, and I will relate a few of the cases which have seemed to me decisive of the question."

Here follow many very interesting cases, showing the power of mercury locally applied in diseases of the bones, testicles, eye, and breast.

"It appears to me that the foregoing cases, which are only a small part of those which I could produce, are amply sufficient to prove, that, when chronic inflammation is going on in any texture, or in any part of the body to which local remedies are applicable, the local treatment, so far from being of the least, is of the greatest importance, and that the most effectual remedies of this kind are, mechanical support, and the local application of mercury.

"I now proceed to consider the employment of these remedies in a formidable class of affections,—namely, the diseases of the joints.

"The term white swelling was once indiscriminately applied to most of the chronic enlargements of the joints. It is now well known, that, however similar they become in their latter stages, the disease originates in different structures. But it is often impossible to distinguish, from the appearance presented at an advanced stage of these diseases, which structure was primarily affected; and the diagnosis chiefly depends on what information we can collect about the previous symptoms and progress of the complaint. This want of distinction, however, is of less practical moment than might have been supposed; the disease, although modified by the structure in which

It is seated, essentially consists in chronic inflammation, and its consequences. Hence the same principle must regulate our treatment in each form of these diseases; the practical object is to proportion the activity of the treatment to that of the disease, and this will be indicated by the urgency of the symptoms, in whichever structure disease is going on."

We pass over the account of the progress of disease, according as it begins in the synovial membrane, the cartilages, and the cancellous structure: also, the progress of disease, according as it attacks the knee or the hip joint; because, although the account is ably executed, and obviously depicted from nature, it contains little novelty to those who are intimately acquainted with Mr. Brodie's standard work on this subject. We likewise pass over, in the treatment of the disease, that part which relates to the constitutional treatment. After minute and full directions about the management of active purgatives, or alteratives, and aperients, according to the active or chronic state of the disease; tonics, alkalies, diet, rest, or exercise on crutches, according to the state of the disease; warm baths, dress, residence in the country, or at the sea side; Mr. Scott proceeds to describe the local treatment which, first in the hands of his father, and now in his own, has been applied so successfully in the chronic states of the diseases of the joints. All active inflammation being subdued by strict rest, local bleeding, and poultices, not cold lotions, to which Mr. Scott decidedly objects: he proceeds as follows:—

*Method of dressing Chronic Diseases of the Joints.*—"The above mentioned irritating, and sometimes very mischievous remedies, (blisters, setons, issues, moxa,) may be all superseded by the following treatment. In the first place, the surface of the joint, suppose the knee, is to be carefully cleansed by a sponge, soft brown soap, and warm water, and then thoroughly dried; next, this surface is to be rubbed by a sponge soaked in camphorated spirit of wine, and this is continued a minute or two, until it begins to feel warm, smarts somewhat, and looks red. It is now covered with a soft cerate made with equal parts of the ceratum saponis and the unguentum hydrargyri fortius cum camphorâ. This is thickly spread on large square pieces of lint, and applied entirely around the joint, extending for at least six inches, above and below the point at which the condyles of the femur are opposed to the head of the tibia; over this, to the same extent, the limb is to be uniformly supported by strips of calico, spread with the emplastrum plumbi of the London Pharmacopœia. These strips are about one inch and a half broad, and vary in length; some are fifteen inches, others a foot, others half these two lengths, and the shorter or longer are selected according to the size of the part round which they are to be applied. This is the only difficult part of the process. This adhesive bandage ought to be so applied as to preclude the motion of the joint, prevent the

feeble coats of the blood-vessels from being distended by the gravitation of their contents in the erect posture, and thereby promote their contraction. Over this adhesive bandage, thus applied, comes an additional covering of emplastrum saponis, spread on thick leather, and cut into four broad pieces, one for the front, the other for the back, the two others for the sides of the joint. Lastly, the whole is secured by means of a calico bandage, which is put on very gently, and rather for the purpose of securing the plaster, and giving greater thickness and security to the whole, than for the purpose of compressing the joint. This is an important point, as otherwise an application which almost invariably affords security and ease, may occasion pain, with all its attendant mischief.

"In some cases, in which the skin is thick and indolent, sufficient irritation will scarcely be excited by the above applications, and this may be promoted by rubbing on a small quantity of tartar-emetic ointment previously to the application of the cerate. This, however, is rarely necessary.

"In some cases, also, it is desirable more effectually to prevent the motion of the limb, particularly in children. This may be done, by applying on each side of the joint, externally to the plasters, a piece of pasteboard, softened by soaking in water, and cut into the length, breadth, and form of splints. These being soft, will accomodate themselves to the figure of the joint, and, when dry, effectually preclude all motion.

"The remedies thus applied will not require very frequent removal. The time during which they may be left undisturbed, will depend chiefly on the necessity for a repetition of the bleeding, in which we must be guided by the degree of pain, or when there are open abscesses, by the quantity of the discharge. Should neither of these influence the question, the only necessity for removing the dressings will arise from their having ceased to keep up any irritation in the skin. In some cases it will be necessary to reapply them every week; in the generality of instances, they may be allowed to remain a fortnight, and in others for a longer time. Even where there are open wounds, I allow them to remain several days, or a week, being firmly convinced by experience that the presence of the matter does less harm than the frequent disturbance of the part. A strumous ulcer can scarcely be disturbed too seldom; nothing does so much harm as officious dressing and probing.

"Consider the condition of a joint thus done up. First, it is thickly encased in emplastrum plumbi, leather and calico, by which perfect rest is ensured, and it is so supported and secured from external injury, as no longer to be a source of perpetual anxiety to the patient. It is a striking sight to see a child, who, before the application of these dressings, was in constant fear of being touched and moved, this fear keeping him in a perpetual state of nervous irritation, immediately after their ap-

plication losing all fear about his joint, and permitting himself to be touched and carried with perfect tranquillity of mind. This circumstance alone cuts off a constant source of irritation to the constitution.

"Besides this, the moderate, uniform, extended support which the plaster-bandage affords, is the best remedy for the vessels, weakened by long disease, and in that state which constitutes chronic inflammation. Of this, the best proof I can offer is the great efficacy of this bandage in old ulcers of the lower extremities, which are kept from healing by a chronic inflammation of the integuments, and which heal on curing this chronic inflammation by mechanical support. Let it never be forgotten, however, that this remedy is inadmissible, as long as active inflammation exists in the joint, which it is sure to aggravate, and that, in applying it, it is of the utmost importance to distinguish between a moderate and uniform support, which affords the full benefit I have been describing, and violent unequal compression, which, by impeding the circulation, is sure to aggravate the disease. There is only one rule that can be a safe guide in this respect—to apply the plaster-bandage in such a way as shall afford ease and comfort to the patient. If it occasions pain, either on its first application or subsequently, it is either applied badly, or the part is not in a fit state for it. So much for the mechanical mode in which this method of treatment operates.

"Next, it is a powerful means of exciting the vessels on the surface, and by that means of determining the blood from within. The skin is rubbed with camphorated spirit until it is red, and smart; it is constantly under the influence of an ointment strongly impregnated with camphor, and, by being enveloped in an impervious covering, the perspiration of the part is confined, so as to keep it constantly in a steam bath. In these ways the action of the vessels of the skin is greatly promoted, as is evident by the surface being no longer pale, and commonly becoming covered with a crop of slight pustules or vesicles.

"Lastly, this surface, thus kept in a constant state of augmented action, is exposed to the influence of a powerful mercurial preparation. That mercury is one of the most powerful means we possess for controlling the action of the capillary vessels, removing congestion, and subduing inflammation, has been so fully proved of late years, more especially in inflammation of the iris, that it would be superfluous to attempt to prove it. In these cases, the whole system must be subjected to the influence of the remedy, in order to control the disease of a part. In diseased joints, however, the debility and irritability of the constitution are so considerable, that if mercury be given so as to affect the system, it invariably aggravates the disease. The only question, therefore, is whether it exerts the same power when applied locally, without affecting the constitution. I am aware that the prevalent notions about the way in which mer-

cury operates are unfavourable to a belief in its local operation. But if mercury did not possess a power when locally applied, why is it ever employed as a local remedy? What shall we say to the many instances in which enlarged glands and other tumours waste and disappear under mercurial plasters? When mercury is introduced into the system by friction of the skin, must it not pass through the vessels of the part before it can reach the system? and how can it pass through these vessels without acting upon them? To deny it, would be to contend, that it did not act upon the part until the whole constitution became impregnated with the remedy, and, as it were, reflected its action again on the part from which it was received; a proposition which implies a much more minute knowledge of the way in which the remedy operates than any man in the profession possesses. I trust, however, that what I have already stated is conclusive on this subject; but, at all events, I am certain that the other remedies are not nearly so efficacious if the mercurial ointment be omitted.

"The remedies I have just detailed may be employed for any length of time, and over any extent of surface that may be necessary, without irritating the constitution, or producing salivation. They also admit of being varied and modified in as great a degree as the disease varies or is modified by them; they are consequently adapted to disease commencing in any of the structures of which a joint is composed, as well as to the various stages in which it may be found."

Notwithstanding the copiousness of these extracts, our limits prevent us from giving an adequate account of all the practical matter contained in this volume. We must, therefore, refer our readers to the book itself. It is written in a plain, clear style, and altogether in a way very creditable to the talents and professional acquirements of the author, but its principal value depends on the mode of treatment recommended, and this can be determined only by time. It has already been adopted by Mr. Hodgson of Birmingham, at present only in diseases of the elbow-joint, and to use his own words, "with very great success."

From the Nouvelle Bibliothèque Médicale.

**CASE OF INTERMITTENT AURICULOFACIAL NEURALGIA**, *successfully treated by the Sulphate of Quinine, employed in the Endermic Method*; by M. CAUCANAS.

Madame C——, æt. 30, of a lymphatic-nervous temperament, and enjoying habitual good health, was attacked after exposure on the sixth of April, 1827, with an acute and lacerating pain in the left ear. The following day, it increased in severity, and towards evening became so excruciating as to be almost insupportable. Frictions behind the ear with equal parts of ether and laudanum were directed, and a mixture consisting of Hoffmann's anodyne and laudanum was exhi-

bited. The latter succeeded in mitigating the pain, and the patient passed a tranquil night.

The next day the pain re-appeared, and involved almost the whole left side of the face. Fifteen leeches were directed to the mastoid apophysis, followed by the application of an emollient cataplasm. The symptoms were somewhat relieved by these measures, but re-acquired towards evening all their former intensity. Three spoonfuls of a mixture, composed of four ounces of lettuce water, twenty drops of laudanum, and an ounce of the syrup of diacodium were given, with the effect of terminating the paroxysm. Some slight degree of pain was felt on the morrow, for which twenty leeches were directed to the angle of the jaw, cataplasms, &c. The paroxysm returned in the evening, but with little diminished severity; the mixture above mentioned was exhibited, with the same good effects as before.

10th.—Anorexia. The pain was scarcely perceptible during the day, but the paroxysm returned at its usual hour, and was treated as before.

11th.—There being no improvement in the condition of the patient, it was decided to make trial of the carbonate of iron, which had appeared to M. Caucanus to have been productive of advantage in an analogous case, and two doses, of a scruple each, were accordingly exhibited; the paroxysm was less severe than before, but scarcely had it subsided it was re-excited, in consequence of fright, with greater violence than ever. The anodyne mixture above mentioned, was prescribed with its usual good effects. A scruple of the carbonate was given on the morning of the twelfth, but nausea and vomiting supervening, it was speedily rejected. The paroxysm recurred in the evening, and yielded to the ordinary treatment. A blister was applied to the arm. The carbonate was subsequently exhibited in the form of pills, but with the same result as before; inducing nausea and vomiting, which did not cease till they were expelled from the stomach. The paroxysm made its appearance three hours earlier, and with such violence as to occasion violent convulsions, loss of consciousness, &c. An antispasmodic mixture had some effect in abating the convulsions, but the neuralgic pains continued till one o'clock of the following morning, notwithstanding the frequent repetition of the anodyne. The patient stated that it appeared as if some one were tearing the flesh from the upper part of the chest, from the neck, jaw, cheek and eye, with pincers. Her nervous agitation was extreme.

The well marked periodical character of the disease, induced a recurrence to the quinine, but as it could not be administered internally, by reason of the great gastric irritability, it was decided to employ it according to the endermic method, and ten grains of the sulphate were applied to the dressings covering the blistered surface. The paroxysm did not recur; a sensation of numbness was felt in the diseased parts, and the blister appeared slight-

ly inflamed. The sulphate was re-applied during three successive days, gradually diminishing the quantity, and the health of the patient was speedily re-established.

The preceding case evinces the powerful effects of the sulphate of quinine in intermittent neuralgias, and the advantages of the endermic method, in cases where it cannot be administered internally.

Commenting upon the above, Dr. Bouche- nel supposes that it was rather a case of inflammation of the auriculo-facial ramifications of the portio dura, than neuralgia, properly so called. The following are the symptoms which he regards as characteristic of the former; the pain is more continued, never subsiding entirely, and subject to exacerbations more or less regular; the course of the nerve is painful upon pressure, and the sensibility of the neighbouring parts is augmented; occasionally the skin is redder than natural, and the motions of the jaw in mastication, &c. are painful. The duration of inflammation also, is much shorter, when it does not pass into a chronic state. Blood both generally and locally detracted is almost always followed by relief; revulsives are very useful in both cases, but in inflammation, instead of effecting a momentary alleviation, as in neuralgia, they ordinarily complete the cure.

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A PRACTICAL AND PATHOLOGICAL INQUIRY INTO THE SOURCES AND EFFECTS OF DERANGEMENTS OF THE DIGESTIVE ORGANS, *embracing Dejection, and some other affections of the Mind.* By WILLIAM COOKE, Member of the Royal College of Surgeons, &c. London, T. and G. Underwood.

Since the digestive organs have been discovered to bear one relation to the rest of the system, scarcely any disease to which mankind are subject, has not been attributed by one author or another to derangement of some part, or the whole, of this apparatus. By one class, the liver is considered a most vile organ, answering scarcely any other purpose in the animal economy than that of nourishing disease and of propagating it to the brain and all other vital parts; the stomach is held by another class to be the nidus in which all human maladies are generated and nurtured. This voracious creature is always ready, with open mouth, to take in every thing poisonous and deadly, whereas it rejects with disdain, those simple and kindly substances which were so congenial to its nature in days of yore. As for the mucous membrane of this organ and of the intestines, we should do much better without it; for is it not the seat of typhus fever, of asthma, of diabetes, of hydrophobia, of small-pox, and of many deadly maladies? Nay, every branch of the tree of nosology may be traced to this morbid root; whose thirst for the vital fluid can be only exceeded by the energy with which the doctor drains this fluid through another channel; and during this

mighty conflict the poor system is obliged to stand, or (as it generally does) fall by itself. If, as we find in a work just published, the whole of this intricate machinery, so liable to derangement, have no other office to perform than that of manufacturing *electricity* to supply the *soul* with *food*, it would be better at once to let it fall to rest, and to furnish ourselves with portable electrifying machines, in order to give the soul a dose through some other channel, as often as she shall want it.

Those who fancy that they can trace every malady to the digestive organs, should try whether they cannot trace the disorder of these organs, also, sometimes to affections of other seats. For our own part we can find no difficulty in doing so, and they would probably find as little if they took the trouble of examining both sides of the question. But the greatest obstacle in their way consists in their laying it down as a maxim that the digestive organs are invariably the seat of disease, instead of proposing to themselves the question, whether or not they be so? That these organs often neglect their duty, or perform it in an improper manner, will be readily admitted, but why they alone should always be accused of guilt, requires some further proof to convince us that they deserve it than we have already had offered us. Do we find the functions of digestion deranged much oftener than those of the skin, kidneys, mucous membrane of the bronchia, &c.? Can the priority of derangement be always traced to the former? How happens it in compound fracture, in concussion of the brain, in common catarrh, and several other affections, where the digestive apparatus *cannot*, according to our present anatomical and physiological knowledge, generate the malady? John Hunter says in the motto which Mr. Cooke has selected for his work, "the stomach sympathizes with every part of an animal, and every part sympathizes with the stomach." But some of Hunter's disciples would say, "the stomach sympathizes with *no* part of an animal, *but every part* sympathizes with the stomach." Mr. Cooke, we are glad to find, adheres to his motto, and traces the connexion between diseases of the digestive organs and those of other seats, without referring the source of the effects to the former organs alone.

The work is divided into two parts, in the first of which the author inquires into the various sources of dyspeptic symptoms; and in the second he treats of the effects produced on remote parts of the body and on the mind by derangements of the digestive organs. He sets out with affections of the mucous membrane. In persons who have died of other diseases, after a long continuance of dyspepsia, he has often found the mucous glands enlarged, but more frequently in a state of vascular plethora. This affection may be usually distinguished by attentive inquiry into the history of the disease, and by a careful investigation of the patient's sufferings. He justly considers that an erythematous condition of the mucous membrane of the intestines, not only deranges the

digestive functions by its direct influence, but that it also exerts an influence on other organs, particularly the liver, whose functions are essential to the formation of chyle. It will excite this viscus into excessive and unhealthy secretion of bile, from which erythema of the whole canal might ensue. This erythematous state of the mucous membrane frequently gives rise to troublesome diarrhœa, or diarrhœa alternating with constipation, and generally requires astringent and aromatic remedies, after moderate local bleeding.

The next subject is pyrosis, which Mr. Cooke considers to depend upon a morbid state of the mucous glands of the stomach. The quantity of watery fluid secreted by the stomach in this affection is sometimes very considerable. The author mentions the case of Morgagni, who, after swallowing a small leaf, had pain at the stomach and sickness, and he discharged sixteen pints of fluid in twenty-four hours. The fluid discharged is generally acid, so much so at times, as to set the teeth on edge. Mr. Cooke generally relies on astringents, such as alum and kino, combined with opium, in this disease; carefully regulating both the quality and quantity of the ingesta.

In speaking of hemorrhage from the alimentary canal, the author notices that it may arise from rupture of a blood vessel, from ulceration, from irritative action, or from a passive oozing from the the turgid villi, pervading often a considerable part of the canal, which may retain after death a dark red colour. We have an interesting case related, where bleeding to a very great extent took place in the stomach, from a mere hemorrhagic action of a small spot of the villous coat. The subject of this case was a young lady, fourteen years of age, who had been a frequent sufferer from bilious and dyspeptic symptoms, through the whole of her life. Her catamenia had appeared, and she had been much better in health for some months before she was attacked with vomiting of blood. She continued for several days to bring up a large quantity, which reduced her strength very considerably. She, however, gradually recovered from this attack, but after a year had elapsed, she had a recurrence of the complaint, of which she died, after vomiting up blood in large quantities, for five or six days.

"As this young lady had discharged at least ten pints of blood within the month, it became an object of great interest to ascertain the source, and the family cheerfully complied with my request.

"On the 9th I examined the body. On opening the abdomen the omentum presented itself in its natural position, covered with a yellowish fat. The intestines were distended, and looked black in some places, as if from dark matter contained in them.

"The liver was considerably enlarged, extending to the left hypochondrium. It was of a reddish colour, and scirrhus throughout. The gall-bladder was distended with bile of a nearly black colour, but which on being dilut-

ed, became yellowish green—showing that its colour was not from blood. The coats of this viscus were thickened. The spleen was enlarged to double its ordinary bulk, having the appearance and solidity of healthy liver, and containing but little blood. When the stomach was cut into, the mucous coat appeared healthy, except near the cardiac orifice. Here there was a spot of redness which presented the appearance of inflammation surrounded with a radius of turgid vessels and ecchymosis, extending about two inches. Though this evidently was the spot from which hæmorrhage had taken place, yet no abrasion was discoverable. The blood had flowed from a mere point, but hæmorrhagic action had been set up in the circumjacent vessels, just as we often see it from a leech bite. There were numerous petechiæ within the œsophagus. The duodenum contained some thick yellowish matter. In the jejunum and ileum there was a large quantity of black morbid matter, which in some places was distinctly mixed with blood, and resembled currant jelly. Within the colon we found some black fæces, and the mucous coat was of a deep red colour—not from turgescence of vessels, but from effusion into the subjacent cellular tissue.

“There was a considerable quantity of fat deposited about the kidneys, and these organs themselves were rather large. The pelvis of the left contained some puriform fluid, and there was a large quantity of mucus in the bladder.

“The veins of the mesentery were so distended by coagulated blood as to give them an appearance of vessels filled with a black injection.

“There was a small quantity of serous deposition in the cavities of the thorax and pericardium, and also in the substance of the lungs.”

Aphtha is a disease considered of little consequence by nurses, and often so even by medical attendants. But it undoubtedly deserves more particular attention than it generally receives. The aphthous eruption in the mouth is a part of its character which requires the least notice. Our attention should be more particularly directed, and our remedies applied to the mucous lining of the alimentary tube, which is, perhaps, always more or less affected in this complaint. The following case will show the morbid condition in which the mucous membrane is frequently found after death produced by aphtha:—

“Mrs. H—, the mother of several fine and healthy children, was confined on the 7th of December, 1826. The infant appeared perfectly healthy, like all the former children. About a fortnight after its birth, aphthæ were observed in the mouth, and the excretions were unhealthy. The nurse used some armenian bole, but a week afterwards the bowels became so much disordered that I was consulted. Some cretaceous and astringent medicine, with an occasional dose of rhubarb, was first recommended. At this time the infant lived almost exclusively on the mother, who

had plenty of milk, but her nipples were extremely sore. Sickness of the most distressing kind supervened, and the little sufferer ejected every thing taken into the stomach, and consequently daily sunk. The woman who had the charge of the mother and infant was making her *debut* as a monthly nurse, and was not very expert or judicious in the management. The mouth became covered with a thick white sordes, extending over the palate and lining the cheeks and fauces. On its being wiped off by means of a soft mop the part appeared raw. Some difficulty of deglutition arose from the accumulation of this matter in the fauces, where it resembled curd. I observed, too, that whenever put to the breast the mother shrunk from it on account of the pain it occasioned, and this, I apprehend, connected with her anxiety, had been detrimental to the infant from the first. Occasionally there was a tinge of yellowness on the skin. The urine was high coloured, and the stools green and fetid. The child lingered on till the 22d of January.

*Examination.*—The mouth was not so thickly beset with aphthous eruption as it had been. There was no appearance of disease in the course of the œsophagus except near the cardiac orifice of the stomach, where the vessels were turgid; and the mucous glands were universally developed.

“The mucous coat of the stomach was slightly inflamed in patches. Parts of the jejunum and ileum exhibited the appearance of a turgid state of vessels, conjoined with glandular swelling; but at the termination of the ileum, and through the whole of the colon, the mucous membrane was of a deep red colour. The muciparous glands were much enlarged, and in some points slight abrasion had taken place. There were fæces in the colon.

“The liver presented rather the appearance of congestion, but the colour of this viscus at birth, and for a little time afterwards, is usually rather deeper than in the adult. The inflammation of the inner coat of the stomach accounted for the sickness.”

The next subject is gastritis. The following case is rather an uncommon one, both in regard to its symptoms and to the morbid appearances discovered after death. The inference which Mr. Cooke seems to draw respecting the cause of death is quite as extraordinary as the appearances themselves, namely, that a few five-grain doses of calomel should produce sphacelus of the stomach.

“On the 10th of July, 1823, I was requested to see a young lass, four years of age. The parents informed me that she had been ill about a month, and under the care of a neighbouring surgeon. He considered it an attack of remittent fever. As the child was not better at the end of a fortnight, a physician was consulted, who regarded the case as one of peritonitis, and prescribed leeches, which were four times applied, but in the whole amounted only to 20. About ten days more had elapsed when the friends thought that death was approaching and sent for me, but the case was hopeless. At

three o'clock the following day the child died, and having been informed that the medical attendants had not coincided as to the nature of the disease, I was the more anxious to examine the body, and the parents readily assented.

"Before proceeding with the examination, I obtained the following information:—The child had been healthy till the commencement of this indisposition, when, after a hearty meal, she was attacked with sickness, the bowels became torpid, and the dejections of an unhealthy colour. Fever succeeded, and was accompanied with dyspnoea. The gentleman first called in told me that he had thought it right to act boldly, and had given very powerful medicines; for instance, six grains of calomel at a dose repeatedly. After the first attack there was but little sickness, and there had been very slight expressions of pain. When I saw the child there was great prostration of strength, respiration with difficulty, the tongue was dry, and the teeth were covered with sordes. The pulse was very quick, and there were excessive restlessness and moaning. She had pretty constantly lain on the right side, and superficial sphacelation had taken place from pressure. There also was a petechial spot on the abdomen. The countenance was pallid, the eyes depressed, and during sleep the palpebrae were unclosed.

"The two medical gentlemen were present when I examined the body. The physician, (a very judicious man,) had abandoned the idea of peritonitis, and now predicted that few or no morbid appearances would be found.

"The abdomen was tumid and tense, and on opening the cavity, there certainly was no trace of peritoneal inflammation. The stomach and intestines were greatly inflated, and on merely turning the stomach to one side its contents escaped. The viscus and a portion of the oesophagus were carefully removed, and the inner membrane of the latter was found to be inflamed, and the whole of the adjacent portion of the stomach was in a state of sphacelation of a deep black colour, and the texture destroyed. Inflammation extended some distance beyond the part where sphacelation existed, and the disorganized part was equal in extent to the palm of an adult's hand. There were no other morbid appearances except slight marks of congestion in the liver, and a few patches in the intestines. The physician considered it an extraordinary circumstance that gastritis should have existed without its being accompanied with vomiting. It appeared to me that no inconsiderable mischief, if not the fatal result, was attributed to the injudicious practice of the apothecary, who, however, was rather inclined to exult at his boldness before the physician was consulted."

Softening of the intestinal tube is a disease not very frequently met with. It is not easy to account for this affection where it occurs unattended with inflammation. We are rather sceptical upon the point that the coats of the stomach can be acted upon by the gastric juice after death, unless the fluid should contain some acid or other properties different from

those which are natural to it. That the gastric fluid is endued with vital properties, can hardly be denied; but from whence does it derive these properties? Is it not from the stomach? If it be so, it is not probable that this fluid can preserve its vitality longer than the stomach itself, which is the only source of this vitality; and it is quite as improbable that the texture of the organ can undergo the process of digestion before it has lost its vital properties.

"In April, 1826, I was requested to see an infant six months old. It had previously been remarkably healthy, and had cut two teeth, but became ill about the beginning of the month. The symptoms were those of cold; namely, cough, wheezing, sneezing, and inflammation of the conjunctivæ. A week elapsed without any advice, and when I saw the child there was difficulty of breathing and frequent cough; and the piercing cries and green stools indicated pain also in the bowels. Leeches, a blister, antimonials, and mild aperients were the means employed, and in a few days there appeared some amendment. At the end of a week symptoms of cerebral affection became manifest. During the preceding part of the indisposition there had been often knitting of the eye-brows, but it seemed rather an expression of pain in other parts than of disease in the head, but now the child fell into a state of coma and became convulsed. Leeches were applied to the temples, half a grain of calomel with a little antimony was given every four hours, and a blister was applied to the nape of the neck. On the second day from this time the coma and convulsions ceased. The calomel was omitted, but saline and antimonial remedies were continued. Whilst we were buoying ourselves up with lively expectations of recovery, the respiration became more difficult, diarrhoea supervened, accompanied with great rumbling in the bowels from wind, and within a few days, namely, on the 18th of April, the little sufferer expired.

"On dividing the integuments I found a large quantity of fatty substance remaining. The posterior surface of the lungs was the seat of inflammation, but not to a great extent. There was no sign of inflammation in the abdomen, but on tracing the jejunum I found it tear in several places, like wetted paper. The coats here looked exactly like jelly, semi-transparent and soft. In the interspaces they were firm as usual. The fundus of the stomach was precisely in the same condition. The appearance greatly resembled that produced by the action of the gastric juice, but as there had been previous disorder of the gastric organs, and as the intestine had undergone the same change as the stomach, it seemed rather the effect of a peculiar morbid process than of chemical action.

"The brain was soft, and considerable effusion had taken place between the membranes, but there was no unusual turgescence of vessels."

The author next recites cases of disorder of the digestive organs caused by chronic inflammation of the peritoneum, and by renal irrita-

tion. We have cases also to show the influence of the brain over these organs. The subject is one of great interest; every practitioner must have witnessed, in children, cases where it was difficult, if not impossible, to find out the real seat of disease. The relation, or, as it is usually termed, the sympathy between the brain and the abdominal viscera, at the age of childhood, is so intimate, that disease in the one seat seldom takes place without affecting the other, and it is often difficult to trace the malady to its origin. Whether the brain, in children, be more susceptible than any other organ, to idiopathic affections, is a question which, perhaps, has not been satisfactorily settled, but so far as our observation has extended, in the examination of the bodies of children after death, the cerebral membranes will be found either very much injected with blood, or to have a gelatinous substance adhering to them; or, lastly, there will be fluid found between them, or in the ventricles, in seven cases out of ten, if not in a greater proportion. Mr. Cooke relates a case of cerebral congestion, producing convulsions and intestinal contractions, which we consider a good specimen of the morbid appearances most frequently found in the brain under these circumstances. Although the case is such as we very frequently meet with, it is not the less interesting for that.

"In the evening of October 22d, 1822, I was requested to see an infant three or four months old. The child had been seized with convulsions twice in the afternoon, and for some days it had been observed straining violently. I was informed by the mother, who had nursed several children, that the bowels were freely open, and that the stools were of a good colour; and yet the straining continued. At the time of my visit it was considerable, and the countenance had a somewhat leaden aspect. There was constant rumbling from the increased peristaltic action of the bowels and from wind, and the abdomen was rather tumid. With the exception of taking a little gruel once soon after birth, the infant had lived exclusively on the breast, and had appeared healthy. There was no convulsions when I saw the child, and therefore I prescribed for it a dose of rhubarb, and some *mistura cretæ* to take afterwards to allay intestinal irritation. On calling the next morning I found the little patient dead. Convulsions had come on early in the morning, and the child expired.

"The body looked rather yellow, and the nurse informed me that there had been this tinge for a day or two. On dividing the abdominal integuments I observed that the cellular substance had received the same discolouration.

"Nothing unnatural presented itself in the abdomen except a somewhat turgid state of the vessels of the stomach. The omentum was very beautiful, from the numerous lines and inosculation of vessels ramifying through a most delicate and transparent tissue, but the appearance was natural. The intestines, as in the former case, presented marks of irregular

contraction, and were rather of a redder hue than usual. The gall-bladder contained bile of a dirty brown colour, and there was no perceptible obstruction in the ducts. The urinary bladder was greatly distended—projecting high into the abdomen—yet so large a quantity of urine had been voided that the nurse's attention had been particularly directed to the circumstance. The stomach was empty, but the mucous coat presented considerable erubescence. The kidneys were rather large.

"In the chest the thymus gland presented itself as usual at this age. The lungs were healthy. The pericardium contained a little fluid, and the vessels of the heart, as well as its cavities, were turgid with fluid blood.

"On opening the head I found the pia mater extremely vascular, and in the interstices of the larger vessels there was a serous deposition, which presented both the appearance and consistence of a soft jelly. The ventricles contained a little fluid in their posterior cornua, and the plexuses, particularly the left, had their glandules much enlarged."

Mr. Cooke treats next of disease of the pancreas, spleen, and liver. In illustration of his remarks on liver disease, he favours us with a great number of interesting cases, but our limits will not permit of our extracting any of them.

We now come to the last chapter of the first part of the work, wherein the author speaks of the influence of the nervous system, and the mind, on the digestive organs. In this country, where the minds of a great portion of the inhabitants are constantly engaged in commercial and similar pursuits, the nervous system contributes perhaps more than its share to the derangement of the digestive functions. Few there are who have not at times experienced some mental anxiety, either of painful hope or the fear of disappointment, and those who have ever been under the influence of such anxiety must be sensible of the effects which it produces on the gastric functions. Perhaps no source of dyspepsia is more fruitful than mental emotion, at any rate no exciting cause can bring on an attack in a more sudden manner. But, it is not the functions of digestion alone that submit to the agency of this powerful cause; the function of almost every organ in the body is either deranged or suspended. The heart either acquires a degree of morbid irritability, or loses a part of its natural sensibility, from the expenditure of the nervous influence on the brain; the stomach rejects that which is presented to it, or retains it in the same state as it was taken in, where it produces a sense of weight, flatulence, and a morbid secretion of acid fluid; the liver forgets that it has any duty to perform in the circle of assimilation, and the stools become often fluid and of a white clay colour, or the bowels lose their natural sensibility, and become obstinately costive; the renal secretion is almost suppressed, or the kidneys assume a diabetic action; the function of the skin becomes deranged, and the muscles lose a great part of their vigour

and tonicity. Whether the medium of relation between the different organs be formed by the nervous system, or by some other agent of a more subtle kind, we shall not now inquire; but sure it is that other organs are as much within the sphere of morbid sympathies as the digestive. We cannot paint this subject in stronger colours than Mr. Cooke has already done.

"To determine the influence which the nervous system exerts over the process of secretion, we need only advert to phenomena of daily occurrence. What a redundancy of tears, and even of urine will sometimes be secreted under mental uneasiness! Not unfrequently, however, we witness the very reverse of this, especially when the feeling of distress is intense. Of this a few years ago a most painful instance passed under my observation. An amiable couple, who had been married about a twelvemonth, and who had realized the fruition of hope in the person of a little daughter, had temporary lodgings in the vicinity of town. Whilst the wife prepared the breakfast the husband accompanied some friends to an adjoining stream to bathe. All was made ready for the repast, and the wife, a woman of the acutest sensibility, sat at the window nursing her lively and healthy babe, watching for the first glimpse of their return, and intending to welcome her husband by the joint attractions of her own affectionate and cheerful countenance, and the lovely face of their little offspring. It has justly been said that "hope deferred maketh the heart sick." The time elapsed, messenger after messenger was despatched, but none returned. The fact was the husband was drowned, and the party waited the arrival of a relative from town whom they considered the best channel of communication to the widow. It will be readily conceived that her mental agony was of the most poignant character, and for many days secretion was almost totally suspended. She was unable to weep. The dejections, which were obtained with difficulty, and were very scanty, were almost colourless; and after the lapse of two or three days she became affected with sub-acute hepatitis. The skin and tongue were uniformly dry, and the kidneys did not secrete an ounce in twenty-four hours. Weeping seems not only an expression but an outlet of sorrow. It appears to sooth the mind, and to allay the irritability of the nervous system. When there exists the inability to weep, the crisis of mental agony from which a mitigation of suffering commences, is protracted.

"Upon no series of organs is the influence of mind more forcibly evinced than on those concerned in digestion. At a moment of full health, and about, with a keen appetite, to partake of the pleasures of the table, the sight of a loathsome object, or the hearing of some distressing tidings, will so disturb the nervous system that articles previously the most inviting to the palate excite disgust. If these occurrences transpire immediately after the stomach is replenished, digestion is sus-

pended, the food lies a heavy and oppressive load, of which the patient is not relieved till the organ has ejected its contents. If the food is retained under these circumstances it proves a source of gastric and intestinal irritation, giving rise to acidity, flatulence, colic pains, and ultimately, perhaps, to diarrhoea. Where there is no relation to food, a dull aching and sense of anxiety at the pit of the stomach, in the region of the solar plexus, long remains, even when the mind needs to be reminded of the cause. Not unfrequently mental uneasiness produces diarrhoea without any other obvious effect. Sometimes the agency of mind is exerted on the whole of the abdominal organs, at others on an individual organ, the whole, perhaps, becoming affected consecutively. The effect exerted on the stomach and intestines has been adverted to, and indeed, also on the liver. Perhaps the functions of no organ in the body are more frequently deranged from this cause than those by which bile is formed, and from whatever cause the liver becomes affected, its action or reaction on the mind and nervous system is of the most extraordinary kind.

"Often when the emotion is severe the effects are so manifest that no doubt can be entertained of their connexion. At other times the effects being less prominent, and not productive of material inconvenience, they command little attention, but may lay the foundation of future mischief. When the cause producing morbid action is withdrawn, the derangement usually adjusts itself, or is adjusted by those casual circumstances by which the balance of the vital functions is maintained. These happy results, however, do not always follow. There may be a continuance of disordered function, and the morbid action may issue in organic lesion, not only of the part itself, but extending to other structures, involving them in the same mischief. In most cases of disorganization the sufferer is quite incapable of tracing the disease to its commencement, or to the circumstances in which they originated, and it is highly probable, when not the result of age, a large number of the instances originate in the obscure manner just adverted to: not, indeed, always from mental emotion, but from any other causes of disturbance in the nervous system. When we consider the numerous sources of emotions and passions, from domestic causes, from business, and from topics of popular excitement, we shall regard this as a fruitful and prevailing cause: but we must not exclude diseases in remote parts acting on the brain and nerves, atmospheric influence, indiscretion in diet, and many other circumstances which have a tendency to disturb healthy action."

And we believe our readers will agree with us in opinion, that the following observations are founded on truth:—

"If then this view of the different sources of derangement in the organs concerned in digestion be correct, how is it possible that any single measure can be devised by which

the disorder will be adjusted! To rely on the same medicines, to prescribe the same diet, to establish the same intervals of taking food, to enjoin the same exercises, is nothing else than empiricism. Even if we admit that the effect of the various causes may be alike, surely there must be some reference in the treatment to the cause itself, and likewise to the constitution. Sometimes bleeding will be requisite—sometimes active purging—sometimes perseverance in the mildest doses of mercurial remedies—sometimes bitters or tonics. At all times the diet must be most carefully regulated, but we should bear in mind that there are persons who cannot undergo very long intervals between the seasons of taking nourishment without being distressed, and this is particularly the case with children. I do not advocate a system of repletion, nor the ridiculous practices of parents who allow their children to be almost constantly eating; yet the digestion of children is generally more rapid than that of adults, and how often do we see some children in a school, or family, who cannot endure the same interval as their companions, but in attempting it are reduced to a state of extreme irritability and languor.

“With respect to the administration of purgatives, there are three points to which I shall advert. There will sometimes be fatal accumulation of feces in the intestines, when both the patient and attendants report that the bowels are freely relieved. When the obstruction arises from a mechanical cause, as hernia, or contraction, great caution is necessary in the administration of purgatives. Purgatives are not unfrequently persevered in to remedy unhealthy secretions, pain, tenderness, and flatulence, solely kept up by the means employed.”

“Whenever dyspeptic symptoms arise from suspended or morbid action in the liver, alterative doses of mercury are usually had recourse to. This is, perhaps, the most common form of derangement, and in its treatment Mr. Abernethy has justly acquired great celebrity. His blue pill administered in a dose, and at intervals, short of irritation, aided by decoction of sarsaparilla, has been most extensively useful. But I may venture to submit that Mr. Abernethy himself has circumscribed his principles too much. He has often been prescribing for an effect without regarding the diversities of cause. The principles, however, which he himself has enjoined have been greatly abused, and many practitioners, whilst avowedly acting upon them, have increased the derangement in the functions of the liver by the too free administration of the remedy. For an adult five grains may be an adequate dose, but it is impossible to determine this with certainty; for some persons are so easily acted upon by mercury, that half the quantity might be as much as can be borne. Not unfrequently it is necessary to diversify the form of the mercurial, and this may be done without violating the principle. Calomel, under some circumstances, will exert a decidedly more beneficial

effect than the blue pill. Advantage will sometimes be derived from the employment of mercurial remedies at much shorter intervals than at others; the inaction of the liver resists the effects of doses administered at the interval of two days, whilst the same quantity taken in divided doses, thrice a day, excites and establishes secretion. Whatever plan is adopted the effect must be watched, for if mercury do not act well, it might, if long persisted in, do excessive mischief. Green and slimy stools are often the result of the excess of mercury, and I believe it is not very uncommon with some practitioners to redouble their forces when these appearances are detected, by which practice they aggravate symptoms they intend to allay. Cases very frequently occur in which, by reducing a grain of calomel to half or a quarter of that quantity, or blue pill from five grains to two or three, the secretions, from being of the morbid qualities just mentioned, assume a healthy character. As it is not the design of this administration of mercury to act on the bowels, except secondarily, patients should be made aware of this circumstance, that they may take some mild aperient if requisite. It is really surprising what essential relief is sometimes experienced from such medicines as gently excite hepatic secretion before any alvine excretion has ensued, but patients are often rendered uncomfortable from an idea that they ought to be purged.”

Mr. Cooke, after tracing, in the first part of the work, the sources of derangement of the digestive organs, to diseases of other viscera, proceeds, in the second part, to inquire into the influence of this derangement on the functions of remote seats, and on the mind. We have first, affections of the head from gastric derangement. The most common effect produced on the brain, by gastric disorder, is cerebral congestion. The vessels of the brain appear to lose their tone, in a great measure, and allow themselves to be dilated, by their contents, beyond their usual caliber. In these cases, general bleeding commonly aggravates the cephalic symptoms, and relief is often procured from cordial and stimulating remedies. Local bleeding, however, by leeches or cupping, and counter irritation applied at a short distance from the head, will tend materially to the relief of the patient.

Thickening of the cerebral membranes, and cerebral effusions, are, also, no uncommon appearances in case of death from disease of the abdominal viscera. The following case will show the connexion of the cerebral disease with that of the heart and abdominal viscera:—

“Miss P——, thirty-six years of age, of a dark complexion, and having a dejected and ill-tempered countenance, complained of uneasiness in the stomach, with extreme depression of mind, and excessive irritability. She had frequent sensations of heat and fulness in different parts of the body, but when these parts were touched by another person they did not communicate any sense of heightened

temperature. The bowels were rather confined, the appetite was impaired, and the eructations were acid. There was pain in the forehead, and occasionally slight confusion of mind. The feet usually were cold. The pulse was ninety-six, and moderately soft. Whenever she attempted to sleep she was roused by violent startings, occasioned by the most horrific dreams and visions. She had tried blisters and some domestic remedies without any good effect. Under these circumstances I was consulted on the 19th of July, 1813, and recommended a grain of calomel to be taken every other night, and some infusion of roses and sulphate of magnesia three times a day. With the first pill she took a saline draught, and did not commence her mixture till the following day. She enjoyed a more comfortable night, and seemed more tranquil in the morning. The bowels had been moved once, but they were afterwards kept freely open by the neutral salts. By the 25th she was so much better, in every respect, that I discontinued my attendance.

"I did not see this lady again during her life, but having heard of her decease on the 29th of September, I obtained permission to examine the body. Before proceeding to the inspection the following report was given me. After my attendance in July she left home for awhile, and seemed much better, but at length the illusive visions returned; she complained of violent pain in the vertex, but extending to the forehead and occiput, sometimes affecting one part more than another. She had frequent flushings, and for months no menstruation. Though constipation was so evidently connected with the early symptoms the bowels were reported to have been perfectly regular during the latter period, confirming a remark already made on the changes which take place on the development of cerebral disease. At one period the powers of the mind were so bewildered, even in the day, that it was judged necessary to confine her, and she was in a lunatic asylum for three weeks. From this period to the time of death she did not retain the correct exercise of her mental faculties.

"*Examination.*—The dura mater was almost inseparably connected with the skull, particularly throughout the upper part of the head, and the vessels in the membrane were unusually turgid. The pia mater was slightly inflamed under the os frontis, to about the extent of the palm of the hand, extending on each side of the longitudinal sinus. There were likewise patches of inflammation or congestion in other parts of this membrane.

"The peritoneal coat of the intestines was slightly inflamed. The spleen was enlarged to double its ordinary size. The mucous coat of the stomach was reddened by petechiæ and ecchymosis, especially at its cardiac orifice, but the same appearance extended some way up the œsophagus. The pericardium contained about an ounce of fluid. The annulus venosus of the left side was contracted to a mere fissure, about half an inch in

length, and when dilated was not capable of admitting more than the little finger. The circumference of this aperture was considerably thickened, and the carnae columnæ were contracted. The lining membrane of the auricle was more opaque than usual, clearly evincing that chronic inflammation had existed. The aortic valves were so much thickened as to prevent their having any valvular effect."

Some of the symptoms arising from derangement of the functions of the abdominal viscera, are, now and then, whimsical enough. Physiology must make a great advancement before we shall be able to account for the very different effects which gastric disorder produces on the brain in different individuals. We may observe that the abdominal disease frequently bears but a very small proportion to the cerebral symptoms, if we may judge from the readiness with which it may be removed.

"On the 8th of February, 1808, I was consulted by an individual, who being unable to give any very explicit account of his indisposition, felt a little embarrassment on presenting himself for my advice. He was conscious of some affection of the head, but it was so exceedingly slight that he regarded it as scarcely worthy of notice. The circumstance that most annoyed him was the nightly appearance of apparitions. He distinctly saw the figures of persons at his bed-side, and held conversations with them; and he assured me that if his judgment had not opposed the idea he should have considered his house as haunted.

"On careful inquiry and examination I found that the man's tongue was somewhat coated, that his excretions were of an unhealthy colour, and that his bowels were rather irregular; therefore, I had no doubt that the ghostly visitors were *bilious phantoms*. He was advised to take six grains of the blue pill every other night, with a slightly stimulant bitter combined with a little carbonate of ammonia twice daily. A blister was also applied *inter scapulas*.

"After having taken two doses of the pills he suffered no farther inconvenience from visions.

"In this instance the evidence of hepatic derangement was but slight, not enough to attract the observation of the patient himself; but the analogy of the circumstances to numerous cases of the same kind, in which similar phenomena had occurred from disorder in the liver, led me to attribute the nightly illusion to that cause. The speedy removal of the annoyance, under the simple means employed, confirmed the inference which had been deduced. Though in humble life, the patient was an intelligent man, and neither credulous nor superstitious. He, therefore, wisely inferred that what appeared to be a deviation from the ordinary course of nature must necessarily be owing to some error in his own perceptions. It will be found, I believe, that derangement in the functions of the digestive organs is the most prolific

source of the phenomena of ghosts, usually ascribed to supernatural agency. Their appearance, indeed, is frequently connected, in the most plausible manner, with events. This often happens from the mind's having dwelt in earnest and solicitous expectation of the event; or it might have resulted from accidental coincidence; but much more frequently they are connected, *post factum*, by persons who delight in telling what is marvellous."

Mr. Cook relates a very interesting case of amaurosis, from an affection of the head, consequent on abdominal disease, but it is too long to extract. The following morbid appearances were observed on examining the body:—

"As the body lay in the horizontal position, the resistance of the other viscera having ceased, the liver and spleen did not feel so tumid as they had done during life; still, it will appear in the sequel that they were much enlarged.

"The eyes did not present an unnatural appearance, except that perhaps there was a little more cloudiness in the pupils than usual. The scalp adhered to the cranium with unusual firmness, and so did the dura mater. There was very slight deposition of fluid between the membranes. The pia mater presented a highly vascular appearance. Before removing the brain from its situation we particularly examined the state of the optic nerves. In common with every other part there was an increase of vascularity about their point of union, but from this point anteriorly the nerves appeared of their full size, and were healthy in texture; but posteriorly, towards the thalami, they were excessively softened. If there were any difference in the degree of softness the right was rather the softer. We carefully removed the brain, and proceeded to a cautious dissection. The cerebral substance exhibited rather more numerous points of blood than we find in the healthy brain; but this was more particularly manifest over the optic thalami, and in a greater degree on the right side than on the left. On laying open the right ventricle we found the lining membrane crowded with vessels, and in many places it presented somewhat of a bloodshot appearance. The thalamus was greatly softened in texture, and on its anterior surface the membrane appeared thickened and opaque, as if from a deposition of lymph. There were corresponding morbid changes in the membrane of the left lateral ventricle, and in its thalamus, though not quite in an equal degree. We traced the nerves into the thalami, and found that they continued soft till lost in the softened bodies from which they originate. The left corpus striatum appeared unusually prominent. The ventricles did not contain a larger quantity of serous fluid than is ordinarily found after death.

"With the exception of heightened vascularity of the pia mater, and a slight patch of opacity in the tunica arachnoides, the cerebellum appeared healthy.

"On opening the abdomen the spleen first

presented itself to view. Its surface was uniform, though it contained a few small tubercles. It weighed four pounds and a half. The liver was about double its ordinary bulk, and rather indurated, but its structure had not apparently undergone alteration. The stomach was healthy and empty. The peritoneal coat of the intestines, and this membrane generally, retained its natural aspect; but we could perceive that the mucous coat was diseased in patches. We opened the cæcum and found the mucous coat much eroded, putting on a worm-eaten appearance; and where the ulcerative process had not gone on, the membrane was of a deep red colour.

"The cavity of the abdomen contained about two pints of serous fluid."

We have several cases recited, to show the effects of abdominal disease in producing a derangement of the intellectual and moral faculties. The author speaks also of affections of the muscular system, of the pelvic viscera, of the mouth and nose, of the heart and lungs, of the lymphatics, and of the skin. Through these subjects our limits will not permit us to follow him. From merely looking at the title page of the present work, we were disposed to regard the motives of the author with rather a suspicious eye, but we found ourselves most agreeably disappointed as we proceeded in the perusal of the volume. Mr. Cooke has treated his subject in a masterly and scientific manner; and we know of no work professing to treat of disorders of the digestive functions, and of other functions more immediately allied with them in the circle of relation, which we could more conscientiously recommend to the notice of our readers, than the present.

From the Edinburgh Medical and Surgical Journal.

REPORTS OF THE PROCEEDINGS IN THE HIGH COURT OF JUSTICIARY. By DAVID SYME, Esq. Advocate. Vol. I. Part I. *from November 1826 to May 1827*; and Part II. *from June to December 1827*. Edinburgh, 1827. 8vo. pp. 280. With an Appendix.

As this is unquestionably a work of great interest to the medical man as well as the lawyer, although more especially addressed by the author to the latter, we feel assured that a short notice of it will not be thought inconsistent with the objects of a Medical Journal.

Mr. Syme's Reports include a notice of every case that is tried in the Justiciary Court of this city, with a more detailed account of all discussions on points of law, an abstract of the general evidence whenever it is important, and a full analysis of the medical evidence in every instance in which professional witnesses are examined. This is the first attempt that has been made by a lawyer to rescue from oblivion the medico-legal inquiries annually made throughout Scotland on the occasion of criminal trials. The value of the records which have thus been preserved in the course of a single year, will lead every medical jurist

to lament that this ample field of observation and experience has been so long shut to science, and to hope that the author will be able to proceed with the same spirit and success with which he has begun. A collection of such cases as are contained in the numbers already published, if continued for a few years, will do more to raise medical jurisprudence from the station which it has hitherto occupied, to make medical evidence be respected by courts of law, and medico-legal studies be prosecuted by the medical profession, than all the declamatory complaints of unjust neglect which have for some years past been anxiously poured forth by its votaries.

To the medical jurist, and more particularly to those who have deplored the slow progress which the science of legal medicine has till lately made in this country, the evidence on the trials reported by Mr. Syme must give unfeigned pleasure, as it bears the most ample testimony to the fact, which must likewise have been forced upon their attention from other sources,—namely, that in this part of Britain medical jurisprudence is now at length claiming for itself on all hands the regard to which it is so justly entitled. It will be seen from Mr. Syme's Reports that our law officers are now sensible of the necessity of entrusting medico-legal investigations to competent hands; that even in remote country districts these investigations are now often conducted in an unexceptionable manner; and that, as a necessary consequence, the chief medical questions in the cases are answered with precision, and with much less of that odious discrepancy of opinion which has so long prevailed, to the disgust of courts of law and the disgrace of medicine. It is an event probably unprecedented in the history of the Justiciary Court, that in no less than three cases of poisoning, which have been made the subject of trial in succession, and which have been all of a complicated nature and have required much nicety of investigation, the question of the occurrence of poisoning, and all collateral points, have been decided without any material difference of opinion, and without the ingenuity of counsel having been able to throw any doubt over the accuracy and trust-worthiness of the witnesses.

The trials of which Mr. Syme has given the medical evidence in detail are the following,—four cases of murder by wounds, three cases of murder by poison, one case of child-murder, a case of assault with intent to murder by strangling, and a case of the administration of poison with intent to murder.

1. The first case, No. 33, is that of *William Alexander and Janet Blackwood*, who was accused of having murdered the wife of the former by repeated blows with the fist and with various blunt instruments. It came out in evidence that the deceased was frequently struck on the face and body by both prisoners, and in particular that, while she was sitting on a chair in a state of beastly intoxication, her husband struck her a severe blow with the fist near the left ear, in consequence of which she

fell to the ground, striking the right side of her head against the cross bar of a table,—that, from that time or soon after, she remained in a state of insensibility till two days afterwards, when she died;—and that besides various contusions on the head, body, and limbs, and a fractured rib, there was found, opposite an ecchymosed spot on the right and upper part of the forehead, an effusion of coagulated blood under the dura mater, to the amount of four ounces.

Mr. Newbigging, the principal medical witness, very properly ascribed death to this effusion, and the effusion to the injury received in consequence of the head having come in contact with the cross bar of the table. He admitted, however, that the injury might have been inflicted by a fall from the bed, if the head struck a hard body; and likewise that the effusion might have been caused by excessive drinking without any external injury at all.\* But he concluded by saying he felt himself fully warranted from the circumstances, in ascribing death to the injury. Another witness was also examined, whose name we shall have the charity to suppress, but whose evidence was much too singular to be passed without notice. He stated that he could not think the chief injury was caused by any of the blows described by the other witnesses, the injury was so severe;—that such an injury would require a violent blow with some blunt weapon, or a fall from the height of a story or two;—and that he could not think it was caused by a fall, *because the room was so small*, besides it was admitted *a drunk man fell lighter than a sober man*. To the serious part of this evidence it is enough to oppose a statement by Mr. Newbigging, that “a blow on the head, though not very severe, often produced serious mischief within.” The prisoner Blackwood was acquitted, and Alexander upon Mr. Newbigging's evidence, that the fatal injury was caused by the fall, not directly by the blow, was found guilty of culpable homicide, and sentenced to fourteen years' transportation. We confess we cannot exactly enter into the nice distinction which in a case of this kind the law draws between the direct injury caused by a blow, and the indirect injury caused by a fall, which is received in consequence of a blow.

2. The next trial we shall notice, No. 38, is that of *Allan Grant, James Stewart, Mary Muirhead, and Isabella Kerr*, for the murder

\* We heard at the time some surgeons doubt the accuracy of the witness in this statement, on the ground that they had never personally witnessed extravasation in any dissections of persons killed by excessive drinking. Mr. Newbigging nevertheless was amply borne out in his opinion by the records of medical jurisprudence: In *Bernt's Contributions* no fewer than four examples of extravasation are related, (*Beitrage zur gerichtlichen Arzneikunde*, ii. 59. iv. 38.) So rash is it to rely, as many would have witnesses do, on pure personal experience.

of Mr. Dow, and for robbery and theft, a mysterious case, which made a great deal of noise at the time. It need not detain us long, however, as the medical evidence threw little light on the circumstances of the alleged crimes. The theft was fully proved; but there was a complete want of moral evidence in support of the charge of murder. What little was procured, indeed, favoured the presumption of accidental death; for the deceased was much intoxicated at the time of his death; his dead body was found at the bottom of a dark stair, and the cause of death was discovered by medical inspection to have been fracture of the fourth cervical vertebra. Mr. Newbigging, the chief medical witness on this trial also, deposed, that the appearances of contusion found by him on the head were such as might have been produced by a fall; that the fracture was a very likely accident to be produced by such a fall; and that it could not be produced in any other way, except by a large heavy body falling on the head, or "some very heavy instrument used with some extraordinary degree of force." The Lord Advocate, considering the evidence, especially of the medical witness, departed from the charge of murder. The prisoners were found guilty of theft and transported for various periods, according to their share in the theft.

3. The third trial to be noticed, No. 44, is a rare one in respect to several of its features. The most important question which was involved in it related to the responsibility of the prisoner for the death of the deceased, death having been caused by tetanus, the consequence of unimportant wounds. *Alexander Mackenzie* was indicted for robbing, assaulting and murdering Alexander Clerk; and the murder was charged to have been committed by numerous blows and wounds inflicted on various parts of the body, in consequence of which he languished for ten days and died. It was proved, that, although the deceased was stunned and confused by the maltreating he experienced, he nevertheless had not received any severe injury. On the third day after, however, he was attacked with incipient symptoms of locked-jaw, which increased so slowly that on the fifth, sixth, and seventh days he was able to go out, and on the seventh day particularly was out most of the day at his work. From that time the symptoms increased more rapidly, and he died on the tenth of true tetanus. On dissection the only appearance of note observed was inflammation of the throat and of the trachea and bronchi.

The medical witnesses examined were Mr. Newbigging, Mr. Liston, and Mr. Mackenzie, besides a medical student and another medical gentleman who deposed to certain particulars of the deceased's illness. In reply to a question which was a good deal pressed on the witnesses, they all agreed that the tetanus was not caused by exposure to cold when the man was out on the fifth, sixth, and seventh days, but that it existed previously. It was likewise agreed on all hands, that the

wounds must have been the cause of it, even though they were trivial, because it had been known in other cases to arise from wounds even more trifling. In relation to the *degree* in which the wound and tetanus stood to one another as cause and effect, it was farther agreed, that tetanus lay in the natural progress of events, just like mortification, although neither so necessary nor so common a result. In so much, in fact, was it not a necessary or common result of such wounds as the deceased received, that it never would have been looked forward to in the prognosis by any surgeon.

These opinions seem all perfectly correct. Perhaps the *accidental* nature of supervening tetanus should have been placed in a stronger point of view, namely, by the fact, that in forming a prognosis not only in a case like this, but likewise in any case whatever of external injury, it is next to impossible to regard tetanus otherwise than as a very rare and unlikely consequence.

The prisoner was found guilty of culpable homicide, and sentenced to fourteen years transportation, the Court holding, that *the wounds not being of an aggravated description, and there not being any proof of previous deadly malice or intention to kill*, the case could not be accounted one of murder. It is of importance that the medical witness and jurist should be distinctly aware of the effect of his testimony on the law as well as the facts of a case. We may remark in explanation, therefore, that in this instance the previous intent, which is indispensable to constitute murder, was not proved to have existed, either by the evidence of the prisoner's words and actions before, during, or after the assault, or by evidence derived from the wounds being in themselves severe or dangerous. Had this intent been proved either in the one way or the other, the case would have been one of murder; for it appears to have been the opinion of the court that tetanus, though an effect, neither necessary, nor common, nor such as in like circumstances may be anticipated, was a consequence not sufficiently *accidental* to have relieved the prisoner from responsibility.

4. The fourth case, that of *Campbell and Helm* for culpable homicide, No. 88, possesses no particular interest. The deceased was killed by sustaining a fracture of the lower part of the occiput, attended with effusion, and the fracture was received in consequence of a fall from a knock-down blow among stones.

5, 6, 7. The three next trials, No. 34, and App. 1 and 2, are by far the most interesting in the work. They are the trial of *Mrs. Smith* for the murder by poison of Margaret Warden, —of *Margaret Wishart*\* for poisoning her sister, —and of *John Lovie*, a farmer, for poisoning his servant Margaret Mackisser. Of the two first nothing farther need be said here, as they are already reported in detail in this Journal by Dr. Christison. The third was exceed-

\* Vide Journal of Foreign Medicine, Vol. I. page 642.

ingly similar to the rest, being a charge of murder committed on a female who was believed to be with child to the prisoner. The case was tried at Aberdeen last autumn circuit. The chief medical witnesses for the crown were Messrs. Jameson and Coutts, surgeons at Fraserburgh, Dr. Blaikie, Professor of Surgery, Dr. Knight, Professor of Natural Philosophy, and Dr. Forbes, Professor of Chemistry at Aberdeen, and Dr. Christison of Edinburgh. The proof of death by poison was complete. After some ineffectual trials made at the time of the inspection, the contents of the stomach and the stomach itself were divided into two portions, of which one was entrusted to the Aberdeen professors, and the other sent to Dr. Christison; and both succeeded in detecting arsenic unequivocally. In the face of very strong moral evidence the prisoner escaped with a verdict of *not proven*, probably because the jury did not consider the crime satisfactorily attached to the prisoner, while there was not some direct evidence of the administration of the poison. There is only one circumstance in the case which calls for special notice here. On the trial of Mrs. Smith it was stated by Dr. Christison that he found *bright yellow* particles on the surface and imbedded in ulcers of the stomach; and he imputed these to the conversion of part of the oxide of arsenic into the sulphuret by sulphuretted hydrogen disengaged in the stomach. Dr. Fyfe stated that he did not conceive this to be a probable explanation; and Mr. Jeffrey, the prisoner's counsel, elicited from Dr. Christison on his cross examination, that sulphuretted hydrogen had never been sought for in the human stomach after death.\* The case of Margaret Mackisser sets this question at rest; for Dr. Christison again found shining yellow particles on the surface, and, instructed by experience, did not leave the existence of sulphuretted hydrogen to be presumed from analogy, but actually detected it in the bottle, in which the stomach was sent, by immersing in the bottle a slip of paper dipped in nitrate of lead. This appears in fact to be by no means an uncommon appearance in the stomach. It was remarked in another of Dr. Christison's cases (Edin. Med. Chir. Trans. ii. 284.) We have ascertained by experiment that the solid oxide is converted into sulphuret by sulphuretted hydrogen if moisture is present. This might have been presumed; but being doubted, it is right to determine it expressly.

8. The trial for child-murder, No. 95, was followed by conviction, a rare result now-a-days; but the circumstances were different from those of the ordinary instances of child-murder, the child being seventeen months old, and the accused person being the father.—*James Glen* was indicted for the murder of his natural child by throwing it in the Forth and Clyde Canal, or by first strangling and then drowning it. In such cases the medical evidence is often unsatisfactory, because the signs

of strangulation as well as of drowning are not unfrequently obscure, particularly in children. The present case was of that nature, for it is not easy to see how the medical witness was borne out in the strong opinion delivered in the report and on the trial. The child was carried away from its mother by the prisoner on the 1st May, was found dead in the canal on the 9th, and was inspected on the 10th. The identification of the body was established in part, and very satisfactorily by the medical evidence; for it was proved, on the one hand, that though the body was in some places considerably decayed, the face retained its natural appearance; and on the other hand, that certain marks produced by burns, which were referred to by the mother and others, existed precisely where they were described to be. The inspectors further found, "on the fore part of the neck, over the windpipe, a softish furrow, with a hardened ridge both above and below; an appearance such as would be produced by violence applied to effect strangulation;" and after explaining that in some cases of death by strangling or drowning no characteristic appearances are detected in the body, they conclude that, although they cannot speak positively, they think it probable that forcible means were employed to strangle the child. On his examination the chief medical witness "leant strongly to the opinion that strangulation was the cause of the child's death." Other particulars observed during the inspection, and not specified in the report, may have supported this opinion. But the report and oral evidence of the *medical* witnesses certainly does not state any circumstance whatever by which death by strangling could be distinguished from strangling effected intentionally or accidentally *after death*. Altogether the report does not appear to us sufficiently detailed or explicit. In all complicated and equivocal cases, such as this must be admitted to have been, it is of the utmost consequence that the inspector specify all the particulars of his examination, since it is well known to medical jurists that the issue of investigations of the like kind often turns upon facts which at first appear to the inspector of insignificant importance. The prisoner, we have remarked, was condemned, and the sentence was put in execution. The moral evidence was exceedingly strong, so that even a presumption of violent death from the medical circumstances was sufficient for condemnation.

9. The next case,—a trial for assault with intent to murder by hanging or strangling,—deserves to be noticed, not so much on account of the medical evidence, which was exceedingly short and simple, as for the singular circumstances under which the crime was alleged to have been committed. The prisoner, *Marion Brown*, an old woman of 69, was accused of having attempted to hang her husband, who was some years older. This variety of homicide is stated in works of medical jurisprudence to be one which can be successfully attempted only by a person very superior in strength to his victim, and is in fact very rarely

\* See report of Mrs. Smith's Trial, *passim*. Vol. xxvii.

resorted to except by several persons conjunctly. In the present instance, the prisoner contrived to twist a small rope three times round the neck of her husband, while he was asleep, and to tie him up to a beam in a room, in such a manner, that, when the neighbours entered, the man was found lying on the floor with the head raised about a foot from it. He was quite insensible, the hands were dropped down powerless by his side, his face was livid, and it was some time before he could be roused; so that little doubt existed that he would have perished had he remained as he was two or three minutes longer. He deposed that he went to bed quite sober, having drunk no more than his share, with five other people, of three gills of whisky; and that *he was not aware of any thing which passed during the attempt to hang him*, or after it, till the surgeon succeeded in restoring him to his senses. The possibility of strangling a person during sleep, without his ever becoming conscious of it, and consequently without his being able to struggle, has been questioned by medical jurists. The case now quoted will show that such a thing is quite possible; and that, contrary to what is generally believed, a person may strangle one much stronger than himself, without any assistance.

The prisoner was proved not to have entertained decided previous malice against her husband, and to have been at the time of the act in a state of intoxication. The prosecutor, therefore, departed from that part of the charge which accused her of intent to murder, and she was sentenced to eighteen months of Bridewell.

10. The case of *Mary Anne Alcorn*, the last we shall mention, is novel, as being one of the few, if indeed not the only instance in modern times, of the crime of poisoning having been substantiated without the discovery of the poison, nay, without any distinct evidence what particular poison had been used. The prisoner, a servant girl, sixteen years of age, was accused of having mixed tartar emetic or some other poison with the meat and the gravy of which her master and mistress partook at dinner,—in consequence of which they were affected with violent symptoms of irritant poisoning, even to the endangering of their lives. A considerable part of the meat was left, and was examined chemically by Dr. Turner and Dr. Christison, without any poison being detected. But it is necessary to add, what has not been mentioned in Mr. Syme's report, that, from the particular way in which the suspected substance was mixed with the meat, it must have happened that all, or nearly all of the poison was taken by the family at dinner. The whole particulars of the case were very important in a medico-legal point of view; but they are cursorily mentioned in Mr. Syme's work, and even upon the trial they were not gone into with that minuteness which is necessary for enabling the reader to see clearly the grounds of the opinion delivered by the medical witnesses. Our limits will not permit us to supply the deficiency. We can only mention that one of the witnesses, Mr. Thomson of Por-

tobello, who attended the family in their illness, deposed,—that he had no doubt they suffered from the effects of poison,—that he could not say what the poison was,—but that he suspected it was some preparation of lead. The other witness, Dr. Christison, deposed,—that, that from the pure medical circumstances, poisoning was highly probable, if not certain,—that his reasons for this opinion were the nature of the symptoms, the time at which they began after a suspected meal, the fact of *three* persons having been similarly and simultaneously affected (for the prisoner also took a little of the meat and was slightly ill,) and lastly, the fact, that all those who were taken ill suffered in proportion to the quantity they had taken of the particular dish which was suspected. He was inclined to suspect tartar emetic to have been the poison administered; but spoke doubtingly on that point.

The prosecutor, in consideration of the moral evidence, gave up the charge of intent to murder, and the prisoner, having been convicted of administering a deleterious substance with intent to do grievous bodily harm, was sentenced to twelve months confinement and hard labour in Bridewell.

The account we have given of Mr. Syme's labours will convey some idea of the addition which is likely to be made to our medico-legal information, by the continuance of them. We have just one word to say with regard to the accuracy of the reports. Mr. Syme gives only an analysis of the medical evidence, although a pretty full one. That this has been done with ability we can testify from ample means of knowledge. Still it is an arduous undertaking for an unprofessional man to give an abstract of medical opinions and statements. The alteration, omission, or interpolation of a single word, may change entirely the meaning of a passage, although the extent of the change is not apparent to the reporter. Some errors were therefore unavoidable; and, accordingly, we could point out several passages where, if recollection serves us correctly, errors of this kind have crept into Mr. Syme's Reports. They are inconsiderable, it is true, and only upon collateral points, not very directly influencing the leading opinions delivered; but still they are such as would lead a medical jurist to conceive the witness spoke loosely or in ignorance. On the whole, however, as we have already said, the abstract of the medical evidence is remarkably accurate; and greater accuracy could not well be ensured, unless the evidence was reported in the very words of the witness.

We must not forget to mention, that the author has in every case published the written medical reports verbatim, and several of them are very important. A natural consequence of this practice will be, that medical inspectors, for fear of having their carelessness exposed, will pay more attention to the framing of the reports, which are often miserably defective. If drawn up as they ought to be, they should constitute by far the most valuable part of the work to the student of medical jurisprudence.

## Medical and Philosophical Intelligence.

*Detachment of the Placenta by the Injection of Cold Water.*—Since the memoir of Dr. Duparcque, contained in the present number, was sent to the press, we have noticed in the *Journal Général de Médecine*, &c. a paper by Dr. Legras on the same subject, read before the *Société de Médecine*, and printed by order of that body. We have not room for a detailed analysis of this article, which is followed by a copious report by MM. Duchateau, Chailly, and Gendrin, and shall therefore lay before our readers a brief summary of its contents, referring them for further information to the journal above mentioned.

Dr. Legras was induced to make trial of the plan recommended by Professor Mojon, from having observed that the ligature of the placental portion of the chord, generally accelerated delivery, an effect which he attributed to the congestion thereby produced in the vessels of the placenta. It was desirable, however, to determine in the first place the safety of the plan, by measuring the degree of cold produced by the injection; and from some experiments made by him, he ascertained that water at 59° did not reduce the temperature of the placenta below 70°, a temperature which he supposed could not be productive of injurious consequences. In proceeding to the injection, the umbilical vein should be preferred to the artery, inasmuch as its greater diameter allows the injection to arrive more rapidly at the placenta; the chord should be divided as near the vulva as possible, and the temperature of the water employed should not exceed 59°; from six to eight or nine ounces will be sufficient. Its first effect is a slight degree of pain, which is speedily followed by uterine contraction. Several cases are related. In one of them, the uterus fell into a state of complete inertness after a long and tedious delivery of twins; the placenta remained adherent, and after having ineffectually attempted to excite uterine action in the ordinary methods, an injection of cold water was thrown up each chord; contraction of the uterus immediately supervened, and the placenta were expelled in consequence. The utility of the plan in question, is still more apparent, when the placenta is retained in cases of premature delivery; in such instances the adhesion of this viscus is generally more considerable, the chord less able to support the efforts made for its extraction, and the introduction of the hand, both difficult and painful. In a case of the kind recorded by Dr. Legras, the patient was delivered of a dead putrified fœtus, in the fifth month of utero-gestation; an injection composed of equal parts of vinegar and water was thrown up, and was followed by the immediate expulsion of the placenta, which was soft and easily ruptured.

Dr. Legras terminates his memoir with the following conclusions.

1. Pure water injected into the vessels of

the placenta while adherent to the uterus, cannot be productive of injurious consequences.

2. In cases where inertness of the uterus is the result of distention, arising from a plurality of children, or the great quantity of liquor amnii, the injection of cold water has always succeeded in occasioning the expulsion of the placenta.

3. In cases where inertness is complicated with uterine hemorrhage, arising from the partial detachment of the placenta, water combined with about a third part of vinegar, has invariably excited the contraction of the uterus, arrested the hemorrhage, and facilitated the expulsion of the placenta.

*Suppression of the Urinary Secretion.*—The patient, æt. 12, during some general indisposition, which lasted five days, and yielded ultimately to the administration of an emetic, had not passed a drop of urine; a physician was then consulted, and prescribed a diuretic mixture, which excited an abundant discharge. The suppression, however, again recurred, although the child continued apparently in good health, and drank his ordinary quantity of liquids. For the space of nineteen days, the warm bath and other appropriate remedies were employed, without being productive of the slightest advantage, and Dr. Ramm was consulted 20th February, 1827. The child had not urinated for more than seven weeks; every two or three days, he had an alvine evacuation of a very solid consistence, and had not been observed to sweat, even after leaving the bath; he drank copiously, his appetite was good, sleep tolerable, and he complained of no uneasiness in the kidneys or bladder. There was no room for deception in the case, as the child was carefully watched by his parents, who could have had no interest in thus imposing upon the physician. A catheter was introduced on the 22d, but the bladder was found completely empty. Thirty drops of a mixture composed of two drachms of castor oil, six of Venice turpentine, and one ounce of copaiba, were directed to be taken three times a day, in an emulsion of sweet almonds; frictions on the loins with the oil of turpentine, twice a day; and a regimen consisting exclusively of asparagus, horse-radish, celery, radishes and beer. On the morning of the 24th, he evacuated more than a *litre* of limpid urine, slightly mucilaginous; an equal quantity was passed on the 25th and 26th, and a still greater amount on the following days. The dose of the mixture was diminished, and on the 8th of March, it was entirely discontinued, the urinary secretion being completely re-established.

A case somewhat analogous to the above, is detailed in the *Nouvelle Bibliothèque Médicale* for 1815, and another, in a work by Dr. Koenig, on the Diseases of the Kidneys, published at Leipsic in 1826.—*Nouv. Biblioth. Med. from Hufeland's Journal.*

Since translating the above, we observe in the *Bull. des Sciences Médicales*, the account of a case by Professor Berres of Lemberg, in which the secretion was suppressed for more than six months. The patient, a boy, at. 13, had on the 22d October, 1822, a violent paroxysm of fever, which was followed, two days after, by a miliary eruption. Exposed to cold when convalescent from this attack, he had a second paroxysm, which induced a state of great debility; obstinate constipation supervened, and resisted purgatives and enemata during twelve days. On the 20th November, he complained of insupportable pain in the rectum, which was especially severe, when he had an alvine discharge. In the month of January, at the same time the constipation continued, the urine decreased in quantity, and was suppressed during 15 days; this suppression was twice repeated till March, when it again occurred, to continue more than six months.

In July, 1823, the patient was in a state of great emaciation; he was still troubled with constipation, and had, moreover, a periodical pain in the loins, so violent that he lost all consciousness, and was agitated by convulsive movements during the paroxysm. The abdomen presented a rounded form, and appeared distended when the patient was in a standing position; in a state of supination, there was an elastic, but not fluctuating prominence, on each side, descending from the lumbar region towards the pelvis, and separated by an interval from its fellow on the opposite side. The bladder appeared to be empty; a slight percussion upon the abdomen produced a sonorous noise, and pressure upon this part was productive of acute pain. The head was unaffected, pupils dilated, tongue clean, appetite impaired, intense thirst, the sense of taste lost, respiration short and accelerated, skin dry and exhaling no urinous odour; pulse frequent, with some degree of hardness, sleep disturbed, &c. Upon exploring the rectum, some elastic tumours were discovered upon its sides, but their nature could not be determined. The urethra was so much contracted, that a bougie larger than No. 2 or 3, could not be introduced; the genital organs were in general very little developed.

During six weeks, Professor Berres employed a great variety of therapeutic measures, but without more success than several other physicians who had previously treated the case. The greater part of the remedies only served to aggravate the pain in the loins. The patient was ultimately carried by his father to Pesth, where he was to undergo an examination by other physicians. At the moment they were about to introduce the instruments, the patient alarmed requested to be permitted to rest for a time, inasmuch as he experienced a strong desire to urinate; the urine began to flow abundantly at the same moment, the prominence of the abdomen subsided, and the disease was cured. Two years afterwards, he had had no relapse.

*able Articulations.* By Dr. GIDHELLA.—This gentleman was consulted by a young man who had a partial ankylosis of the temporo-maxillary articulation, accompanied with considerable induration and tumefaction in the surrounding cellular membrane; emollient and resolvent applications had been tried unsuccessfully during nine months. Several incisions were made by Dr. G. upon the tumefied part, which corresponded with the parotid gland; and upon the masseter muscle, both longitudinally and transversely with respect to its fibres. A considerable hemorrhage was the consequence, the swelling subsided, and by means of a bistoury introduced into the mouth, Dr. G. succeeded in dividing the resisting parts and bands which remained, and thereby enabled the patient to open his mouth, which had been firmly closed for so long a time, during which liquid aliment had been introduced with difficulty through an accidental opening occasioned by the loss of two incisor teeth.

*Case 2.*—A robust countryman had been for eight months affected with an ankylosis of the articulations of the two upper cervical vertebræ, in consequence of violent exertion in carrying a beam. Symptoms of acute encephalitis made their appearance in the first place, and yielded only to copious venesection, seven times repeated. The rotatory motion of the cervical vertebræ above mentioned, was entirely lost, and there was, moreover, a general tension of the muscles attached to the occiput, with remarkable tumefaction of the surrounding parts, and a severe tetanic rigidity of the sterno-mastoid muscles; he was unable to turn his head laterally, without moving his whole body in the same direction. A variety of means having been employed as unsuccessfully as in the preceding case, Dr. G. made several deep incisions upon the posterior part of the neck, at the point corresponding to the junction of the two vertebræ; others were directed toward the occipital region; blood flowed abundantly, and the pericranium was observed to be considerably thickened. The hemorrhage continued during half an hour, and in a little while, as the induration and swelling subsided, the patient was able to rotate his head pretty freely. Six days afterwards, the actual cautery was applied opposite the articulation of the two upper vertebræ, which gave rise to an abundant suppuration, and gradually removed the pain in the occipital region, which had continued for some time, and was supposed by Dr. G. to be dependent upon chronic inflammation of the cerebral envelopes. The suppuration was maintained a considerable time, but although the rigidity was greatly lessened, it was not entirely removed.

*Case 3.*—A blacksmith had an old ankylosis of the elbow joint, arising from a wound of this part; the forearm was bent upon the arm, and was affected with convulsive movements. The joint was one-third larger than in its natural state. Blisters, issues, &c. having altogether failed, more than a hundred incisions were successively made with a bistoury principally upon the sides of the articulation; a gra-

duated extension was made by means of a splint extending from the axilla to the hand, and at the expiration of three months the cure was completed, the patient being able to extend his arm and move it in every direction. Two months afterwards, he again consulted Dr. G. complaining of pain in the elbow, and a kind of electric shock at this part, which during the night occasioned an emission of semen. After the failure of other measures, the actual cautery, applied along the course of the ulnar nerve, which was larger and more sensible than usual, dissipated every symptom.

Other examples of the disease are given as it appeared in the articulations of the shoulder, wrist, knee, and ankle, but we are restricted by our limits to the conclusions drawn by the author.

1. The stiffness of joints caused by any alteration of the surrounding parts, is the ordinary result of inflammation, by which they become engorged, indurated and thickened.

2. In such cases, topical applications, and other remedies usually employed, are of little avail, and although they may be productive of a slight improvement, are incapable of restoring the motion of the joint. The most effectual method, is the division of the morbidly thickened and contracted ligamentous bands by means of incisions; while internal and external remedies may be used conjointly, according to circumstances.

Dr. Gidhella observes, that incisions were not productive of equal advantage in very old subjects, in whom this disease was of long continuance, as in the cases above mentioned; they removed, however, the tensive and spasmodic pain, and prevented any further alteration of the indurated parts. Where the ankylosis depends upon a consolidation of the cartilages, they are, of course, ineffectual.—*Giornale di Chirurgia Practica.*

*Observation sur un ascité suivie d'hydrocèle, et terminée l'une et l'autre par la chute des testicules;* par M. Hutin, Chirurgien en chef de l'hôpital de Joinville: Monin, æt. 12, a drummer in the French service, was attacked with fever, for which he was treated about six weeks without receiving any benefit, when he was sent to the hospital of Blamont, where fifteen days after his arrival he was seen by M. Hutin, who had just been appointed to that institution. Upon attentive examination, M. Hutin ascertained the existence of a large quantity of water in the abdomen. The following were the symptoms he then presented: laborious respiration, pallid countenance, irregular and almost constant palpitation, frequent syncope, profuse sweats, febrile exacerbations followed by thirst, which was greater towards evening, infrequent excretion of urine, and lastly, general malaise. Diuretic beverages and pills, composed of squills, digitalis and asafetida, were prescribed, and continued some time without benefit. One morning, while under this treatment, M. Hutin observed a considerable tumefaction of the penis and scrotum, attended with a subsidence of the

abdomen, and some degree of melioration of the general symptoms. The tumefaction was ascertained to be owing not to simple œdema, but to accumulation of serum in the tunica vaginalis. Dry lint and a suspensory bandage were directed, and M. Hutin awaited the proper moment for the introduction of the trocar. Sometime afterwards he was surprised to find the posterior and upper part of the scrotum, near the perineum, in a gangrenous condition. An antiseptic cataplasm was applied, and renewed three or four times daily. A large slough separated on the fifth day, and together with the testicles in a state of complete disorganization, was removed with the poultice which had been applied the preceding evening. No hemorrhage followed, the penis preserved its integrity, the hydrocele and ascites disappeared, the ulcer left by the slough readily healed, and by assiduous attention during three months, the health of the patient was completely re-established.—*Journal Général de Médecine, &c.*

*Apoplexy of the Medulla Spinalis.*—A man aged sixty-eight years, of a robust constitution, middle stature, and sanguineous temperament, died suddenly in the Bicêtre. The following account is given of his previous condition. His body was slightly curved forwards, and his legs were weak and stiff, so that he was obliged to make use of a cane to prevent his falling. He could walk from his residence to Paris, but in doing so, considerable time was required, and notwithstanding his cane, he would have frequently been in imminent danger of falling, had it not been for the accidental support afforded by a tree or some other object. The superior extremities had also become much enfeebled. To a mind naturally confined, he added a difficulty and extraordinary slowness of pronunciation, which had almost excluded him from society. He lived alone, did not complain, and enjoyed a good appetite. The day preceding his death, he walked about the hospital as usual, retired to rest at his accustomed hour, and at four in the morning was found dead in his bed. It appeared that he had been several times attacked with apoplexy, of which the weakness of his extremities was the consequence.

*Dissection twenty-eight hours after death.*—Flesh firm; forearm and fingers flexed and strongly contracted; legs stiff and extended. On dividing the integuments of the head, a considerable quantity of black fluid blood escaped, as did also upon separating the cranium from the dura mater; this membrane was thin and transparent, all the sinuses were engorged with blood, the pia mater was injected, the substance of the brain was sufficiently firm, except at the superior and external portion of the left hemisphere, where it presented a slight superficial softening. In each of the corpora striata was a cavity capable of containing an almond, lined by a celulo-vascular membrane, infiltrated by a yellowish serum; the superior portion of each of these cavities appeared to be formed by the

arachnoid membrane lining the ventricles, in a thickened condition. Nothing remarkable was observed in the cerebellum; the tuber annulare was slightly softened. A large quantity of black blood escaped upon laying open the spinal canal, and the meningeal veins were distended with that fluid; the arachnoid cavity contained much serum, upon which floated small drops of a fatty matter, and some globules of air. In the lumbar region, the arachnoid presented a number of cartilaginous lamina, fringed at their extremities, some of which exhibited in their centre a small projecting osseous point, opaque, and composed of radiating fibres. The veins of the medulla were distended, and especially at its lower part, were greatly dilated and flexuous; the partial septum which penetrates into the anterior and posterior fissures was deeply injected; the substance of the medulla preserved its natural consistence; it was furrowed in every direction, by numerous minute vessels. Between the origin of the fifth and sixth pair of cervical nerves, there was an extravasation of blood, about the size of a pea, which had destroyed the cineritious commissure; another, much more considerable, was found on a level with the fourth dorsal pair, which in this place, had almost entirely destroyed both the white and cineritious substance of the medulla; the coagulum equalled in volume a large hazel nut, and was slightly elongated in the direction of the spine; in the vicinity of these extravasations, the medulla was slightly softened and infiltrated with blood. The lungs were engorged with blood, and large coagula were found in the cavities of the heart; the intestinal canal was sound.—*Nouv. Biblioth. Médicale.*

*Preternatural Enlargement of the Processus Dentatus.*—The subject of this case, æt. 24, was found dead in the road without any mark of violence being discoverable on his body. He was emaciated, in height did not exceed that of a child twelve years of age, and presented no indications of puberty, either on his face or genital organs. The bones of the cranium were very thick relatively to the development of the other parts; the meninges were gorged with blood; the substance of the brain was firm, and also contained much blood; about half an ounce of serum was found in each lateral ventricle. The pineal gland was about the size of a pea, and contained some sandy particles. The tooth-like process of the axis was of an unusual length, projecting into the interior of the cranium, two lines beyond the level of the occipital foramen; the head of this apophysis was more than ordinarily developed, and its summit was fixed to the border of the occipital foramen, by means of a ligament, extending horizontally forwards. This abnormal disposition, reduced the occipital foramen to about half its natural size, in consequence of which, it is said, the medulla oblongata was compressed and restrained in its development. The right lung was œdematous, and the left adherent throughout to the

parietes of the thorax; the heart was proportionally very large, firm, and surrounded with fat; the liver and spleen adhered to the abdominal parietes. The stomach was distended with food, a considerable portion of which had not yet undergone the process of chymification; the internal membrane of this viscus was very red. For several years, the patient had been subject to anxiety and palpitations of the heart, which were heard at a considerable distance. His death was evidently the result of apoplexy. By professor Leuhosseck, the protracted childhood and other symptoms, are attributed to the enlargement of the tooth-like process, though no evidence exists that during life, any compression was occasioned thereby; moreover, the occipital foramen will admit of considerable diminution ere compression of the medulla is induced. May not this extraordinary development of the tooth-like process, have been the consequence rather than the cause of disease, the result of a rachitic affection?—*Bull. des Sciences Méd.*

*Remarkable cerebral disease successively attacking four children of the same parents;* by Dr. Stingel. A woman residing in the vicinity of Roeraas in Norway, was delivered in 1801, of a well formed male child, which continued in good health till his seventh year, when his vision began to fail, and his intellectual faculties and sensibility to be equally impaired; in the course of his ninth year, vision was almost entirely lost, and he had become insensible to external impressions: of all the senses, that of hearing was longest preserved. Between the ninth and fourteenth year, epileptic paroxysms supervened, and gradually became more and more violent. At the age of fifteen he was subject to convulsions, and occasionally put forth horrible cries. This state of things continued till his death, which took place in his twenty-first year. His sister, some years younger than himself, was attacked at the same age, in a manner precisely similar. The disease followed the same course, and she died, without having menstruated, in the twentieth year of her age. Some years after the birth of the latter, another male child was born, which at the age of seven years was affected in a similar manner. At the time the present account was drawn up, he had attained his sixteenth year, and the disease had followed a course precisely analogous to that which had occasioned the death of the other two. A fourth child, a female, was removed at the age of four years, to another situation, on the supposition that the metallic mines in the neighbourhood of Roeraas, might have some influence on the production of this terrible disease. She continued very well till her seventh year, when the same symptoms manifested themselves, and there is much reason to fear, will terminate alike unfortunately with the preceding cases.—*Bull. Univ.*

*Intestino-vesical Fistula.*—Pierre Desnel was wounded, during the war of La Vendée, by a ball, which, penetrating about the middle

of the right iliac fossa, passed out at the left groin. A considerable hemorrhage occurred from the wound. Placed at first in the hospital of Nantes, he was afterwards transferred to Painbœuf. Pus was discharged in great abundance from the wound in the iliac region, and that in the groin, gave exit to a large quantity of fecal matter. At the expiration of a year, the former healed, the latter, some months later; and the patient, supposing himself completely cured, joined the army of Spain in 1807. The escape of wind, however, from the penis after urination, evinced a communication between the bladder and intestines. In 1813 he began to void feces by the same route, when affected with diarrhœa; and the following year, a piece of bone, four lines in length and two in breadth, was discharged through the urethra. From that time he has continued to evacuate wind by the penis, together with feces when his bowels were more open than ordinary; and when the urine was long retained, it escaped through the rectum. Symptoms of stone ultimately made their appearance, and when examined at St. Louis in 1826, by MM. Richerand and Cloquet, a calculus was discovered; they were unable to detect any communication between the bladder and the rectum. The patient declined to submit to an operation.

M. Monod, the reporter of the preceding case, differs from the gentlemen above mentioned, who suppose the rectum was the portion of intestine injured; this, he observes, from the direction of the ball, is almost impossible, while the discharge of feces from the wound in the groin alone, would indicate that the injured intestine must have been situated to the front and left side of the bladder, a position which might readily be assumed by the iliac portion of the colon, and especially the small intestine; the latter, therefore, he is inclined to suppose to have been the part traversed by the ball. That the bladder should have been injured, it is not difficult to conceive, but the passage of a splinter of bone, twenty years after the accident, is not quite so susceptible of explanation. No incrustation was observed upon it, and he is rather inclined to suppose that it had worked its way from the cellular tissue of the pelvis, into the bladder, than that it should have remained twenty years in the latter, without inducing the formation of a calculus.—*Nouvelle Biblioth. Med.*

*Mercurial Pediluvia in Syphilis.*—In the *Osservatore Medico di Napoli*, for January 1828, Dr. Verducci proposes to substitute pediluvia of corrosive sublimate, for entire baths containing this preparation, as being at the same time more convenient and economical. About a fifth or sixth part of the following solution is added to a quantity of warm water sufficient to reach above the ankles. R. Hydragryri oxymuriat ℥ij., alcohol ℥i., aquæ distillat. ℔i.

The temperature should be maintained at about 112° by means of heated water, and the pediluvia should be continued half an hour

each time. Dr. Verducci has in this manner effected the cure of a considerable number of chronic ophthalmias, ulcers in the throat, and primary buboes. Among other cases is the following.

In the course of the preceding April, a man æt. 50, consulted Dr. V. for a tumour on the sternal extremity of the fourth true rib, which was attributed to an old syphilitic affection, and mercurial frictions were employed without benefit. A drastic cathartic, given at this period, occasioned a chronic inflammation of the intestines; a pustular eruption broke out upon the body, the patient was troubled with pains in the bones, and a neuralgic affection of the left leg, obliged him to maintain a recumbent posture. In the month of August, when the use of the mercurial pediluvia was commenced, the limb was much emaciated. At the expiration of ten days of this treatment, the pustules were almost entirely dissipated, the tumour on the rib has disappeared, and he no longer complained of pain in the leg. On the twelfth day he rose from his bed, and on the thirteenth was entirely cured.

*Traumatic Erysipelas cured by the application of the Actual Cautery.*—A soldier had received several contused wounds on his head, which denuded the bones and periosteum in several places. Symptoms of concussion first made their appearance, and subsequently those of cerebral congestion and incipient compression, for which active measures were directed; notwithstanding which erysipelas supervened, and extended over the posterior part of the neck, shoulders, and superior part of the back. M. Larrey directed the application of an incandescent iron, which speedily arrested its progress,—minute brownish spots were the only traces left by the cautery.

This plan, which has been adopted for several years, in all cases of this nature admitted into the Hôpital de la Garde Royale, has been uniformly successful. According to M. Larrey, the form of the instrument employed is a matter of little consequence; it is only necessary that the surface be narrow, and that the affected part be touched with it in a great number of points. The cautery doubtless acts by concentrating the inflammation in the parts to which it is applied.—*Archives Générales de Médecine, &c.*

*Inflammation by Contiguity; by Drs. Graves and Stokes.*—There are many reasons for believing that inflammation is sometimes propagated by the mere contact of a sound with an inflamed part. Drs. Graves and Stokes furnish us with the following curious fact. In the dissection of a fatal case of enteritis, it was observed that the omentum, which lay extensively over the intestines, was healthy, except where it was in contact with the inflamed portions of the latter. These portions (of intestine) were circumscribed and limited in extent—some being highly vacular and red—others gangrenous—and one perforated. The inflamed portions of omentum were very

vascular and red, about the size of a dollar, and lay exactly over the inflamed portions of intestine. Similar phenomena have been often observed in other parts of the body, but no explanation has been attempted. Thus, when the pleura costalis is much inflamed, a portion of the pl. plum. corresponding or opposite to it, is always found inflamed also. So, when inflammation spreads from the pleura to the lungs, it does not follow the reflections of this membrane, but passes directly from the pleura of the ribs to the pleura of the lungs, without any communication of vessels. The following explanation is offered by the gentlemen in question. "When a portion of a serous membrane becomes inflamed, it is rendered highly vascular; it becomes at first dry and rough, but afterwards exhales either a morbid fluid secretion, or coagulable lymph; there is some reason to believe that its temperature is also increased. Now in this state of things, that portion of the opposite membrane which corresponds to it, is thus exposed to the contact of a membrane, whose *sensible properties* are altogether altered from their natural state, and which may therefore be now considered to be as it were a *foreign body*, which presenting a surface quite different from that to which the sensibility of the opposite membrane had been accustomed, must of course act as a stimulus to it, and thereby excite in it an inflammatory action." This explanation seems at least as satisfactory as Mr. Hunter's "Sympathy of Contiguity."—*Med. Chir. Rev.*

*Sympathetic Apoplexy.*—Some people are as tenacious of life as worms, or as animals which we may divide into pieces without destroying vitality. A gentleman, who had long shown symptoms of what Rostan and others would have termed "*Ramollissement du Cerveau*," fell down, the other day, in a fit of apoplexy, at the age of 68, and not the slightest impression was made by cupping, leeching, blisters, enemas, and all the means which a trio of physicians (including Dr. Warren) could suggest. Mapleson left the patient for dead, after taking four ounces of blood from the head; and he was apparently in articulo mortis, after 48 hours of general paralysis, total insensibility, stertorous breathing, glassy eyes, and "dead rattles" in the throat! The physicians parted—to meet no more—at least in that case. The ordinary physician took his leave at 12 o'clock at night, requesting to be informed in the morning at what hour the patient died. No message having been sent, the physician called in the morning, and found, to his no small surprise, the patient at his breakfast, quite sensible, and with the full power of all his muscles!! The patient, soon after this, disgorged some pints of fetid bile, and had no return of apoplectic or paralytic symptoms. This is one of the many cases, where *irritation* of the chylopoietic nerves will simulate diseases of the most fatal character—and especially those of the brain and nervous system generally. Nothing is more common than partial paralysis,

particularly in children, from indigestible matters in the primæ viæ; and we are convinced that many of those apoplectic attacks, from which we see people quickly recover, without any remaining paralysis, are dependent on gastric or intestinal irritation. The forgoing case is a good example.—*Med. Chir. Review.*

*Habitudes of Sulphuric Acid.*—M. Bellani found that the glacial sulphuric acid of Nordhausen, like common concentrated sulphuric acid, had a specific gravity of 1.843 at 50°F. and that it congealed at 53°.6. When it was exposed to air, at nearly a freezing temperature, the surface absorbed water and became covered with crystals, which, when separated and fused, gave a liquid of s. g. 1.78. On the other hand, when glacial acid was mixed with enough water to give a fluid of s. g. 1.793, it, when frozen, supplied crystals which, being melted, gave a liquid of s. g. 1.78, whilst the residual liquid had a s. g. of 1.73.

When sulphuric acid, concentrated or diluted, congeals, it undergoes a diminution of volume, almost equal in extent to the increase which takes place with water under similar circumstances. Thus, 1000 parts of fluid glacial acid become 925 parts by volume of solid acid; and 1000 parts of acid of s. g. 1.78, by congealing, become 910 parts, from which it would appear that the water in the diluted acid does not undergo the same change in solidifying, as when in the free state.—*Giornale di Fisica.*

*Identity of Althea and Asparagine.*—Mr. Bacon, some time since, announced the discovery of a new vegeto-alkali, to be called *Altheine*, or *Althea*, in the roots of the marsh-mallow. He obtained it as an acid malate of althea. An examination of the facts, however, by M. Plessin, leads him to conclude, 1st, that the fine green colour of Mr. Bacon's salt is not essential to it; 2d, that his altheine is a malate; 3d, that the acid malate of althea is not a salt, but a particular azotated substance, having the properties of asparagine; 4th, that this substance, by treatment with hydrate of lead, produces ammonia and a new acid; 5th, that magnesia produces the same change, forming, ultimately, a salt, with apparently alkaline properties; 6th, that the asparagine of marsh-mallow can assume several different forms by crystallization.—*Quarterly Journal of Science, &c.*

*Purification of Alcohol.*—A prize was offered by the Royal Academy of Brussels to the person who should prove upon what the differences between alcohol, extracted from various substances, as fruits, grain, roots, sugar, &c. depended. This was obtained by M. Hensmans, who was led, by numerous experiments, to conclude that the alcohol was always identical, but that the difficulty, more or less great, always found in rectifying it, as well also as the difference in taste, depended upon the presence of a fatty matter, and a

little acetic ether. The fatty matter, when alone, may be separated by several distillations, but the acetic ether is not removed in this way. It is better, in every case, for the removal of both, to add a little caustic potash, or soda, to the alcohol, to be rectified. Carbonated alkali does not act with sufficient energy.—*Bull. Univ.*

*On Proust's Caseous Oxide and Caseic Acid.*—The results obtained by Proust, relative to the substances produced by the fermentation of cheese, have been examined and described by M. Henri Braconnot. The substance which Proust distinguished as caseous oxide, he shows to have no claim to such a title, and proposes to call it *Aposepedine*, as being produced by putrefaction. It also appears to be produced in certain morbid diseases.

The properties which Proust has assigned to *caseic acid*, belongs, according to M. Braconnot, to various contaminating substances, none of which have any title to be considered as a particular acid. The substances present are free acetic acid; *aposepedine*; animal matter soluble in water and insoluble in alcohol, (*ozmazome*); animal matter soluble in both water and alcohol; a yellow acrid fluid oil; a brown resin; acetate and muriate of potash, and traces of acetate of ammonia.

On examining the fatty matter of cheese, Braconnot found it to consist of margarate of lime with margaric and oleic acids; the butter having undergone the same kind of change during the fermentation of the cheese, as that produced when it is saponified by the action of alkalies or other bodies.—*Annales de Chimie.*

*On Testing the Presence of Ammonia in a Substance.*—Having occasion to ascertain whether the action of a salifiable base upon a body containing azote, was simply that of evolving ammonia, previously existing, or that of forming ammonia by the combination, M. Plessin was induced to search for a base which would effect the former object, but not the latter. Potash, lime, magnesia, and many other bodies do both, but the hydrated oxide of lead answered the purpose very well. It gives no indication of ammonia when put into contact with azotated substances not containing that alkali; even urea is not affected by it; but being put in contact with an ammoniacal salt, ammonia was instantly evolved, and rendered evident by the visible fumes which arose upon the approximation of a little acetic acid.—*Annales de Chimie.*

*Memoire de M. Civiale sur les resultats obtenus par l'emploi de la lithotritie en 1827, et sur les reproductions des affections calculeuses.*—Of thirty patients operated upon by M. Civiale, since the month of April of the year just mentioned, twenty-five have been cured, and the remainder were still under treatment when the memoir was published. Among the former was a lad seven years of age, in whom the operation was attended with great difficulty, by

reason of the imperfect development of the parts, and the irritability of the patient; every obstacle however was surmounted; the stone, of the size of an almond, and composed of oxalate of lime, was perforated, and extracted in three sittings of ten minutes each.

The author asserts that the reproduction of the calculi, is not more frequent after lithotritry, than the common operation of lithotomy; and if the reverse should appear to be the case, it arises, according to him, from the fatality attending the latter, which must of course diminish the number of relapses. *A priori*, we might suppose they would occur more frequently after lithotritry, in consequence of some small fragments being left in the bladder, which would thus serve as nuclei to fresh formations; but this is in a great measure obviated by the construction of the instruments, which affords great facility of extracting small bodies. In proof of which, M. Civiale states, that he has removed pieces of straw, and other substances equally minute, which had served as nuclei.—*Nouv. Biblioth. Med.*

*Influence of Electricity upon the Emanation of Odours.*—It is stated in a late number of the *Antologia* of Florence, that when a continued current of electricity is passed through an odoriferous substance, camphor for example, its odour becomes gradually weaker, and eventually disappears altogether. When all electrical influence is withdrawn, it does not recover its odour till after the lapse of a considerable time. Camphor reacquires its former properties very gradually. M. Guillaume Libri, the author of this curious experiment, has promised further details upon the subject.—*Annales de Chimie, &c.*

*Influence of Mercury in the Plague.*—M. Moreau de Jonnés communicated to the Academy of Medicine, the following circumstance, which he observed was well attested by official documents. An Ionian boat having had some communication with a Turkish vessel, her commander contracted the plague, and was labouring under the primary symptoms of this disease on his arrival at Cephalonia. The physician of the lazaretto, supposing that the crew, in number twelve, who had lived together in the boat, had contracted the germs of the disease, endeavoured to prevent its development by subjecting them all, to the full influence of mercury, both internally and externally applied. All the individuals were successively attacked with the disease, but with this remarkable difference; in the captain and one of the sailors who had experienced no sensible effect from the mercurial treatment, it assumed a severe form, and proved fatal; while the others, in whom it had induced salivation, were affected much less severely, and all recovered.—*Archives Générales de Médecine.*

*Extraction of Morphine without the use of Alcohol;* by MM. Henry and Plisson. This process is founded on the facility with which

morphine may be separated from its union with narcotine, by the action of weak muriatic acid. Treat 500 grammes of finely divided opium, with about a pound of warm water, acidulated with 20 grammes of muriatic acid. Repeat this process three times, and having expressed the residuum, add to the filtered or decanted liquors, a slight excess of liquid ammonia or caustic soda of 2° or 3°; the precipitate is to be collected and carefully washed. Acidulate slightly the mother waters, concentrate them to about three-fourths, and decompose as above. The precipitate consists principally of resin, caoutchouc, morphine and narcotine, coloured by brown extractive matter. Treat it repeatedly at a gentle heat, with water very slightly acidulated, as long as any thing is taken up. Filtre and evaporate the liquid, which holds in solution a small quantity of resin, extractive, and a large proportion of the muriate of morphine, and when brown crystals have formed, they are to be washed gently, and twice purified by means of animal charcoal and new crystallizations. The black mother waters are to be decomposed by the alkalies and treated separately. The muriate of morphine purified by these crystallizations, is then dissolved in a very small quantity of weakly acidulated water, and decomposed by a slight excess of liquid ammonia; the morphine received upon a filtre, is washed, and dried by the heat of a stove. 400 grammes of opium of commerce, yielded about 27 grammes of pure morphine.

The following is the process of M. Girardin. Exhaust opium of commerce by means of pure water, concentrate the liquors and precipitate them by a slight excess of liquid ammonia. The dried precipitate is then to be washed with a small quantity of dilute alcohol, and subsequently treated with sulphuric acid till it be entirely dissolved. Filtre, decompose by ammonia, and treat the previously dried precipitate, with sulphuric ether. Dissolved afterwards in alcohol, pure morphine is obtained. Repeating the process of M. Hottot, M. Girardin ascertained, that 100 parts of the precipitated caoutchouc contained 4 of narcotine, 10 of morphine, and 86 of resinous and colouring matter.—*Journal de Pharmacie, &c.*

*On Iodo-fluoric Acid, by M. J. Varvinsky.*—On mixing vapours of iodine and fluoric acid in a glass globe, the latter became lined with a white film, and the iodine was absorbed; when the action appeared to have ceased, water was poured into the globe and caused an immediate deposition of gelatinous silica. By filtration, a liquid was obtained, yellow from free iodine, but becoming colourless by heat; carbonate of ammonia was then added in excess, which separated the rest of the silica, and carbonic acid gas was disengaged. The filtered solution was very alkaline, but by ebullition gradually became quite acid. Being afterwards cooled, it deposited many small crystals of a fine golden yellow colour, and possessing all the properties of a

strong acid. They dissolved more readily in hot than in cold water, and, with caustic potash, produced a gelatinous salt, having a very disagreeable bitter taste. These crystals are the substance which I have called iodo-fluoric acid. On repeating the experiments I have always obtained the same results.—*Bull. Univ. A. viii. 360.*

*Heat evolved during Combustion.*—D. Despretz has lately read some memoirs on the heat evolved during combustion. He found that hydrogen is the body of which a given weight gives out most heat, and the metals the least. But the result is of the opposite kind if referred to equal weights of oxygen. Carbon, which, in burning, does not alter the volume of the oxygen gas it consumes, produces three-fifths of the heat evolved by the metals, iron, zinc, and tin, which reduce the oxygen to the solid state. Hence it is in the act of combination, that we must seek for the principal cause of the development of heat, and not in the approach of the particles. D. Despretz has also found that the quantity of heat developed by a certain quantity of a body which burns without changing the volume of the gas is the same whatever be the density of the gas.—*Phil. Mag. N. S.*

*Salivation cured by Calomel.*—A young man of a cachectic habit of body, was, after sleeping near a wall, attacked with a swelling in the face, unaccompanied with pain, but followed with a profuse salivation. A great variety of means were successfully resorted to, to restrain this immoderate discharge, and Dr. Guimper ultimately directed calomel in quantity of from one to two grains, three or four times a day, by which, at the expiration of six days, it was completely arrested.—*Hufeland's Jour.*

*Remarkable disorganization of the Stomach;* by Dr. Weidemann. In a woman who died at the age of 42, the great curvature of the stomach was found scattered over with tubercles about the size of a hazle nut. In the neighbourhood of the pylorus, the membrane was enormously thickened; towards the lesser curvature, a series of tubercles was found of the size of an egg, of the same nature with the preceding. Similar tubercles were also found on the surface of the liver and peritoneum.—*Bull. Univ. from the Eyr. et Medicinsk Tidsskrift.*

*New Principle detected in Asarum Europæum.*—M. Régimbeau of Montpellier has recently announced the existence of a new principle in this plant, to which he has given the name *asarine*. This substance, which he supposes analogous to violine and emetine, exists in combination with gallic acid. He prepares, in the first place, an alcoholic extract from the dried root of the plant; this extract is subsequently treated with water, and

the asarine precipitated by the carbonate of magnesia. MM. Lassaigue and Feneulle had already obtained from the same plant, a principle similar to that furnished by the seeds of the cystisus Laburnum, and which they have designated under the name of *cystisine*.—*Revue Médicale, &c.*

*M. Larrey's method of curing Hydrocele.*—The following plan, adopted by the gentleman just named, is stated never to fail in effecting a radical cure of this disease. The patient being in a recumbent posture, the operator gathers up transversely, the skin covering the most depending portion of the tumour, one extremity of which fold is held by an assistant, the other by himself; an incision is made into this part, and the tunica vaginalis being thus exposed, a flattened trocar, terminated with a triangular point, not unlike that of a pharyngotome, is introduced. After the serum is evacuated, a gum-elastic catheter, pierced near its extremity, is passed into the cavity of the tunica vaginalis, by the assistance of the canula of the trocar, and permitted to remain till it has excited a degree of inflammation sufficient to effect the adhesion of the parietes of this membrane.—*Archives Générales de Médecine.*

*Poisoning by Sulphuric Acid; by Professor Carus.*—A woman at the completion of the full term of utero-gestation, poisoned herself with concentrated sulphuric acid; she concealed the deed till the instant of her death, when the last efforts of nature were employed in the expulsion of the child. On dissection the acid was found in the cavity of the pleura, in that of the peritoneum, in the heart, bladder, and even in the water of the amnios.—*Gemeins. deutsche Zeitschr. für Geburtskunde.*

*Retention of the Placenta; by Dr. Primus.*—A woman was delivered on the 25th January, of a dead child, in which the putrefactive process had already commenced. The midwife made several unsuccessful attempts to extract the placenta, in one of which she ruptured the chord. The placenta was retained in the uterus, the os tinæ closed, and no contractions indicating its expulsion supervened—there was no lochial discharge. The woman notwithstanding, continued in good health until the 14th of the following May, when she suffered from colic, and had a slight hæmorrhage. These symptoms which continued but a short time, reappeared with greater intensity on the 22d of the same month, and were followed by the expulsion of the placenta, the long retention of which had been productive of no unpleasant symptom.—*Ibid.*

At a recent meeting of the Société Anatomique, M. Bérard presented an extremely interesting case of hypertrophy of the great

sympathetic, in its cervical portion. The superior ganglion equalled in volume a damask prune. M. Bérard has promised a more detailed description of this case, which is probably the only one on record.—*Nouv. Biblioth. Médicale.*

## New Publications.

Prodromus Systemate Naturalis regni vegetabilis, pars tertia. Auctore Aug. Pyrame De Candolle.

Ce troisième volume est tout entier consacré à la continuation des plantes dicotylédones. L'auteur commence par les colicanthées et finit aux grossulariées. Vingt-cinq orders y sont décrits avec leur genre et sous-genre, etc. Une table des noms et des synonymes des plantes mentionnées dans le volume, termine cette troisième partie.

Memoires de l'Académie Royale de Médecine. Tome 1er. 1 fort vol.

De la *Percussion Mediate* et des signes obtenus à l'aide de ce nouveau moyen d'exploration dans les maladies des organes thoraciques et abdominaux; par A. Piorry, D. M. P. etc. etc.

Etudes Clinique sur les émissions sanguines artificielles; ouvrage qui a remporté le prix proposé par la Société Académique de Médecine de Marseille, par Isodore Polinière, M. D. Médecin de l'Hôtel Dieu de Lyon, etc. etc. Deux vol. in 8vo.

Anatomie Comparée du système dentaire chez l'homme et chez les principaux animaux; par L. F. Emmanuel Rousseau, M. D. chargé des travaux anatomiques au Muséum royal d'histoire naturelle de Paris, etc. avec trente planche dessinées d'après nature; par I. C. Wernet, peintre au Muséum; dédié à M. le baron G. Cuvier.

## In Press,

Abrégé Pratique des Maladies de la Peau, fait d'après les auteurs les plus estimés, et surtout d'après des documents puisés dans la clinique de M. Biéty, M. D. Médecin de l'hôpital St. Louis; par MM. Cazenave et Schedel, docteurs de Médecine, etc. un fort vol. in 8vo.

Die Steinbeschwerden der Harnblase, etc. Des Maladies de la vessie et de l'opération de la taille chez les deux sexes; par Vincent de Kern, chirurgien de l'empereur d'Autriche. In 4, avec le portrait de l'auteur, et 9 grandes planches.

Handbuch der Staatsarzneikunde—Manuel Systematique de Médecine légale; par Jos. Bernt. 3me. édition, augmentée.

Osservazioni intorno all' efficacia del seme di senapa bianco, etc.—Observations on the efficacy of mustard seed, taken internally. Tenth edition, enlarged.

THE  
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From the Transactions of the Medico-Chirurgical Society  
of Edinburgh.

**ON THE SUDDEN SPONTANEOUS OBSTRUCTION OF THE CANALS OF THE LARGER ARTERIES OF THE BODY; with some Observations on the process employed by Nature to prevent or arrest Hæmorrhage from lacerated Arteries.** By JOHN W. TURNER, Professor of Surgery to the Royal College of Surgeons, and Consulting Surgeon to the Edinburgh New Town Dispensary.

It has sometimes been observed, that the pulse has suddenly and permanently disappeared in one part of the body, while it has continued distinct in the other parts; and, in some cases of this kind, on examination after death, it has been found, that an obliteration had taken place of a portion of the tube of the artery in which the pulse could not be felt during life. This morbid affection of the arteries appears, till very lately, to have escaped the notice of pathologists, and no full or accurate description of it has as yet been published. I have therefore been induced to attempt to give a connected account of a considerable number of cases of it, which have either fallen under my own observation, or have come to my knowledge as observed by others.

The first instance with which I am acquainted, where this sudden and spontaneous obstruction of the arteries was observed, and its true nature suspected, was in a patient under the care of Dr. Thomson and myself in the year 1814. An account of this case was, soon after its occurrence, communicated to Mr. Hodgson, who published it in the Appendix to his valuable Treatise on the Diseases of the Arteries and Veins. But as it appears to me interesting, from its having been the first of the series, and as it was minutely and carefully observed, I conceive it will not be out of place to repeat some details of it here.

CASE I.—A gentleman, about 40 years of age, of a full habit, was exposed to cold on the 1st of January, 1814. This produced a feeling of chilliness, and occasional alternate cold and hot fits, which continued to increase in frequency and severity. On the 9th, when he first sought professional assistance, he was affected with smart fever, accompanied by bilious

symptoms, a slight cough, and feeling of oppression on the chest. Under the use of laxative medicines, spare diet, and confinement to bed, these symptoms speedily abated, and on the 15th he considered himself so well as to get up and leave his bed-room. In consequence of this, he suffered next day a return of his complaints, to which were added a diffused pain over the right side of the chest, with oppressed breathing. He was now bled at the arm; and aperient medicines and diaphoretics were given. The blood was not buffy: the cough abated; and the pain left the breast, but extended down the side, and two days afterwards attacked the right groin near the insertion of the pectineus muscle, shooting down the limb, and particularly affecting the calf of the leg, and sole of the foot. This pain continued violent for two days, and alternated between the groin and the calf of the leg. There was considerable heat in the pained part of the thigh, and the femoral artery was observed to throb very violently in the groin. The fever continued, but was mild; the feelings were easy; the pulse 96; and the surface generally covered in the course of the day with a free perspiration. In about a week a solution of the fever seemed to be taking place, when, after fatigue, he was again attacked by rigours, accompanied with feeling of great depression, and an affection like globus hystericus. This was followed by feverish heat, and an increase in the frequency of the pulse, which ranged from 94 to 112. These symptoms continued without amendment, and the patient became much emaciated and debilitated. On the 11th of February, the pulse suddenly fell to 70, but without relief of the other symptoms. It was weak, and beat twice rapidly in succession after every fifth stroke. With the advice of Dr. Hamilton, who had been consulted, port-wine was given, instead of claret, which he had previously been using to a moderate extent; and, after taking three glasses, the pulse became regular, and more firm. On the 13th he had three copious evacuations from the bowels, of fluid consistence and black colour, not unlike dissolved blood. This was accompanied by very alarming symptoms of sinking. He was affected by a severe rigour, followed by profuse sweating. The pulse became quick, and very feeble. He

complained of extreme langour. The countenance was pale and fallen, and he had lost the recollection of past events.—Wine was freely administered; and, on the 15th, he was better, being collected, and less languid. By the 20th the evacuation from the bowels, which had continued black, had become of a lighter colour; and from this time he gradually continued to improve in strength, and to take his food with appetite.

It is to be remarked, that during this last attack, about the 7th of February, a slightly painful and hard circumscribed swelling was observed on the inner side of the tibia, at the upper part of the calf of the right leg. This swelling disappeared spontaneously in a few days. In the earlier stages of the ailments, the pulsations in the arteries were occasionally observed to be remarkably sudden and throbbing.

On the morning of the 5th of March, after the patient had been for several days so well as to be able to leave his bed-room, and when he appeared gradually convalescent, on raising himself in bed, by resting on the palms of his hands, with his arms stretched behind him, he experienced a sensation as if something had given way at the elbow joint of the right arm. This was immediately followed by numbness of the hand and fore-arm, and a sensation of circumscribed tightness over the fore part of the elbow joint. The hand soon became cold, and, on trying to feel his pulse at the wrist, he was surprised and alarmed to discover that it had entirely ceased. On examination during the day, no pulsation could be detected at the wrist, nor in any part of the fore-arm below the tendinous expansion of the biceps muscle. Above this spot, the pulsation in the humeral artery was strong, and opposite the elbow joint it appeared stronger than in the other arm, but entirely confined to the site of the artery. No tumour or swelling could be perceived; but on feeling the course of the artery between the finger and thumb, immediately above the part where the pulsation ceased, its tube seemed to be rather larger and harder than in the other arm. He complained of pain, of a pricking kind, at the bend of the arm, somewhat increased by pressure. The numbness, paleness, and coldness of the hand continued, and he had a slight tingling feeling in the fore-arm, but the powers of sensation and of motion were not impaired. In the evening the pain had increased, and extended along the fore-arm; and the pulsation opposite the elbow joint had diminished in strength, being now not so strong as in the other arm. A very feeble pulsation could be observed in the radial artery at the wrist, and the hand, which was covered with flannel, had become warmer. On compressing the fore-arm, the veins in the back of the hand and wrist were seen to fill slowly.

On the 6th, the parts surrounding the artery at the elbow were less lax, and a slight hardness was felt along the inner edge of the tendon of the biceps muscle. The pain was more severe on pressure and motion, and ex-

tended, in the course of the artery, one inch above the point where the pulsation ceased, and two inches downwards on the radial side of the fore-arm. The numbness had considerably abated, and the pulse could be felt at the wrist very feeble but sufficiently distinct to permit its being counted. The fulness and hardness in the course of the artery, and the pain on pressure, rather increased till next day, when they began to abate, and then very gradually disappeared. The numbness of the hand also went off. The pulsation at the wrist did not increase in strength from the second day of the affection.

While we were anxiously watching the progress of these changes in the arm, the patient was attacked by a similar morbid affection in the right leg. On the 9th March, five days after the stoppage of the pulse in the arm, about midnight, in moving his right leg without any great exertion, he experienced suddenly a sensation of numbness and weight, extending from the ham downwards. On examination, in half an hour after, no pulsation could be felt in any of the arteries of the foot, or in the ham. The pulsation of the femoral artery was very strong, and could be felt all along the thigh to the part where the vessel perforates the triceps muscle, but no farther. The foot was cold: no pain was excited by pressure on any part of the limb, but he complained of occasional cramp-like pains in the calf of the leg. The next day pain was excited by pressure on the course of the artery in the lower part of the thigh, where the pulse ceased, and in the ham. Above this the femoral artery beat very strongly, the pulsations in the groin communicating the sensation to the fingers of a pulsating tumour; and a strong pulsation was felt in the abdomen, along the course of the aorta. The pain, occasioned by pressure and motion of the limb, continued to increase in the lower part of the thigh for three days, after which it gradually abated.

The morning after the attack, the foot was pale and cold; and below the ankle the integuments were entirely devoid of sensation, when pressed, pinched, or tickled, and the muscles of the foot seemed to have lost the power of contraction. The next day several mottled purple patches appeared on the instep and fore part of the ankle, which gradually extended over the whole foot, till the surface by the fifth day had become entirely livid. At first, minute branches of veins, full of blood, were intermixed with the purple efflorescence: they could be emptied by pressure, and slowly filled again on the compression being removed. As the discoloration advanced, the foot swelled slightly, and became œdematous, and appeared to acquire an increase of temperature. Soon after the attack, he complained of severe burning pain in the foot, and a feeling as if it were crushed, which continued till near the death of the patient. About the ninth day, the soft parts above the ankle began to swell, and to be hot and painful on pressure: the swelling gradually increased, and extended till it reached the upper part of the calf of

the leg. The integuments above the ankle began to be discoloured on the ninth day, and the discoloration increased till the lividity reached the calf of the leg, and at last rapidly extended nearly to the knee. The soft parts adjoining the discoloured skin were swollen, and very painful on pressure; but no redness appeared, nor any inflammatory line between the dead and living parts. The parts discoloured were completely insensible.

On the seventh day after the attack, several tense globular vesications and bullæ appeared on the foot, some filled with reddish, and others with pellucid serum. They increased in number, and extended above the ankle to the calf of the leg.

When the patient suffered the attack in the leg, he was already much reduced by his previous illness. He afterwards became still weaker, his pulse increased in number, and he had frequent copious sweatings, and occasionally a tendency to faint. After the swelling began to reach the leg, his constitution became more affected; the pulse reached 120. He had great languor, and profuse perspirations. His breathing became laborious, with aggravated paroxysms of dyspnœa, and attacks of cough, with expectoration of mucus tinged of a red colour. His throat also became aphthous. These symptoms continued, the pulse becoming more feeble, till the 27th March, when, while sitting up in bed, he suddenly fell back, and, after two or three deep inspirations, expired, eighteen days after the pulse ceased in the leg, and twenty-two after the arm was first affected.

The state of the body was minutely examined after death, in doing which, we were assisted by the late Dr. Gordon.

The brachial artery was exposed, and traced to the fore part of the elbow, where it was imbedded in a hardened mass, composed of the surrounding cellular membrane, veins and nerves, which adhered firmly to each other, and to the artery. When the last was slit open, it was found to be impervious immediately before dividing into the radial and ulnar branches. At this part its caliber was much contracted, and filled with a cylindrical clot of firm lymph, which extended about a quarter of an inch upwards, from the division of the vessel, and adhered firmly to its internal surface. The orifice of a large branch was situated immediately above the termination of this clot of lymph.

The radial artery was obliterated for nearly an inch and a quarter. Its canal was much contracted, and filled with lymph adhering firmly to its internal surface, and terminating in a delicate conical process, about two lines in length, which projected downwards into the tube of the artery. The obliteration extended only to the commencement of the ulnar artery; the internal coat of which terminated in a distinct edge, which proceeded about half an inch downwards, in an oblique direction from the obliterated part. The coats of the arteries in the obliterated space were hard, thickened, of a whiter colour than natural, and adhered in-

separably to the surrounding parts. Above and below, they appeared sound in every respect.

On cutting into the inferior extremity, a considerable effusion of a clear serous fluid was found in the thigh, and upper part of the leg, and the muscles were rather pale. But lower down, where the skin was discoloured, the muscles were of a dark colour, very flaccid, and easily torn; and in the course of the incisions, copious coagula of black blood were brought in view. On tracing the vessels, considerable hardness and adhesions were found, where the femoral artery perforates the tendon of the triceps muscle, and in the upper part of the ham. When the artery was slit open, an ovoid sac was found in its course, immediately after the vessel passed through the tendon of the triceps. This sac contained a hard solid substance, resembling lymph, of the size and shape of a nutmeg. The surface of this coagulum was covered with a thick, grayish coloured fluid, resembling pus. The coagulum was unconnected with the containing sac, except at its upper extremity, where it adhered very firmly. Immediately above this sac, the tube of the artery was obliterated to the extent of half an inch. Its coats were much thickened, and its parietes were in contact, and adhered to each other. From the obliterated portion, a large conical coagulum extended up the vessel for three inches. To the distance of more than an inch and a half below the sac, the coats of the artery were thickened, and its canal was filled with a very firm coagulum of lymph, which adhered strongly to its internal surface, and sent a strong conical process, half an inch in length, downwards. The sac appeared to consist of several layers. The inner of these arose from the coats of the artery, but no appearance of the internal coat of the artery, or of circular fibres, could be observed. At the attachment of the sac to the inferior portion of the artery, the internal coats of the vessel terminated in a ragged margin. The inner layer of the sac appeared to be composed of the dilated external coat of the artery, and the other layers to be formed by the surrounding cellular substance.

Beyond the lower obliteration, the popliteal artery was pervious, and empty, for the extent of about two inches; it appeared sound in every respect, except in there being a small crucial fissure, or laceration of the internal coats, opposite to the origin of a large branch, which arose from this portion of the vessel. Below this part, the artery was again obliterated, to the extent of an inch and a half, by lymph effused into its cavity, and adhering to its parietes, though not so firmly as in the obliterated portions already described. This closed portion terminated in a thin sac, which would contain a small bean, filled with a fluid resembling pus, and similar to that found in the sac of the femoral artery. The sac was easily torn; its internal surface was ragged, and of a white colour, and it was connected with the artery both at its upper and lower part. The origin of the anterior tibial artery

was attached to the middle of its parietes, and the coats of this vessel were so much thickened, that its canal was nearly obliterated. Below the sac the posterior tibial artery was filled with lymph for nearly two inches; the lymph adhered to the vessel, and terminated in a conical clot, where the peroneal artery is given off. The internal coat of both the inferior and superior portions of the artery were observed to terminate abruptly at the commencement of the sac. The plugs of lymph found in the four obliterated portions of the artery, terminated opposite to the origin of branches from its trunk. The peroneal artery was in a natural state for a short space, but its coats suddenly became hard and thickened, and its canal almost closed. Beyond this point, its internal surface was of a dark purple colour, and the coats of the vessel had lost their elasticity, marking the place to which the gangrene had extended. The femoral vein adhered to the femoral artery, opposite to the dilated part of the latter. The coats of the vein in this situation were thickened, and its diameter somewhat diminished. Below this part, its cavity was, in several places, filled with coagula of blood. The femoral artery above the first closure was healthy, as were also portions of the aorta which were examined. The brain was sound, having only a slight serous effusion on its surface. The abdomen was examined, but no diseased appearances were discovered. There was an effusion of about ten ounces of bloody serum in the left side of the thorax, and about one-half that quantity of pellucid serum in the right side. The lungs were healthy; the heart was of large dimensions, but its structure was natural.

I have been induced to detail the above case at considerable length, as it appears to me interesting, not only on account of the singular affection of the trunks of the arteries, but also as illustrating the effects of the sudden cessation of the circulation in the extreme parts of the body. Thus it was observed, that the obstruction to the supply of blood to the leg produced important effects on the functions of the nerves of the foot; for it was ascertained very soon after, that the skin of the foot was insensible, and apparently the muscles had lost their power of contracting; at the same time a burning pain, and feeling as if crushed, were induced in the foot. It is not easy to ascribe these effects to any thing else but to the deficient supply of blood to the branches and extremities of the nerves, for the trunk of the nerve was undisturbed and uninjured. The progress and appearances of mortification from sudden deficient circulation, were well exhibited, and minutely noticed. They seem to confirm the opinion, that, even in mortification from deficient circulation, a certain degree of reaction or inflammation precedes the complete death of the part, as was shown by the vascular congestion, the swelling, the increase of heat, and the tense vesications which took place on the foot and leg. They also support the very graphic description which has been given by Mr. Guthrie of gangrene from inju-

ries of the main trunks of the arteries of the extremities.\* The commencement of the disease in the extreme part of the limb, the increase in its rapidity, and the more acute and inflammatory character of the gangrene, after it reached the lower part of the leg, were very distinctly marked.

CASE II.—The next case I have to notice is that of a gentleman, 52 years of age, who was under the care of my friend Mr. W. Wood, and who was visited during his illness by Dr. Thomson and Dr. Abercrombie. He was first taken ill in January, 1821. His complaints were not of a very defined character, nor did the symptoms indicate much danger, except from their continuance. He became weak and emaciated, with quickened pulse and occasional feverish feelings, and sometimes complained of a slight transient uneasiness across the epigastric region. Although he was able to take a good deal of food, these symptoms continued, and at last he was attacked with diarrhoea, three days after which he died, on the 5th of July of the same year.

A few weeks before his death, while turning his hand behind his back, to put it in his coat-pocket, he experienced a sudden acute pain at the bend of the elbow joint, and a sensation of numbness in the hand and fore-arm. On examination, a slight hardness was felt in the fore part of the elbow joint, which was painful on pressure. No pulsation could be felt in any of the arteries of the fore-arm, but in the humeral artery the pulsation was natural as far as the elbow joint. In a few days the pulse could be again felt in the fore-arm; and after a few days more, a similar accident occurred, in the same manner, but the pulsation in the fore-arm never returned. The pain at the elbow gradually abated.

On examining the body after death, a considerable abscess was found in the spleen. On inspecting the arm, the humeral artery, about half an inch above its division into the radial and ulnar branches, was found firmly adhering to the surrounding cellular substance, which was indurated. On slitting open its tube, the artery was found contracted, and completely obliterated at this part, the internal parietes adhering firmly together. The outer coat of the artery seemed incorporated with the surrounding parts. Immediately below the obliteration, the inner coats appeared to terminate in an abrupt edge, and they could also be seen terminating in a distinct margin at the upper part. Above the obliteration, the vessel contained a firm coagulum of lymph.

I have given the account of this case chiefly from a statement of it published by Dr. Abercrombie, in a paper on Diseases of the Spleen,† and partly from information afforded me by Mr. Wood, and by my own examination of the artery after death.

When the stoppage of the circulation in the

\* See a Treatise on Gun-shot Wounds, &c. 2d Edition. p. 129.

† Edinburgh Medical and Surgical Journal, vol. xxii. p. 4.

arm, in the case first related, took place, it naturally excited speculations as to the cause of so singular an occurrence. The idea immediately suggested itself to Dr. Thomson, that the obstruction was produced in the same manner as hæmorrhage is sometimes prevented in arteries which have been torn across, and, as he believed, obstruction of the arteries had been occasioned, in some cases of gun-shot wounds which he had observed, where the ball had passed close to the trunk of an artery without dividing it; viz. by the laceration of the internal coats of the vessel, and the projection of their edges and flaps into its tube, by which an obstruction is immediately produced to the passage of the blood, the detained portion of this fluid coagulates, and afterwards the vessel is obliterated by the adhesive inflammation excited in the site of the lacerations.

It is well known that lacerated wounds bleed less than incised wounds of the same parts; and it has also been shown, by numerous cases, that, even where large arteries are ruptured, the hæmorrhage ceases more readily than in such arteries when cut; and, in some cases, where even the largest arterial trunks of the limbs have been torn across, no bleeding, or only a very slight one, has occurred. There are various instances in the records of surgery, of limbs torn off near to the trunk of the body, which established these facts. Among these, perhaps, the most remarkable are the well known case recorded in the *Philosophical Transactions*, of the young man who recovered after his arm, along with the scapula, had been torn away by the wheel of a mill, and in whom the bleeding speedily ceased, and did not recur, although no means were taken to obstruct the artery;\* and one which occurred to Mr. Carter, a surgeon at Kettle, in which the thigh of a boy was torn off at the groin by the wheel of a slitting mill, and in which no bleeding took place during four days that the patient survived the injury.† There are numerous other cases of the same kind, to which I consider it unnecessary particularly to refer; but I may remark, that no satisfactory explanation is given in the accounts of any of them, of the cause of the absence of the bleeding.

When an artery is torn across, the edges of the division of the coats is irregular, and the vessel has a tendency to retract into the cellular substance, which also is divided with a ragged surface. The blood will thus flow less freely, and will be more entangled in its passage from the orifice of the vessel, than in an artery cut across, and a greater tendency in the blood to coagulate be thus produced, which is known to be one of the means by which nature opposes an obstruction to the flow of blood from the orifice of a vessel, when hæmorrhage spontaneously ceases. This effect will be still more certainly produced, if the

artery has given way within its sheath, at some distance nearer the heart, than the surface of the wound, and a portion of its tube has been drawn out, as has occurred in some of the cases of limbs torn off, which have been recorded. But these circumstances are obviously by no means sufficient to account for the very slight bleeding, or the total absence of it, in cases where the large trunks of the arteries of the limbs have been divided by laceration. Indeed, we know, that, in many cases, profuse, and even fatal hæmorrhage has taken place from torn arteries, or those divided in contused wounds, both in accidents in the human body, and in experiments on the lower animals.

From conversations with Dr. Thomson, and from his lectures, I have learnt, that, when he began to give lectures on surgery, his attention was particularly directed to the subject of hæmorrhage, and to the means by which nature obstructs the flow of blood from arteries, in cases in which the bleeding spontaneously ceases, or does not take place; of which, those where limbs had been torn off without bleeding, or with only a slight degree of it, especially attracted his notice. He made various experiments on the effect of violence on the arteries in the dead body. He was aware of the fact which had been long ascertained, that the internal coats of the arteries are less extensible and more friable than the external coat. He found, that if an artery be forcibly elongated, the internal coats first give way, the external continuing to stretch. He also found, as had been ascertained by the experiments of Nichols, that if an artery be violently distended by a fluid thrown into it, the internal coats are lacerated, the external coat remaining entire; and he confirmed the observation made by Desault, illustrating the same fact, that, on tying an artery firmly with a ligature, the internal coats are cut through in the course of the thread, while the external remains undivided. When the inner coats were lacerated, and fluid injected into an artery, he remarked, that they were separated from the external coat; and, projecting into the canal of the artery, obstructed the flow of the fluid through it; and that, if the fluid was continued to be propelled, it insinuated itself under the torn portions of the inner coats, and dilated the external tunic in the portions of the artery nearer to the syringe than the site of the detached flaps.

About this time, a case occurred to Dr. Thomson, of a lacerated wound of the hand in a young man, in which the soft parts between the metacarpal bone of the thumb and of the fore-finger were torn asunder by the bursting of a pistol. A considerable portion of the radial artery hung out from the wound on the back of the hand, with an open mouth, yet no blood flowed from it, though the pulse was distinct at the wrist. These circumstances convinced Dr. Thomson that the obstruction to the hæmorrhage must have been produced by some change within the tube of the vessel itself; and he conjectured, that it was by the laceration of the internal coats, which he had

\* *Philosophical Transactions*, vol. xl. p. 313.

† *Medical Facts and Observations*, vol. i. p. 17.

often produced in the dead body on the forcible elongation of arteries; and which he had found to prevent the flow of fluids through them, when thrown into them by a syringe.

This conjecture, though sufficiently probable, was not verified by any observation on living animals, until the period of the investigations undertaken by the late Dr. Jones, at the suggestion of Dr. Thomson; the important results of which were published by Dr. Jones, in his well-known valuable work, entitled, "*A Treatise on the process employed by Nature in suppressing Hæmorrhage from divided and punctured Arteries.*"

Dr. Jones, in this work, has not particularly directed the attention of pathologists to the effect of the division of the internal coats in arresting hæmorrhage from lacerated arteries, although the fact is very distinctly marked in the relation of one of his experiments. The carotid artery of a horse was divided by a sudden and violent pull of a director passed under it. The upper part of the artery retracted, whilst the other hung out of the wound three inches. The hæmorrhage was considerable from the former, and would have been profuse from the later, but was prevented by making a ligature on it. The wound was closed, and the bleeding from the upper portion soon stopped: the animal was killed eighteen hours after the operation. On examination, the external coagulum was formed, as usual, in the canal of the sheath. To it adhered a long black coagulum, filling four inches of the canal of the artery. This was the internal coagulum, and purely of blood, yet it adhered to the artery in very many parts. To account for this unusual appearance, the artery was examined. Its internal coat had been lacerated in numberless parts, and at every laceration there had been an effusion of lymph, to which the internal coagulum adhered. In this instance, Dr. Jones adds, contrary to all preceding and subsequent experiments, "the internal coagulum seems to have had some share in the security against hæmorrhage, but only in consequence of the injury done to the internal coats of the artery."\*

The influence of the injury of the internal coats of a lacerated artery in preventing bleeding, were particularly pointed out by Dr. Thomson, in his *Surgical Lectures*; and, in his work on *Inflammation*, he states, in treating of wounds of arteries, "If, lastly, an artery, instead of being divided with a cutting instrument, be torn asunder, a very inconsiderable quantity of blood will in general be lost. The inner surface of the ruptured artery will be found torn in various places, contracted, and containing coagula of blood. This is a state of arteries which has been first accurately ascertained by Dr. Jones, in his *Experimental Investigations.*"† This explanation of the absence of hæmorrhage in certain cases of

ruptured arteries, I believe to have been new; and I do not find it mentioned in various systematic works on surgery, published since it was proposed. The late Professor Beclard of Paris, whose premature death must be regarded as a serious loss to the progress of the science of medicine, is the only author with whose works I am acquainted, who has adopted a similar view of the subject. In his work, entitled, "*Additions à l'Anatomie Générale de X. Bichat,*" published in 1821, in speaking of the wounds of arteries by laceration, he says: "J'en ai rassemblé un certain nombre d'observations prises dans les auteurs; la plus remarquable est celle de Samuel Wood, rapportée dans les *Transactions Philosophiques*, et depuis dans divers ouvrages. Dans quelques-unes de ces observations, la mort a été la suite d'une hémorrhagie abondante; mais dans le plus grand nombre, comme aussi dans les expériences que j'ai faites sur les animaux, la guérison a eu lieu. Outre la rétraction et la resserrement indiqués par Bichat (page 310,) deux causes s'opposent encore dans ce cas à l'écoulement du sang et favorisent l'oblitération de l'artère. En effet, à l'instant même de l'accident, celle-ci cède et s'allonge avant de se rompre; mais les membranes internes, moins extensibles, se déchirent d'abord inégalement et en divers endroits, puis se séparent complètement, tandis que la tunique celluleuse continue à s'allonger, en se rapprochant de plus en plus de l'axe du vaisseau, comme le fait un tube de verre fondu qu'on tire par les deux bouts. Quand la séparation est achevée, l'artère offre donc à son extrémité un prolongement conique, terminé par une ouverture étroite, et dans son intérieur des lambeaux irréguliers qui en obstruent la cavité. Cette dernière circonstance paroît la plus importante des trois, car, 1<sup>o</sup>, la rétraction manque souvent, le bout de l'artère est pendant, sans qu'il y ait pour cela d'hémorrhagie; 2<sup>o</sup>, en coupant sur un animal le sommet de l'espèce de cône que représente l'artère, on ne renouvelle l'écoulement du sang qu'autant que la section est pratiquée au-dessus des déchirures intérieures."\* Beclard, in his *Anatomie Générale*, published in 1823, in speaking of the cessation of bleeding from arteries torn across, also states: "On a attribué cette cessation prompte et définitive de l'hémorrhagie, qui a presque toujours lieu dans ce cas, à la retraction de l'artère, et à d'autres causes imaginaires: beaucoup de cas observés dans l'espèce humaine, et beaucoup d'expériences fait sur les animaux, m'ont convaincu que c'était aux ruptures intérieures plus ou moins multipliées qu'éprouve l'artère avant de se dévisser totalement en un point, qu'il fallait attribuer ce phénomène remarquable. Les phénomènes consécutifs sont les memes qu'après la section transversale."† Whether Beclard was led entirely to the observation of these phenomena from his own experiments, he does not mention;

\* On the Process employed by Nature in suppressing Hæmorrhage, &c. p. 42.

† Lectures on Inflammation, p. 512. 1813.

\* Additions, &c. p. 95.

† *Elémens d'Anatomie Générale*, p. 384.

but he does not refer to any other authority. The works of Dr. Jones and of Dr. Thomson were given to the world long before his were published; and I know that the preparations of the obstructed arteries of the first case I have related, preserved in the Museum of the College of Surgeons, were shown, and their pathology explained to him, several years before the appearance of his works, from which I have made the foregoing extracts.

There are but few cases recorded, in which the state of the arteries torn across, has been ascertained by examination, in the human body. Besides those mentioned generally by Beclard, I shall shortly notice some, which illustrate the subject, and appear to me to confirm sufficiently the explanation I have stated, of the absence or speedy cessation of hæmorrhage from vessels in such circumstances.

The first which I shall relate, is one which occurred in the Royal Infirmary here in the year 1811, under the care of my friend Mr. Newbigging, the particulars of which have been kindly communicated to me by Mr. George White, who was then his clerk.

William Elliot, aged 18, was brought into the hospital on the 29th March, at the hour of visit, having about two hours before got the right arm and left thumb shockingly mangled by the rollers of a lint mill. The axillary artery, which was exposed, had been ruptured about three inches below the axilla, and a coagulum of blood, of about three-fourths of an inch in length, had been formed in the proximal extremity of the vessel. The axillary plexus of nerves was as bare as if it had been dissected. The parts around the shoulder joint, and below the clavicle, were quite undermined. Amputation, a little below the joint, was immediately performed by Mr. Newbigging, who removed the arm, so as to get a flap from the tattered integuments to cover the bone, which was sawn about two inches below the joint, removing only the ragged extremity where it was fractured. The axillary artery was tied a little above the coagulum. Three other arteries were also secured by ligature. As there was a considerable oozing of blood from the end of the bone, a dossil of lint was applied to it. The stump was then dressed in the usual way. Sloughing took place of the lacerated soft parts, and of a portion of the neighbouring integuments, after which the wound granulated. In the progress of the case, abscesses formed over the sternum, a small portion of bone exfoliated, and the patient became affected with diarrhœa and sweating, and the granulations unhealthy and indolent, so that the progress of the cure was slow. In July, his general health had improved, but the sore was still unhealed, and, as his residence in the hospital was thought to retard his recovery, he was advised to go into the country. The sore afterwards completely healed. Mr. White had, a few weeks ago, the pleasure of accidentally meeting his patient in good health.

Mr. White examined carefully the amputated arm. On tracing the humeral artery, it

was observed to be swollen out at several places into little knots, between which it appeared contracted; and, on slitting it up, it was found that this appearance was produced by the internal coats having been lacerated at various parts and coiled up within the external coat which remained entire, so as completely to obstruct the canal of the artery. In the description of the case I have given, which is that drawn up by Mr. White, when the man was received into the hospital, it is stated, that a coagulum of blood had been formed in the torn axillary artery, about three-fourths of an inch in length. Mr. White has since mentioned to me more particularly the appearances that presented themselves. Immediately above the torn orifice of the artery, from which no blood flowed, it appeared swollen out into a bulb. This enlargement exactly resembled those he found in examining the humeral artery. No opportunity was afforded of examining the state of the portion of the artery above, as, after the ligature was applied beyond it, it was left attached to the stump; but Mr. White has no doubt that the knot on its extremity was produced in the same manner as those in the course of the humeral artery.

The next case I have to mention is one which occurred to Mr. Lizars, and has been published by him as illustrating the manner in which nature stems the bleeding of a great vessel. It is that of a gentleman, who, in the overturn of a mail-coach, was thrown from the top, had his leg entangled under the iron-rail, and was dragged with his limb in this position for a short distance. The leg was dreadfully shattered, and the ham exposed to the bone, exhibiting the nerves, blood-vessels and muscles, as if they had been displayed for demonstration. The popliteal artery felt like a cord, had no pulsation, and the leg below was cold and lifeless. Amputation above the knee was performed about six hours after the injury, and the patient recovered. Previously to examining the amputated limb, Mr. Lizars attempted to inject the popliteal artery, but little or no injection entered. On examining the arteries, the anterior tibial, the posterior tibial, and fibular branches, were found empty and collapsed. The popliteal artery was then laid open, when, at one part, the serous and muscular coats were seen ruptured across, a portion of them insulated and plugging up the vessel; and, above this, a clot of blood in the artery, and blood effused under the cellular coat, which had remained comparatively sound and entire. Mr. Lizars has accompanied his description by a delineation of the artery, which illustrates very distinctly the state of its coats.\*

This case proves very satisfactorily the effect of the violent elongation of an artery in the living human body, in producing a laceration of the inner coats, while the external coat remains entire; and also the complete me-

\* *Edinburg Medical and Surgical Journal*, vol. xix. p. 365.

chanical obstruction of the canal of the artery which is produced by this.

Another case, to which I may refer, is one which occurred to a Dr. Mudie of Montrose, so long ago as the year 1761, and published under the title of an Amputation of the Arm without Hæmorrhage,—an occurrence which appeared to the author extraordinary and not easily explicable.

A vigorous woman, thirty years of age, had her hand caught in the machinery of a lint-mill, and her fore-arm and arm drawn into the mashing machines. By this the fingers and hand were severely crushed, and several of the bones of both broken, the ligaments at the wrist torn away, the radial artery divided at the place where the pulse is felt, the fore-arm and arm very much contused, particularly at their articulations, and the os humeri broken within 2½ inches of its upper end. Amputation was performed immediately above the fracture of the humerus. On slackening the tourniquet no blood squirted from the brachial artery, nor could any pulsation be observed at its extremity; and although no ligature, compression or styptic was applied to it, no bleeding took place during the cure.\*

In this case there was nothing in the state of the orifice of the artery or surrounding parts to prevent the flow of blood, for they were fairly cut across by incision; and I can have no doubt that the hæmorrhage was prevented by laceration and coiling up of the inner coats of the artery having been produced above the part where it was divided, by the violent elongation and contusion to which this vessel had been subjected.

Dr. Thomson had an opportunity of seeing some cases which appeared to confirm the opinion he had formed, in instances of cannon-shot wounds which occurred at the battle of Waterloo. In a dissection which he saw made by Deputy-Inspector Guthrie, of a limb cut off above the knee for mortification, in consequence of a contusion by a cannon-ball on the posterior part of the leg; the tibial and peroneal arteries had been torn across, and about two inches above this the popliteal artery was found closed in the lower part of the ham by coagulable lymph, which seemed to have proceeded from a rupture of the internal coat of the artery.

In another case, that of a man whose leg had been shot off by a cannon-ball, it was remarked, on amputating his limb above the knee, that the arteries of the thigh did not bleed, nor did any of them afterwards require to be tied.

A case similar to this also presented itself, in which the arm had been shot away close to the shoulder-joint.†

In the two cases of sudden spontaneous obstruction of the trunks of the arteries which I have related, the appearances in dissection

seemed to verify the conjecture formed by Dr. Thomson, as to the cause of that phenomenon; but, in both, from the interval which had elapsed between the occurrence of the accident and the death of the patients, and from the parts having been consequently altered and matted together, by the inflammation which had been produced, it was impossible to determine exactly what had been their original state when the cessation of the pulse took place. This, however, was satisfactorily ascertained in the following case that occurred to Dr. Abercrombie, which he some time ago pointed out to the notice of the Society. He has obligingly furnished me with notes of it, and I feel greatly indebted to him for permitting me to insert it here, as, in my opinion, it adds very materially to the value of the series of observations on the subject of which I am treating.

CASE III.—John Anderson, aged sixty-three, of regular and sober habits, and enjoying good health, was, in October 1819, much exposed to cold, after which, about the end of the month, he was affected with pain in his head and back, accompanied by shivering, but was not confined to the house. About the 6th of November he complained of a fixed pain about the top of the right thigh and groin, and after a day or two the thigh became numbed and weak, the muscular power of it being considerably impaired. On the evening of the 11th he observed swelling of the leg, first at the ankle. On the 12th the limb was swelled throughout, and was emphysematous, crackling when handled at every part. When punctured, it discharged a fetid gas, and some dark-coloured fluid. The colour of the skin was at first little changed, or rather red, but towards night it began to be livid in many places, and large vesications appeared upon its surface. He died early on the morning of the 13th, having sunk gradually, but rapidly.

On examination of the body after death, there was found a great quantity of a dark-coloured fluid effused into the limb, and the muscles universally dark and gangrenous; and behind the peritoneum, on the right side of the abdomen, there was a large gangrenous cavity, containing much dark-coloured fluid, and ill-conditioned pus. The femoral artery being laid open, there was found disease in several places of the inner coats. These were soft, and separated at various points from the outer coat, so as to lie across the area of the vessel-like valves. One of them was about a third down the thigh, and there were others less remarkable. On slitting open the external iliac artery, the inner coats were found soft and thick, and at one place completely lacerated in the whole circumference of the artery, and separated from the external coat for nearly an inch and a half, the portion thus separated having fallen down and coiled up, so as completely to obstruct the canal of the artery nearly at the place where it passes under Poupart's ligament. Between this and the origin of the internal iliac artery, there were considerable lacerations of the inner coats. In

\* Essays and Observations, Physical and Literary, vol. iii. p. 502.

† Report of Observations made in the British Military Hospitals in Belgium, &c. p. 23.

two places they were slightly detached from the outer coat, and their edges projected into the tube of the vessel. Above the obstruction at the lower part of the iliac, produced by the more extensive laceration, there was a coagulum of blood in the artery. The aorta was in several places diseased; its internal surface ulcerated, and the inner coat partially separated. In some of the smaller branches of the arteries in the pelvis there was ossification, but none in the right iliac artery, where this singular disease was situated. The left iliac artery appeared sound. No other diseased appearances were detected.

In this case death took place so rapidly, that the inflammation had not time to alter materially the appearances of the parts; and in it, it is obvious that the obstruction to the circulation was produced by the torn and detached internal coats of the artery.

Besides these three cases of the sudden obstruction of arteries, in which the state of the vessel was ascertained by anatomical examination, I shall now adduce some others, in which an opportunity of examining the parts by dissection has not occurred, but the symptoms and progress of which appear to me clearly to show, that they have been of the same nature as those to which I have already directed the attention of the Society.

CASE IV.—On the night of the 4th June 1815, a German, of middle age, a strong muscular man, and old soldier, got very drunk, and fell down a steep stair. I was sent for, and saw him very soon after the accident. I found his right elbow-joint violently sprained, and a great degree of swelling and tension in the soft parts all around it. I could not discover any fracture or dislocation, but the joint appeared to me looser than natural, as if some of the ligaments had been torn. I was surprised, on endeavouring to feel his pulse at the wrist, not to be able to discover any pulsation in the radial artery, and I could not detect any in the ulnar, nor in any part of the arteries of the fore-arm. The pulsation of the humeral artery was distinct, and could be traced to the bend of the elbow. I was enabled to examine the parts very freely, as the intoxication seemed to have rendered the patient quite unconscious of his situation, and insensible to pain. He did not complain, in the least degree, on the parts being handled or moved; and he continued occupied in giving cheers to the health of the king, frequently waving the injured arm round his head; and he remained in this state for a considerable part of the night, when he at last fell asleep. Next morning he was in a very different condition. He suffered severely from the usual symptoms which follow a debauch. He complained, in addition, of soreness in several parts of his body which had been contused; and he had violent pain in the arm, not being able to bear the slightest pressure or motion of the parts. The swelling and tension were rather increased. The absence of pulsation in the arteries continued.

He was, at this time, sent to the Royal Infirmary, where he remained till he recovered.

I accidentally was prevented from seeing him there, for more than a fortnight after his admission. At that time under the usual treatment, the swelling and pain of the elbow-joint had almost entirely subsided. On examining the arteries of the fore-arm, I could not discover any distinct pulsation in any of them; and the pulsation in the humeral artery could be distinctly felt to the fore-part of the elbow-joint, where it suddenly ceased. The fore-arm was pale, and the veins much smaller and more collapsed than those in the other arm; and when they were compressed, they filled very slowly. The sensation and motion of the fore-arm and hand were perfect. In a short time he was dismissed from the hospital cured, and I had no opportunity of afterwards seeing him; but I heard that he appeared to have got quite well, and that no defect or inconvenience had been occasioned by the injury.

In this case, there appears to me to be no way of accounting for the cessation of the pulse, except by supposing that it had been produced in the same manner as in the cases I have already related. It is impossible to conceive that the artery had been entirely torn across, as, had this happened, there must have been an extensive effusion of blood into the cellular substance of the arm and fore-arm, of which there were no symptoms; nor can we believe that the pulse was stopped by pressure from the effusion into, and consequent tension of, the parts surrounding the artery. Usually no such effect is produced; and I conceive the stoppage would not have been permanent, but that the pulsation would have returned as the swelling and tension abated, as they began to do very soon after the accident.

CASE V.—Mr. Bryant, a surgeon in Edgeware Road, has related a case of obliteration of the external iliac artery, followed by gangrene and mortification of the foot and leg, of which the following is an abstract. The patient was a lady, twenty-eight years of age, who, for five years, had been subject to occasional pains in the region of the pelvis; and, during this period, had also suffered from three or four attacks of gout in the right extremity. Ten months before the attack, now to be described, she had received a very trifling blow in the groin from the basket of a laundress, which was followed by slight temporary pain. She had likewise a little enlargement of the inguinal glands, occasioned by excessive exercise on horseback.

On the 21st January 1822, she was affected with acute pain in the right hypogastric and iliac regions, with quick and full pulse, and other symptoms of inflammatory fever. For these symptoms she was freely bled, and treated antiphlogistically. On the fifth day the pain had ceased in the iliac region, but had attacked the course of the ureter and kidney. By free local bleeding, and other appropriate remedies, this pain also was relieved, and the fever diminished; but she complained of slight uneasiness in the left groin, increased on pressure. On the evening of the 28th, seven days after the commencement of her ailments, while using the hip-bath,

she was suddenly seized with numbness, and coldness of the left extremity, succeeded by severe pain in the calf of the leg. The coldness, and the pain of the leg which extended to the ankle, continued, with great diminution of the sensation of touch. On the fourth day after the attack of numbness, petechial spots appeared on the foot, with total loss of sensation, and the calf of the leg swelled, and became exquisitely sensible. From this time the gangrene of the foot went on, the symptoms resembling very much those which occurred in the first case, related in this Essay, but the affection was more acute in its character; and, after the gangrene passed beyond the ankle, a stop to its progress took place, a separation between the dead and living parts began, the progress of the disease being attended with intense suffering, and great danger. In six weeks after the commencement of the mortification the bones of the leg were exposed, which, in a short time after, were sawn through as close to the soft parts as possible. After this the wound granulated; and, at the last report in July, was healthy, a portion of the fibula being in progress of exfoliation. Very soon after the first attack, it was observed that no pulsation could be felt in the femoral artery; and, on examination after the dead parts were removed, no pulsation could be felt in the artery, as far as the bifurcation of the common iliac, nor could any aneurismal tumour, or other unusual appearance, be traced in its course.\*

During her illness, this patient was attended by Mr. Cline, Mr. Brodie and Mr. Clarke, who, it is stated, had never seen a case exactly alike, and who were of opinion, that the obliteration had been produced by injury done to the external iliac artery. There can be little doubt, I conceive, that it has been a case of the same nature as those I have described. It illustrates the fact, that, in mortification from the obstruction of the main trunks of arteries, although the collateral circulation may not be sufficient to nourish and preserve the vitality of the whole limb, yet it may be sufficient to supply a considerable part of it below the place where the vessel is obstructed.

The next case I have to mention is one which occurred to my friend Dr. Gairdner, who observed it very carefully during its progress. Unfortunately, from accidental circumstances, the body of the subject of this case, who died a few months ago, was not examined. Dr. Gairdner intended himself to have laid this case before the Society, but, on learning that I was occupied in preparing a paper on the subject, he was induced kindly to forego his intention, and to permit me to enrich my Essay, by adding it to the others I had collected. I give it in his own words, as communicated to me in the following letter.

CASE VI.—*“Dear Sir,—I take my pen, according to promise, to put you in possession, as far as I am able, of the history of the remarkable case of arterial obstruction, which you*

*once saw with me. The subject of it was by trade a carpenter, and was upwards of thirty-nine years of age in the month of October 1824, at which date this narrative commences. In the course of that month he met with an unfortunate accident. On withdrawing from his work-shop at a late hour, he fell backwards over an outer stair-case,—a height of about twelve feet. He was pretty severely bruised about the ninth and tenth ribs of the left side, and his left humerus was dislocated; he was also very much stunned. With the assistance of Dr. Robert Hamilton, who saw him immediately after the accident, I succeeded, without much difficulty, and without the application of any great degree of force, in reducing the dislocation. The pain in the side next demanded attention, more especially as it became accompanied with cough, difficulty of breathing, and some degree of fever. Several days were consumed in vanquishing these symptoms, by the usual remedies,—venesection, purgatives, and the local application of saturnine lotions; by these the fever and pain were removed, and the cough greatly diminished, though not entirely banished. During the time that he was confined to bed after the fall, the arm which had been dislocated was, of course, carefully tied up, and I always felt his pulse at the opposite wrist; but one day I chanced to put my finger on the wrist of the left side, and discovered, to my great surprise, that there was no pulse to be felt. I examined the whole course of the radial, humeral, axillary, and subclavian arteries, as far as it was possible to trace them with the finger; there was no pulse in any part of these vessels. In the carotids the pulsation was natural, the affected arm was coldish and pale, and the veins on the back of the hand were much less distended than those of the right hand. Desirous that facts so remarkable should be witnessed by others besides myself, I submitted the patient, about this time, to the inspection of Dr. Thomson and of Dr. Robert Hamilton; and both these gentlemen satisfied themselves, by minute examination, of the accuracy of the above statements.*

*After this accident, my patient was never entirely well; he had been liable to coughs before its occurrence, but after it, he was scarcely ever without some degree of cough, which was accompanied with a little mucous expectoration: he was disturbed occasionally by palpitation at the heart, especially on going up an ascent, and was often obliged to stop short in the middle of a slight acclivity, in consequence of dyspnoea, and indescribable sensation of anxiety in the region of the heart. I never happened to see him on one of these occasions, but I have examined his heart at other times, both by auscultation and otherwise, and I never could detect any defect in its rhythm, or irregularity in its pulsations.*

*“On the 19th of March 1825, rather more than five months after the accident above mentioned, he sent for me in the evening. I found him much alarmed about what he conceived to be a paralytic affection of his right arm, in which he had been suddenly seized with a sen-*

\* Edinburgh Medical and Surgical Journal, vol. xix. p. 45.

sation of numbness and coldness. On examining the arm, I was immediately struck with the similarity of its appearance to that of the left arm formerly; it was colder than the opposite limb; the veins on the back of the hand were flaccid; a paleness of the skin indicated an imperfect circulation in the cutaneous vessels; and the pulse, as in the former case, was imperceptible at the wrist. The arm was less capable of exertion than usual; its strength was diminished, but its muscles obeyed the will as readily and with as much precision as at other times; and though he complained that he felt as if it were benumbed, yet I ascertained that the sense of touch was sufficiently acute for all useful purposes: there was no pulsation in any part of the arteries of the fore-arm, nor in the lower part of the humeral artery; but the subclavian and axillary arteries were found to pulsate freely. Pulsation was also felt in the very beginning of the humeral artery, but none whatever in the rest of its course. Having ascertained the exact spot where the artery was interrupted, I made, with the point of my finger, a little pressure on it, which gave him a slight sensation of pain; the same degree of pressure applied one finger's breadth higher up, or lower down, produced no feeling of this description. I have therefore no doubt, that, at the obstructed part, there was a disease or laceration of the inner coats of the artery; but, although I questioned my patient most minutely on the subject, I could not discover that any blow or bruise on the part, or any sprain of the arm, or even any unwonted exertion, had preceded those symptoms which had no suddenly alarmed him. The left arm had by this time recovered its customary power and warmth; and the colour of the skin, and the degree of distention of the veins were indicative of increased freedom of the circulation. No distinct pulsation could, however, be felt in the great arteries of the limb. A very feeble pulse, indeed, could occasionally, be discovered; but it was so very slight, that I should have had much difficulty in numbering the pulsations of the heart by means of it.

"In the months of April and May, he was much distressed with his pectoral symptoms, and with the palpitations and inability for exertion: he became much better in June, and continued so till the beginning of November. Upon the 10th of that month I was called to him, and found him labouring under severe catarrhal symptoms, fever, cough and dyspnoea, mucous *râle*, and copious viscid expectoration, sometimes a little streaked with blood. In the course of a week, these symptoms increased to an alarming degree; the urine, at the same time, fell short, and the legs became œdematous. It is unnecessary to enter minutely into the circumstances of the treatment; it is sufficient to say that, by moderate evacuations, together with blistering, and the use of diuretics, the symptoms, which at one time seemed almost insurmountable, were gradually overcome, and by the middle of the following month he was enabled to return to his customary occupation.

"On the 8th of April 1826 he had a sudden attack of paralysis of his left side; the muscles of the face were evidently distorted, and those of the left leg were so much paralysed, that he dragged the limb in walking across the floor—there was also, for one or two days, a degree of delirium. All these threatening symptoms, however, disappeared entirely in three or four days, and he enjoyed better health during the rest of last year, though he never became strong or free from his pectoral symptoms.

"In the early part of January last he went on a journey of about 60 miles in very severe weather, notwithstanding the repeated warnings which I had given him of the injury that might arise to his impaired constitution from exposure to cold. In the very night after he arrived at the end of his journey, he was seized with an asthmatic attack, similar to that from which he had suffered in 1825. It was attended from the first with bloody sputum, and the quantity of blood increased rapidly as the disease proceeded. In the course of a few days the dyspnoea increased very much, notwithstanding repeated blistering, and other remedies employed to arrest it; the legs became œdematous, he rapidly lost strength, and died on the 15th of January at Dunkeld, which was the scene of his last illness. Immediately on hearing of the fatal termination of his disease, I wrote to Mr. Fletcher, who had attended him there, requesting him to ascertain the state of the heart and arteries by dissection, and giving him a detailed statement of my reasons for this request. To this letter I received a very polite reply, in which Mr. Fletcher expressed his regret that my communication had arrived too late to be of any service.

"I am very sorry that this important case should be thus left incomplete in its details. I am sensible that this imperfection takes much from its value; but I hope it will appear to you of sufficient interest to deserve a place in the series of cases which you propose to communicate to the Medico-Chirurgical Society. I am, my dear Turner, yours very faithfully,

"JOHN GAIRDNER.

"18, Hill Street, 15th Feb. 1827."

To these cases I may add one published by Mr. J. Marshall of Leeds, so far back as the year 1813, as there appears to me every reason to believe, from its history, that it has been of the same nature with those I have described.

CASE VII.—An old worn out quarter-master of a ship was attacked by retention of urine, in consequence of stricture. The retention relieved itself after the use of an opiate, a bougie was passed, and the patient passed a tolerably easy night, but on waking in the morning he found the use of the right arm entirely gone. On examination, the fore-arm was found benumbed, cold and stiff, and no pulsation could be perceived in any part of the brachial artery, from the subclavian downwards. On the third day the fingers became livid, and the hand discoloured, and pain ensued in the fore-arm, the lower part of which also became livid. On the 15th day a line of demarcation showed it-

self between the dead and living parts, a little below the elbow, and amputation was then performed above the joint. Although the patient was in a most unpromising state at the time of the operation, the wound healed chiefly by the first intention, and was cicatrized in a month, and he ultimately completely recovered.\*

After a careful perusal of three cases, related by Dr. Storrer, in a paper entitled, "An instance of the entire want of Pulsation in the Arteries of Paralytic Limbs," I am disposed to believe that they also are to be regarded as of the same nature as those which have been described.†

CASE VIII.—The first of these, which came under Dr. Storrer's own observation, was that of a lady about fifty years of age, who was attacked with fever of a mild kind, which in a few days put on a remittent form, and became attended with marks of determination to the lungs, the symptoms resembling, in a very remarkable degree, those which occurred in Case I. By bleeding and antiphlogistic treatment, these complaints abated, and in the third week of her illness the patient seemed to be gradually improving.

At this period, however, after a disturbed night, she was suddenly affected with what is described as a paralytic affection of the whole left side of the body, but without any corresponding affection of the face or speech, or any disturbance of intellect. This was accompanied by excruciating pain in the shoulder, extending to the elbow, soon followed by violent pains in the calf of the leg and foot. She had some capacity of moving the shoulder, but scarcely any of moving the fore-arm and hand; the thigh and leg were in the paralytic state, possessing a very obscure degree of sensation and motion, and the foot and hand were insensible even to the prick of a needle. No pulsation could be felt in the fore-arm or arm, but the subclavian artery was observed to beat distinctly; and Dr. Storrer was unable to perceive any trace of pulsation in the groin or in the ham. During the ensuing four days, the powers of sensation and motion in both extremities were gradually very considerably restored. The pulse did not return, and it diminished in force, so as hardly to be perceptible in the portion of the subclavian artery, in which it was at first distinct. On the fifth day she became affected with uneasy feelings, and attacks of suffocating sensations in her chest, with hurried respiration, and died suddenly during the night. Unfortunately an examination of the body after death was not permitted.

Dr. Storrer regarded this case as one of hemiplegia, but was much at a loss to account for the absence of pulsation in the arteries. He conceived that this could not depend on obstruction in their tubes, from the improbability

of this occurring in both limbs, and from the absence of any external marks of the cause of such obstruction, and also from the gradual cessation of the pulse in the subclavian artery; and he suggests the hypothesis that it might depend on a palsy having been produced in the coats of the arteries, which he states would completely explain all the phenomena.

Now, even supposing that the coats of the arteries were paralysed, it appears to me that this would not give rise to a cessation of the pulse, for the blood would be still propelled into the pervious arteries, and would have an impulse communicated to it on each contraction of the heart. I can have no doubt that, in this case, a sudden obstruction had taken place in the arterial tubes in the axilla, and above the groin; and that the paralytic symptoms were the consequence, and not the cause, of the interruption of the circulation. This supposition seems consistent with all the symptoms; and would also explain the disappearance of the pulse in the subclavian artery; for a coagulum might be expected to extend into it, to the first collateral branch above an obliteration of the artery at the upper part of the axilla.

Dr. Storrer next relates a case, which was communicated to him by Mr. Vickers, surgeon at Loughborough.

CASE IX.—In a man about sixty years of age, apparently in good health, while taking his breakfast, the hand which held the teacup dropped lifeless on the table. He felt no other ailment. In two hours after, Mr. V. found the arm, it is stated, completely paralytic, and he could feel no pulse in it, while the pulse in the other arm was natural in strength and frequency. The arm was cold, and its colour pale; but it soon grew dusky, and the dingy appearance increased hourly. About four hours after the attack, the patient's constitution became affected, symptoms of sinking appeared, and he died six hours after the seizure. The state of the parts was not examined after death.

The third case is one which was brought to the recollection of Dr. Wells, by the reading of the two already related. It had been mentioned to him by Sir G. Fordyce, who had attended the patient, along with Dr. Sequeira. Dr. Wells communicated the particulars, which he obtained from the latter gentleman, to the Society.

CASE X.—A gentleman of sixty-two, in jumping over a puddle of water with tight boots on, suffered a disagreeable jar, and was slightly stunned at the moment. Next day he was attacked with a pain in the right foot, and in about a week after with a pain and swelling of his leg; and at the same time suffered from cough and difficulty of breathing, to which he was liable in winter. While things were in this state, he awoke one morning with excruciating pain in his left arm; and, in the afternoon, it became numb and motionless, and no pulse could be felt in it. He had no symptom of affection of the head, and his pulse in the other arm was natural. About

\* Edinburgh Medical and Surgical Journal, vol. ix. p. 449.

† Transactions of a Society for the Improvement of Medical and Surgical Knowledge, vol. iii.

the same time the pain and swelling of the leg disappeared. No change took place during the subsequent two days; but on the morning of the third, while making an exertion, he was seized with a convulsion or fainting fit, it seems uncertain which, and in a few minutes expired.

In this case, it is stated that the affected arm was examined next day by an eminent anatomist, who was unable to discover any organic disease in its blood-vessels. It is mentioned that the relations would not permit the head, thorax, or abdomen, to be opened; and it does not appear that the brachial artery was traced to its origin, so that an obstruction above the clavicle may have escaped detection.

The late Dr. Parry of Bath notices three instances which he had met with, in which the pulse ceased suddenly, in one arm, while it remained distinct in the other.\*

**CASE XI.**—In the first, this took place two or three days after parturition, and was attended with coldness of the arm; but the power of motion remained. The other arm had lost all power of voluntary motion, but the pulse was distinct in it. The patient soon died, but a dissection was not obtained.

**CASE XII.**—The second was the case of a young man, affected with pulmonary hectic, who was found to have lost the pulse in one wrist, immediately after coming out of the warm-bath. Several months afterwards it had returned, though in an almost imperceptible degree.

**CASE XIII.**—In the third case, a middle aged female had been affected with a severe cough, from which she was convalescent, and was walking about her house, when it was discovered that the pulse in one arm was wholly wanting. A few days afterwards she died suddenly.

In the first two of these cases, it may be supposed that the cessation of the pulse depended on a sudden obstruction of the artery, by rupture of the coats; but the third case is more difficult to explain, for it is stated that the whole course of the artery to the aorta was carefully examined after death, but no deviation from the natural state could be perceived in it. The particulars of the dissection are not related; and supposing that the artery had been only examined externally, it is possible that the morbid state I have described might have escaped observation. If the canal of the artery was actually pervious at the time of examination, some other cause of the stoppage of the pulse must be sought. I have already stated my reasons for believing that this could not be produced by a paralysis of the artery; and the only other supposition appears to be, that a spasmodic contraction had taken place in some part of its course, to which Dr. Parry seems inclined to attribute the absence of the pulse in all these cases. On making experiments on animals, it has been observed by Dr. Parry and others, that if a portion of the tube of an artery be exposed, a decided contraction sometimes takes place in it. It is possible that

a similar contraction may take place in the human body, in certain circumstances, in an artery in its natural situation, but it must obviously occur extremely rarely; and notwithstanding this case by Dr. Parry, I conceive that it still requires to be established, that such a state of the vessel ever actually has been produced.

It is a question worthy of consideration, What is the cause of the sudden spontaneous rupture of the internal coats of the arteries? In cases where great violence has been suffered, as in Case IV, or in the reduction of a dislocation, as in the first stoppage in Case VII, it may be conceived that the inner coats of the artery, even in the healthy state, may have given way, without the infliction of an external wound. Richerand has stated, that, in the dead body, if the knee-joint be forcibly extended, till the ligament of the ham crack, and the limb be afterwards dissected, it will be found that fissures are formed in the middle coat of the artery, from the separation of its fibres from each other.\* But in most of the cases of the affection which I have described, it seems necessary to suppose predisposition in the coats of the arteries to laceration, as this took place on the ordinary motions of the limb, which, in their healthy state, the arteries usually permit without injury, and as these obstructions occurred in different arteries of the same individual. In the third case, there was obviously a tendency to disease in the internal coats in general; and in the parts where the lacerations occurred, they were found thickened, soft, and pulpy,—a state resembling that which is known to be produced by inflammation in these coats. But whether this state preceded, and was, as I believe, the cause of the laceration, or was the result of incipient inflammation, succeeding to that laceration, must be regarded as doubtful. In the other cases in which the bodies were examined, there was nothing remarkable, or apparently diseased, in the arteries examined, except at the parts where the obliterations had taken place. It must, therefore, at present be regarded as undetermined on what the tendency to laceration depends. In several of the cases I have related, a considerable degree of constitutional affection and fever preceded and accompanied the obstruction of the arteries. Is this to be regarded as connected with, or dependent on, the state of the vessels, which makes them liable to the laceration?

As to the practical results to be deduced from the account of this disease which has been given, there is little which can be said. The constitutional symptoms which have preceded the attack in several of the cases, appear to have had an inflammatory character; and supposing that they may depend on a certain state of inflammation in the inner coats of the arteries, which by some pathologist has been regarded as the cause of a peculiar fever, and that this could be ascertained by the

\* On the Arterial Pulse, p. 139.

\* *Nosographie Chirurgicale*, t. 4, p. 197, 5th ed.

symptoms; the antiphlogistic plan of treatment appears to be that which holds out the greatest prospect of interrupting the progress of the disease. But it must be remarked, that, in two of the cases I have related, in which the constitutional symptoms were most distinctly marked, the first and the sixth, the antiphlogistic treatment was actively employed from the commencement of the illness, yet it had not the effect of preventing the obliteration of the arteries.

When the obstruction actually occurs, the lower part of the limb may be supplied with blood by anastomosis, as happened in several of the instances I have related; as in the obstruction of the lower part of the popliteal artery, and of the brachial artery in the first case, and in that of the brachial in the second; in the same artery in the fourth; and in the subclavian and axillary artery in the sixth and twelfth. In such cases, however, there will always be reason, I conceive, from the result of the cases I have related, to apprehend the occurrence of a similar affection in some other vessel, or that the patient may die suddenly from causes which are as yet involved in obscurity.

When the larger trunks of the arteries, where they pass from the trunk of the body to the limbs, become suddenly obliterated, there is reason to fear the occurrence of mortification of the limb, as happened in five of the cases I have detailed. In the first indeed, the mortification was produced by the obstruction of the upper part of the popliteal artery,—a situation in which it might be conceived that the lower part of the limb would, without difficulty, be supplied by the collateral circulation. But in this case, it must be remembered, that this obstruction took place at a period when the patient was much debilitated by his previous illness; and when the lower part of the popliteal artery was already obliterated, which would create a considerable impediment to the supply of the distant part of the limb by the anastomosing vessels.

When the obstruction has actually taken place in an artery, the treatment to be adopted is obviously that which is found to be most advantageous after the ligature of the vessels in operation. The collateral circulation is to be promoted by placing the limb in a proper position, and by maintaining its temperature by wrapping it in flannel, and by artificial heat placed at some distance from it. I should conceive it necessary to be cautious not to be induced by the pain which may exist in the limb, to apply any considerable heat or cold to it, or to have recourse to stimulant applications, for these seem to be liable to excite gangrene in a part in which the circulation is stopped or much diminished.

When mortification takes place, the principal objects of the practitioner appear to me to be, to support the strength of the patient by nourishing diet and stimuli, in such quantity, and of such quality, as have not the effect of increasing the fever, or aggravating the local inflammation, and the proper exhibition of

anodynes for the alleviation of the pain, while attention is paid to keep the bowels regular, without purging. It is a point for consideration, whether amputation should be had recourse to, when the gangrene has fairly commenced, as has been recommended, and, in some cases, successfully practised, by Mr. Guthrie, in gangrene of the extremities, from gun-shot wounds of the main arteries of the limbs; or whether we should wait for the stopping of the disease, and the commencement of the separation between the dead and living parts? I may here remark, that it seems established by experience, that mortification is not so likely to ensue from the application of a ligature to an artery, by which the surrounding parts are little disturbed, as from a gun-shot wound of a similar blood-vessel; and that, when gangrene is produced by the first of these causes, it does not so uniformly spread till it terminates fatally, as, according to Mr. Guthrie's observation, that does which is produced by the injury of an artery by gun-shot. There are various cases recorded of gangrene from the ligature of arteries having stopped, and separation taken place, between the dead and living parts, at a considerable distance below the point where the artery has been tied.

The greater frequency of gangrene, and its more malignant character, after a gun-shot wound of an artery, than after its obstruction by ligature, may depend, as Mr. Guthrie has suggested, on the neighbouring vein or nerve, being generally also injured in the former case; but it appears to me, that they may also, in some measure, depend on the swelling and tension produced by the inflammation excited by the wound, impeding the circulation of the blood through the anastomosing vessels, to the lower part of the limb. In the spontaneous obstruction of the arteries, I should conceive that the probably diseased state of the arterial system, and the affection of the constitution, would be likely to render the success of the operation more doubtful than in a person in whom the gangrene has been produced by a wound, and that the surgeon will be warranted in waiting till the mortification shows a tendency to stop. In two of the cases I have related, the gangrene stopped, and the patients recovered; the one, by the bones having been sawn through at the line of separation; the other, by amputation above this line. In the other three cases in which gangrene took place, I do not conceive that amputation was admissible, or held out any prospect of advantage.

The rupture of the internal coats of an artery is, however, by no means always followed by the obliteration of the vessel. It perhaps more frequently gives rise to aneurism. After a violent exertion, or a strain, the patient sometimes feels an acute pain in some part of the course of an artery; in a short time a tumour is discovered, which communicates the sensation of pulsation, at first small, but which gradually increases in size, and follows the progress of aneurism. Here the internal coats have been torn, and the external is dilated by

the impetus of the blood; the canal of the artery above and below remaining pervious. The external coat is gradually extended, till at last it in some cases gives way, and the blood is effused into the neighbouring parts, forming a diffused aneurism, succeeding to a circumscribed; in other cases, the dilated external coat becomes adherent to, and incorporated with the surrounding parts, it is absorbed, and the sac of the aneurism, as it increases, is formed by these parts matted together by the adhesive inflammation. In cases of aneurism thus produced, there is every reason to believe that the inner coats of the artery have usually been in the state of disease, in which they become thickened and tuberculated, and sometimes ossified, and have in consequence been in a considerable degree deprived of their elasticity and flexibility,—a state in which they are readily broken, and seem to be rendered much less susceptible than in the healthy condition, of taking on the adhesive inflammation. It is perhaps by these circumstances we are to explain why the artery remains pervious, after the laceration, instead of becoming obliterated as in the cases I have related. In some cases of aneurism, however, it has been found that the inferior aperture of the artery communicating with the sac has been obliterated, but it is not possible to determine whether this had taken place at the commencement, or in the progress of the disease. In the first case of obliteration of the arteries I have given, a dilatation of the external coat, resembling a small aneurism filled with a coagulum, was discovered, on dissection, in one of the arteries; but its progress appears to have been stopped by the speedy closure of the artery both above and below the dilatation, by the process of adhesion.

Numerous examples are recorded in pathological and medical works, of obstruction and obliteration of different arterial trunks having been discovered after death. These may, however, have been produced in various ways, besides that to which I have directed the attention of the Society. In some cases the obliteration has obviously been the result of the pressure of tumours in the arteries; in others of the spontaneous cure of aneurisms which had formed. It may be also produced by the thickening of the internal coats of an artery diminishing its canal.

In many instances in which the external diameter of an artery is not altered, or only slightly increased, we find, on cutting it up, that the inner coats are thickened, sometimes having acquired a more friable and pulpy texture than natural; sometimes being converted into a curdy matter, sometimes cartilaginous, at others with an intermixture of ossific or calcareous deposition.

Dr. Abercrombie, in a paper on apoplexy, relates an interesting case of an old woman, in whom the pulse ceased in all the extremities in succession, and for a month previous to her death, could only be felt in the carotid arteries. She was at the same time affected with violent palpitations, and severe dyspnoea. On exami-

nation after death, the whole arterial system was found extensively ossified. In some places the tubes of the arteries were considerably diminished by the disease of their coats, and several of the large arteries were obstructed by firm coagula at particular points.\* This state of the arteries is most frequently found to exist in a greater or less degree in cases of chronic mortification of the extremities. It generally extends over a considerable portion of the vessels of the affected limb, and not unfrequently over the whole arterial system, and seems to give rise to the gangrene, from the supply of blood being diminished, in consequence of the contraction of the tubes of the arteries; and partly, perhaps, from the interruption it occasions to the healthy functions of the arteries necessary for carrying on the circulation. In a case of chronic gangrene of both feet, in a middle aged man, which occurred in the Infirmary here some years ago, the arteries of the limbs were found universally diseased. In one thigh the vessels, though not increased externally in diameter, were found to have their middle and inner coats thickened to so great a degree, as almost entirely to obliterate the tubes of the vessels. In some parts these coats were soft, easily torn, and separated from each other; in other parts they formed a firm coherent mass, so that when the vessel was cut through transversely, it appeared a solid fleshy cylinder, with a small point in its centre, indicating the remains of the canal, into which it was not possible to pass an ordinary probe. The arteries of the other thigh, and of the legs, were so completely encrusted with osseous matter, as almost to form inflexible bony tubes. The parts illustrating this case are preserved in the Museum of the College of Surgeons.

This alteration of the coats of the arteries may be confined to a small space, and produce there the obstruction of the vessel, and the consequent cessation of the pulse in the limb below. Mr. Andral of Paris mentions a case in which, during life, the pulse of the left wrist was much more feeble than that of the right, and presented frequent irregularities, and, on examination after death, he found in the interior of the brachial artery, a little above its division, an irregularly rounded tumour, of a cretaceous consistence, which had formed under the inner coat, but had torn this membrane, and obliterated about three-fourths of the canal of the artery.† In cases of this kind, it is obvious that the closure of the artery, and the diminution of the pulse, must be gradual.

Even the trunk of the aorta itself has, in some instances, been obstructed and obliterated; yet the individuals in whom this occurred had survived, and the inferior parts of the body had been nourished, by the anastomising vessels. These cases illustrate many interesting points in pathology; but, at present, I shall

\* Edinburgh Medical and Surgical Journal, vol. xv. p. 15.

† Clinique Medicale, t. i. p. 458.

only attend to them in relation to the manner in which the obstruction seems to have been produced. In two of them, a complete obstruction of the canal of the aorta had taken place immediately above its division into the iliac arteries. In one of these, accidentally discovered by Mr. Goodesson in a body, the previous history of which was unknown, a considerable portion of the aorta and of the iliac arteries was obliterated, and their canals filled by a firm fleshy substance; and their outer surface adhered firmly to the neighbouring veins, and surrounding parts. The artery above was diseased. It is not easy to determine, from the appearances described, whether the obstruction arose from disease and thickening of the coats of the artery, or was the remains of an aneurism, which had been cured spontaneously, or had been produced by the rupture of the internal coats.\* In the other case, which is related by Dr. Monro, the obliteration was obviously the consequence of the progress of the spontaneous cure of an aneurism, the tumour of which still remained about the size of a common orange.†

But still more remarkable are those cases of obstruction of the aorta in which it has taken place in the thoracic portion. Three instances have been recorded in which this has occurred immediately beyond the arch of that vessel. In one of these, related by Sir Astley Cooper, a stricture was discovered in this part of the aorta, which, with difficulty, admitted the little finger, and was found to be produced by a thickening of the circular fibrous structure of the vessel, accompanied with some ossification of its coats. In this case, the contraction may be supposed to have arisen from the gradual progress of disease in the coats of the artery.‡ In the other two cases, it is more difficult to assign a cause for the obstruction; and it does not seem possible to explain it by any of the processes which I have noticed as giving rise to the obliteration of arteries. In the case described by M. Paris, the cylinder of the aorta, for a small space, was contracted to the size of a common quill, so that, when allowance was made for the thickness of the coats, which was not diminished, the canal that remained was exceedingly small. Above and below, the vessel was of its natural size; and M. Paris mentions, that, on the most careful dissection, he could not discover any cause for this extraordinary state.§ Dr. Graham has given a very interesting account of a case of this contraction of the thoracic aorta, which occurred in a boy of fourteen years of age, who was under his care. In this instance, the artery was completely impervious, for the space of about a line, the appearance being exactly such as if a ligature had been tied tightly round

it. Between the stricture and the branches given off to the head and superior extremities, the aorta was preternaturally contracted, but nearer to the heart it was unusually expanded. Below the stricture it resumed its natural size. The coats were not thickened nor diseased.\* In these cases, it is obvious that there was no tumour pressing on the artery, no appearance of remains of aneurism, nor any thickening of the coats, by which to account for the production of the obstruction. Neither can it be supposed that the obstruction was occasioned by the laceration of the internal coats of the vessel, for the appearances were not such as to indicate this; and, besides, it is almost impossible to conceive that the interruption to the passage of the blood could have been produced suddenly, without an immediate or very speedy fatal consequence.

It is true that the abdominal aorta may be suddenly obstructed without any immediate very violent injurious effects, and that the inferior parts of the body may be afterwards supplied with blood. In investigating the effects of the application of ligatures to the larger arteries, Sir A. Cooper, whose active zeal in the study and improvement of surgery has not been abated either by the labours, the gains, or the gratified ambition, attendant upon having attained the highest place in his profession, made the experiment of tying the abdominal aorta in dogs, and he found that no immediate injurious effects were produced in the functions of the animal, that it maintained its usual health, and that the hinder extremities were freely supplied with blood by anastomosis.† Guided and encouraged by his knowledge of the wonderful resources of nature in compensating for the obstruction of the supply of blood through the ordinary channels in the human body, and by the results of his experiments in the lower animals, he was, some years afterwards, induced to apply a ligature to the abdominal aorta in the human subject, as affording the only hope of safety to a patient who was in immediate danger of perishing from hemorrhage, from a large aneurism of the left iliac artery, which had given way. He accordingly performed this operation with the dexterity and coolness for which he is so eminent.

This operation, perhaps the boldest ever undertaken, was unfortunately not successful in saving the patient, yet it did not appear productive of any injurious consequences, or to have any effect in hastening his death. On the contrary, the man lived for forty hours afterwards; the hemorrhage was restrained, and his state for some time seemed considerably improved; the process of closure of the artery had proceeded favourably; and although the aneurismal limb continued insensible, and became cold and livid, the right extremity had

\* Dublin Hospital Reports, vol. ii.

† Edinburgh Journal of Medical Science, No. iv. p. 362.

‡ Surgical Essays, Part i. p. 103.

§ Journal de Chirurgie. Par M. Desault. tom. ii. p. 107.

\* Medico-Chirurgical Transactions, vol. v. p. 287.

† Medico-Chirurgical Transactions, vol. ii. p. 260.

recovered its natural temperature, and its sensibility, which had been at first impaired, was returning, indicating the restoration of the circulation in it.\*

Although, from the facts just stated, it may be concluded that the abdominal aorta may be suddenly obstructed, and the circulation continued by other channels, yet they can by no means be considered as rendering it probable that the circulation could be abruptly interrupted through the aorta, so near to the heart as the site of the contractions in the cases which have been related, without rapidly producing fatal consequences.

Dr. Graham, in his remarks on the case to which I have referred, has expressed an opinion that in it the blood had not been but recently diverted from the natural channel. Notwithstanding the arguments in support of this opinion which he has adduced, I am disposed, from a consideration of the history of this case, and the absence of all appearance of recent disease or change in the contracted part of the artery, to doubt its correctness, and rather to adopt the first impression made on Dr. Graham by the inspection of the parts, and by "the limber and healthy appearance of the coats at the stricture," that the affection was connate, or at least that it must have occurred at a very early period of life. He has judiciously directed our notice to the identity in the site of the stricture in the different cases of contraction of the aorta within the thorax which have been observed, "lest after examples should prove a peculiar tendency towards its formation in this portion of the vessel, definite portions of continuous and similar structure being, in many instances, liable to particular diseases." An attention to this circumstance may, it appears to me, perhaps throw some light on the origin of these obstructions. In all the cases, it is remarkable that the stricture was situated exactly in that part of the aorta into which the ductus arteriosus enters; and it may be conceived that a similar tendency to obliteration to that which exists in this vessel, from a peculiarity of structure, or some cause as yet unknown, may have extended beyond its orifice into the neighbouring portion of the coats of the aorta, and given rise to the contraction of this vessel at the period of birth, or at the same time with that of the ductus arteriosus.

I may remark, that these cases of obstruction of the aorta, illustrate in a very striking manner a curious fact with regard to the supply of blood by anastomosis, and prove that the dilatation of the communicating vessels is not merely the result of their mechanical distention, by the increased impetus of the blood thrown into them. It is obvious that the blood would be sent with a greatly increased force into all the vessels between the seat of the stricture and the heart; but it will be observed, from the account of the dissections in these cases, that the only branches of the arteries

which had become enlarged, were those useful in conveying blood towards the parts from which the supply of that fluid had been cut off. The subclavian arteries, and their branches, were much increased in size, while the carotid arteries remained of their natural dimensions.

I have now to conclude this paper, which has extended to a much greater length than I expected or intended. My object has been to give a connected view of the disease to which I have directed the attention of the Society, and of its relation to some other affections of the arteries, to which it bears analogy. It is sufficiently obvious how much any value the essay may possess in this respect, is derived from the contributions of my professional friends, and from the observations of others already published, which I have collected together from scattered sources. From the number of cases I have been able to collect, which have been observed within a few years, I am disposed to believe that the sudden spontaneous obstruction of the arteries is a disease of not unfrequent occurrence. It appears not improbable, that, on carefully looking back on cases which are recorded in medical writings and journals, particularly those of gangrene, others might be found in addition to those I have adduced, which might be referred to the same cause. I have no doubt, that, when the existence and symptoms of the disease become generally known to the profession, instances of it will recur to the memory of practitioners; and, at all events, when such cases occur in future, their real nature will be better understood, and we will thus obtain a more accurate knowledge of the pathology and progress of this morbid affection than we as yet possess.\*

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\* Since the preceding paper was sent to the press, I have met with an account of an instance of cessation of the pulse still more remarkable than any of those I have noticed, which is given in a very interesting essay on diseases of the heart, by Mr. Adams of Dublin. The following is a short abstract of this case:—A medical gentleman, æt. 68, suddenly discovered that his pulse was imperceptible in the right arm, and unaccountably weak in the left. Next day no pulsation could be detected in any artery in the body; nor was the movement of the heart sensible to the hand laid over the breast. An obscure undulating motion could alone be heard, when the ear was attentively applied to the side of the thorax. He suffered much from dyspnœa, which prevented sleep, and which was not relieved by any remedy that was tried.

He continued in this state for seven weeks, when stupor came on, and speedily terminated in death. On dissection, the semilunar valves of the aorta were found completely ossified, and the ossification extended into the coronary arteries, so as entirely to obliterate their canals for an inch. Mr. Adams attributes the symp-

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\* Surgical Essays, part. i. p. 114.

Since the article on the Spontaneous Obstruction and Effects of Laceration of the Arteries, contained in this volume, was printed, an opportunity has occurred to me of examining the popliteal arteries in two cases of severe, and rather uncommon, injury of the knee joint, in which the condyles of the femur were dislocated backward,—the integuments and soft parts of the ham had given way,—and the vessels in it had been violently elongated and lacerated. One of the cases occurred at the Royal Infirmary here, under the care of Professor Ballingall; the other was in a patient of Staff-Surgeon Stewart, Principal Medical Officer in Scotland, and Dr. Anderson, Surgeon of the 92d Regiment. I feel much indebted to these gentlemen for the obliging manner in which they have communicated the particulars of these cases, and permitted me to publish them.

The following is the statement given me by Dr. Stewart: "Private James Kennedy of the 92d regiment, a strong muscular man, twenty-four years of age, having been confined to barracks, and not allowed to go into town, on the 8th of April, made an attempt to force his way out of the castle, by leaping out at an embrasure on the southeast face of the fortification, a height of about thirty feet. The fall caused a compound dislocation of the left knee joint. When he was brought into hospital, the condyles of the femur were observed to be thrust through the muscles and integuments of the ham down over the head of the tibia. His countenance was pale and ghastly, and the whole system in a state of great depression and debility. The limb was quite cold, and no pulsation could be felt in the popliteal artery when the finger was placed over its course between the condyles; and the idea was, that the vessel had been ruptured. From the very extensive injury done to the knee joint, it was too evident that there was no chance whatever of saving the limb. As soon, therefore, as the system had sufficiently recovered, to allow the patient to undergo an operation with safety, which was about two hours after the receipt of the injury, Dr. Anderson, the surgeon of the regiment, amputated the limb by the circular incision. The femoral artery was the only vessel that required to be tied. The stump is nearly healed; and the patient has been altogether exempt from fever.

On examination of the knee joint, all the ligaments were found torn across, with the exception of the internal lateral ligament; the tendon of the popliteus muscle was also torn

from its insertion; and partially the head of the gastrocnemius externus. There was a small fracture of the head of the tibia; the joint was filled with blood; and the two internal coats of the popliteal artery were ruptured.

ALEX. STEWART.

"April 21, 1828."

Being desirous to ascertain exactly the effects produced on the artery, by the violent elongation which it must have suffered, I inspected it carefully along with Dr. Knox, the Conservator of the Museum of the College of Surgeons. The outer surface of the vessel was found entire throughout its whole course in the ham; but, opposite to the projecting condyles of the femur, the artery was, for a third of an inch, suddenly constricted to about one-third of its diameter above and below. About half an inch above this constriction, the inner coats of the artery terminated abruptly in its whole circumference, in a slightly ragged edge. Between this edge and the constricted part, the internal surface of the outer coat was exposed, somewhat rough, having adhering to it some fragments of the inner coats, and ecchymosed. At the seat of the stricture, the canal of the artery was impervious; and, on slitting it up, the obstruction was found to be produced by flaps of the inner coats separated from the external, and irregularly coiled up in the vessel. At this part the external coat was thin, and its fibres drawn together, so that it could not be expanded to its usual breadth. The popliteal vein and nerve were entire.

The particulars of the other case are contained in the following letter, with which I have been favoured by Dr. Ballingall:

"My Dear Sir—You are most welcome to make use of the preparation of ruptured popliteal artery, which I deposited in the Museum of the College of Surgeons some time ago, if it can in any way illustrate your interesting and valuable paper, on the Sudden Spontaneous Obstruction of the Canals of the Larger Arteries.

"The patient from which it was taken, a middle aged female, was brought into the Royal Infirmary with a deep lacerated wound in the ham, through which it was stated, in a written report from a professional gentleman, that the condyles of the femur had protruded, and that the knee joint had been completely dislocated. The injury had been produced by a fall, which this poor woman had suffered while carrying a heavy load of coals on her back. There was no hemorrhage from the wound; but a good deal of swelling, and inflammation of the contiguous parts, extending up the whole thigh, had supervened, and was attended with much constitutional fever, which rapidly increased, and carried off the patient ten days after the accident.

"On the dissection of the limb, although

toms, in this case, with great probability, to diminution of the muscular power of the heart, arising from the deficient supply of blood to its substance, combined with the mechanical obstruction to the passage of the blood into the aorta, occasioned by the ossification of its valves.—*Dublin Hospital Reports*, vol. iv. p. 443.

the crucial ligaments of the joint were torn, the capsular ligament was found to be entire. The blood-vessels of the ham were torn through; but as I made but a cursory examination of the parts at the time of the occurrence, and have not examined them more minutely since, you are probably better acquainted with the state of the artery than I am. I remain, &c.

"GEORGE BALLINGALL.

"Queen Street, 16th April, 1828."

Availing myself of Dr. Ballingall's permission, with the assistance of Dr. Knox, I examined minutely the state of the parts which are preserved; and the following are the appearances which presented themselves.

The large vessels of the ham are completely torn through, and their extremities, about two inches apart, are each surrounded by a bulb of very dense cellular substance, intermixed with fat and lymph, which adheres firmly to their coats, and cuts like cartilage. The vein near its torn orifices is contracted, and its coats thickened by interstitial deposition, and the tube of both the superior and inferior portion is filled by coagulum for about two inches. The coagulum is friable, and, internally, of a brown colour, apparently showing that it consists of coagulated blood. The clots fill the canal of the vein, but are without difficulty detached from its surface, except where some branches enter, also filled with coagulum, continuous with that within the main trunk. About half an inch from each orifice a valve is seen; and, opposite these valves, the tube of the vein is slightly swollen out, and the coagulum somewhat larger than in the other parts. The portion of the popliteal artery above the rupture, about four inches of which have been preserved, is diminished in size, and, for an inch from its extremity, adhered firmly to the surrounding cellular substance, and to the vein. The orifice was so completely covered by, and incorporated with, the condensed cellular substance, that it was not possible to distinguish its situation. On slitting up the artery, it was found to terminate half an inch higher than the extremity of the vein; and its canal contracted and obstructed for more than two inches. For half an inch above its extremity no remains of the two inner coats can be perceived; and the inner surfaces of the external coat appear to have adhered to each other. Above this, for about an inch and a half, the circular rugated appearance of the inner coats is distinct, and the canal is filled by a tough white coagulum, which adheres very firmly to the coats of the artery, particularly at various points, where it appears to send processes into interstices of these membranes, from which it is impossible to separate it without laceration. Above the coagulum, the tube of the artery is pervious but narrow; and, for some distance, the transverse lines on its inner surface are more strongly marked than in healthy artery. The orifice of the lower portion of the artery

was found contracted, and adherent to the surrounding lymph and cellular substance, which, in fact, closed it. On slitting it up, its canal was found larger than that of the superior portion; its coats are thickened, its outer surface adhering to the surrounding parts; and its inner surface marked with strong circular rugæ. It does not contain any coagulum. It divides, about half an inch from its orifice, into two large branches. The popliteal nerve is entire; and, in its substance, there is seen a tortuous arterial branch, almost equal in size to the temporal artery.

I have been induced to detail minutely the appearances of the arteries in these two cases, as there are but very few instances in which the changes produced in lacerated arteries of the human body have been described, and opportunities of observing them are rather uncommon; and because they appear to me to illustrate very distinctly the effects produced on the coats of the vessels at different stages of the progress of the injury.

In the first case, the artery may be regarded as in the act of laceration; and a very slight additional elongation must have torn it through. In it, the laceration of the internal coats, and the effect of this in immediately obstructing the tube of the artery, are distinctly demonstrated. The appearances also confirm, in a remarkable manner, the description given by Beclard, of the steps in the process of laceration, as quoted in my essay, "*tandis que la tunique celluleuse continue à s'allonger, en se rapprochant de plus en plus de l'axe du vaisseau,*" &c.

In the second case, the time that had elapsed, and the inflammation that had taken place, between the occurrence of the accident and the death of the patient, render it impossible to ascertain exactly the effects which had at first been produced on the vessel; but the absence of the internal coats for half an inch from the torn orifice, and the firm adhesion of the coagulum to their surfaces above, seem sufficient to indicate the injury they must have sustained.

I may here mention, that in writing the Essay read to the Society, it escaped my recollection, that Mr. Hodgson, in his Treatise on the Diseases of the Arteries and Veins, has noticed the fact of the laceration of the inner coats of torn arteries, as ascertained by Dr. Jones in his experiment; and that he has also pointed out the constriction, sometimes observed in the extremities of arteries torn across, afterwards more particularly described by Beclard, of which he has given an illustration, by an interesting case of lacerated humeral artery, quoted from a paper published by Mr. Guthrie.\* I gladly avail myself of the present opportunity to repair this omission, in relation to a work which derives so much value from the full and accurate account it contains of

\* Hodgson, p. 461.

every thing important to be found in the writings of preceding authors, with regard to the subjects of which it treats.

From the London Medical Gazette.

## VACCINATION.

NO. IV.

*To the Editor of the London Medical Gazette.*

SIR,—The object of my last communication was to show, first, the incorrectness of that theory which would attribute the failures of vaccination to deterioration of the virus by successive inoculations; and secondly, the futility of that advice which would send us back to the cow for fresh sources of lymph, on the occurrence of any fresh sources of alarm. By a singular coincidence, the same number\* of your Journal contained a brief notice of some recent experiments in Egypt, tending to show, first, that the cow was susceptible of the small-pox; secondly, that this animal converted the small-pox into the cow-pox; thirdly, that this converted disease was communicable to man; and fourthly, that from man it might be propagated without degenerating into small-pox. You remarked, with great justness, that these facts, if true, would prove of the utmost importance. They would set at rest that long disputed and curious question—whether the cow-pox is, or is not, a modification of variola. They would go far to determine the identity of the cow-pox, and the grease of horses; but above all, they would ensure to us, at all times, the means of combating the small-pox, since that baneful contagion might be made, on any emergency, to furnish its own antidote.

The respectability of the source whence this information was derived, precluded all doubt as to its authenticity; though I could not but consider it as singular, that facts of this importance should have been first made out by medical gentlemen in Egypt; and that, being so important, they should have first found their way to this country by the very circuitous route of India. The idea, however, once started, it became very desirable to set the matter at rest, more especially as several circumstances were omitted in the statement communicated to Dr. M'Michael,—viz. first, whether the cow, in converting the small-pox into cow-pox, underwent any constitutional indisposition; secondly, whether the same animal was susceptible of the disease more than once; thirdly, whether the local appearances in the cow resembled those described by Jenner, as appertaining to his cow-pox; and lastly, where, when, and how, the inoculation of the animal might be most successfully performed.

To determine these curious points Dr. Nay-

lor and Mr. Mayo made the experiment upon an Alderney cow at Paddington; while Mr. Sewell, of the Veterinary College, anticipating very ingeniously an objection, that possibly this cow might have already passed through the cow-pox, and been thereby rendered unsusceptible, made (with the assistance of Mr. W. Wheeler) a similar experiment upon a fine calf, at the Small-Pox Hospital. Mr. Sewell, anxious that no means should be left untried of improving our knowledge of the connexion of human and epizootic maladies, took the same opportunity of inoculating, with fluid small-pox matter, two lambs, and some rabbits. In none of these instances did the inoculation take effect. Conceiving that possibly this disappointment might have arisen from accidental circumstances, and understanding from Dr. Paterson (late of Ayr) that he had succeeded, in various instances, in communicating the small-pox to a cow, while in milk, Mr. Coleman, zealous in the same cause, has since inoculated another cow and an ass, with variolous matter furnished by me. Mr. Alcock assisted at this experiment. Speculating upon the possible causes of the former failures, I was led to suspect that they might depend upon the animal's constitution being unsusceptible of a truly human poison. To determine, if possible, this point, I vaccinated, on Friday last (in presence of Mr. W. Wheeler,) the calf (the subject of the prior experiment,) with lymph, descended in an uninterrupted stream from that which was originally supplied to us from the cows of Mr. Harrison's dairy in 1799. The results of the former experiment cannot yet be known; but I may add, that the vaccination has taken no effect upon the calf beyond that of a common irritant.

Whatever conclusions these experiments may lead to, with respect to the pathology and mutual relations of cow-pox and small-pox, one thing at least appears to be certain, viz. that no reasonable hope can be entertained of procuring, by this means, a supply of vaccine matter in any district or country into which small-pox may have accidentally found entrance. Though I am far from thinking that these experiments have hitherto been carried to their farthest reasonable limit, yet enough has been done to show that the communication of small-pox to the cow is both difficult and uncertain; and, therefore, that other means must be resorted to for keeping up the supply of vaccine lymph, and rendering it at all times available for the protection of the human race. The difficulty of effecting this has always been acknowledged; but few, except those who have turned their attention seriously to the subject, could imagine how very great that difficulty is. In the West India islands, and all other small societies, keeping up their own supply of recent lymph is impossible. No single practitioner in England, however extensive his practice, is capable of doing it. Even the public establishments in London, devoted to the task, find a difficulty in effecting it during the months of December and January, when parents, with a natural and praiseworthy

\* Vide Journal of Foreign Medicine, Vol. II. page 92.

caution, hesitate to expose their children to the keen blasts of a wintry air. It may be stated, without fear of contradiction, that such public establishments are indispensable to the preservation of vaccine lymph in this or any other country; and the question for consideration, therefore, resolves itself simply into this—how can such establishments be kept most effective?

In all (or almost all) the countries of Europe,—in India, in Ceylon, at the Cape of Good Hope,—and, I believe, in most of our other large colonies,—the supply of vaccine lymph to the public is taken under the protection of government; and when we reflect how much the attention of individuals is subdivided, no one can doubt for a moment that a wise government will never submit a matter of so much consequence to the community to the chance of neglect, or delay. In London, the liberality of the public, and the philanthropic spirit of a few individuals, have opened sources of vaccine lymph, independent of those which the government furnishes; but still it cannot be questioned, that, to the National Vaccine Establishment, this country must mainly look, for its regular and permanent supplies of vaccine lymph.

Vaccination had been known and generally practised in England ten years before it was taken under the protection of government. It was not until 1808 that the National Vaccine Establishment was formed. Within a very short time afterwards, it was placed on the footing, and under the superintendence which it now enjoys. That it has been eminently serviceable in distributing vaccine lymph to all parts of the United Kingdom, and to many distant countries; and that those connected with it have been most assiduous in their exertions to extend its usefulness, is most true. No one is more fully sensible than I am of its well-earned character; and if I throw out a doubt how far, as at present constituted, it is calculated to meet the wants of the times, it is for this very reason,—because I have seen and watched its sphere of usefulness, and wish to see that sphere extended.

But a very few months have elapsed since my opinion was asked on the subject of a central or National Vaccine Establishment at Edinburgh; and I then learned, for the first time, that the North of Scotland had no other *certain* means of supplying themselves, on an emergency, with vaccine lymph, than application to London. The ample and steady supplies which an enormous population, like that of Edinburgh and Leith, is so well calculated to afford, are, in a manner, lost for want of care. Nor can a private institution supply the deficiency. The free transmission of lymph by post, essential to its general utility, is a power vested in the National Vaccine Establishment alone. In reply to an offer which I made to the government in 1825, to supply country practitioners with vaccine lymph from the Small-Pox Hospital, I was informed that such a privilege could not be granted, without ad-

mitting a precedent which would be attended with great inconvenience.

A conviction of the necessity of vaccinating with recent lymph, if we desire to ensure the success of the operation, and of the importance of public establishments having the privilege of free transmission by post, for the due supply of such recent lymph, induces me to think that great advantages would arise if the plan of the National Vaccine Establishment were enlarged; if *branch banks* were established for the greater facility of obtaining, and distributing lymph; and if the whole were placed under the superintendence of a central committee in London. It is far from my wish to occupy your columns by a detail of the measures which such a plan would render necessary. I cannot, however, have daily before my eyes the advantages which the affluent population of London enjoy in the possession of public establishments for the diffusion of vaccine lymph, without desiring, as far as in me lies, to extend the same to the poorer classes in the country.

When I call to mind who are the individuals who at present guide the National Vaccine Establishment, I feel convinced that if such a measure as I now contemplate be really required, it will be carried into effect. I can of course know but little what facilities of obtaining effective lymph the population of the provinces enjoy, and I may perhaps have overstated the case. In the event, however, of my suggestions being thought worthy of attention by those to whom the government entrusts the important task of superintending the public supplies of vaccine lymph, I venture to add the following hints.

The districts in which branch establishments are formed should be such as will afford not less than 500 vaccinations annually. They should be conducted by medical men, sufficiently paid to enable them to give up three of the best hours of the day to the purposes of vaccination. Their remuneration should come in part from the medical men in the vicinity, (who, in return, would have free access to the establishment) and in part from the government, who would claim, in return, the privilege of directing and overlooking their proceedings. For this purpose one or more persons should be charged with the duty of inspectors, and by them all matters of detail would be regulated.

On a careful consideration of the whole of this interesting subject, I have satisfied myself that no measure would go so far to meet the emergencies of the case as that which I have now suggested; and I cannot for a moment believe that the government of the country would grudge the additional expense which it would entail, or refuse to extend to the provinces that boon which the experience of twenty years has proved to be of incalculable value to the inhabitants of the metropolis.

I have the honour to be, Sir,

Your very obedient humble servant,

GEORGE GREGORY.

From the London Medical Gazette.

ON SOME OF THE DISEASES OF THE STOMACH, connected with Watery Secretions from that Organ. By EDWARD J. SEYMOUR, M. D.

The word pyrosis has been applied, and is very generally used, to designate the secretion and discharge from the stomach of a transparent watery fluid without any taste; the action of the stomach by which it is evacuated being unattended, in the majority of cases, by any pain, which is, however, occasionally present.

The complaint is unaccompanied by fever.

The disease commonly yields to the employment of astringents and opiates, and, in very mild cases, astringents and alkaline, or antacid medicines, are usually successful: to these are added a regulated diet of animal food, and abstinence from the use of acids or acerb vegetables.

This disease is more common in women than in men, and in my observation more frequent at the middle and latter periods of life than in youth.

The pyrosis which is said to be prevalent in Scotland and Ireland is believed to arise from the constant use of vegetable food: of such cases I have no experience, and I am not acquainted with any authentic observations on the pathology of the disease, as it occurs in those districts.

The following remarks apply to cases of secretion of watery fluid from the stomach, as they are presented to the observation of the physician in large towns, and among persons from whose manner of living it is manifest that the exclusive use of vegetables cannot be considered as a remote cause of the disease.

The first cases I shall relate are those in which this disease appears to arise from increased sensibility of the nerves supplying the secreting surface of the stomach. The best theories of secretion attribute to the nerves the principal part in the performance of this function, and the fact of the frequent occurrence of the disease in persons whose nervous system is unusually deranged generally, would lead us to this conjecture.

But in addition to describing such cases—viz. derangements of the functions of the stomach—it is the object of the following remarks to point out some organic diseases of the stomach itself, or of the neighbouring viscera, of which pyrosis is a leading symptom, that we may not in practice undervalue, when first presented to our notice, a symptom occasionally of serious and even fatal disease, because, in the majority of cases, it attends a morbid condition of the stomach of frequent occurrence and easy relief.

I proceed to speak of the secretion of watery fluid from the stomach, without disease of structure of the organ itself, or of the neighbouring viscera.

One of the most ordinary forms of this disease is the secretion of watery fluid from the stomach in elderly people, who are easily disturbed or distressed by the operation of trivial

causes: such cases must be in the knowledge of every practitioner, and I shall therefore only select one case from among several to exemplify its nature and treatment.

*Case 1.*—In the summer of the last year, a lady, æt. 63, who resides in the country, came to London for advice, being greatly annoyed by the frequent recurrence of pyrosis. These attacks occurred often spontaneously, but more frequently were the result of some sudden emotion. A sensation of “the gurgling of fluid” was first felt about the umbilicus, rising upwards, and this was immediately followed by the discharge from the stomach, without effort, of about a pint of clear tasteless fluid. Although the discharge was attended with no pain, yet a sensation of great relief was reported to follow its excretion. The bowels were regular; the tongue clean; no pain or tension were found to exist on pressing the epigastrium; no fulness or hardness were observed in either hypochondrium. The urine was clear and of natural quantity and colour; the appetite good. There was no vomiting after taking food. It appeared to me to be a simple case of increased secretion from the stomach, owing to the increased sensibility of the nerves of that organ. I desired the patient to leave off the use of vegetables and malt liquor; to take after each meal a glassful of lime water, and to substitute *weak* brandy and water with her dinner for wine.

To strengthen her general health she was ordered the use of the shower-bath, and to check the existing state of disease the following draught was ordered thrice daily for a week:—℞ Confect. aromat. ℥j.; confect. opiat. gr. xviii.; T. rhæ. ʒiij.; aq. meuth. pip. ʒix. M. Fiat. haust.

I had the satisfaction of hearing recently that the patient's health had not suffered from an attack of her complaint for several months past.

I have said that the watery secretion from the stomach is sometimes attended with pain; such cases often resist the means so serviceable in those to which I have just alluded, the stomach being altogether unable to change into nourishment the animal diet so useful in more recent or less severe instances.

The following will best elucidate my views on this subject:—

*Case 2.*—The servant of a lady of rank, æt. 29, applied to me in the spring of the year 1827, under the following circumstances:

She had acute pain at the pit of her stomach, often followed by the rising of tasteless fluid into her mouth, so suddenly as occasionally to pass through the nose. She was much emaciated; her pulse was weak; her tongue white, with a cream-like fur on it; her sight, she said, was dim; her appetite very indifferent; and on taking food she occasionally, but seldom, returned it by vomiting. The catamenia were less than usual in quantity, but observed the ordinary period: she slept tolerably well. Her complaint had lasted four months, during which time she had taken every species of medicine usually ordered for disorder-

ed stomach and bowels: mercury in full doses, and, as an alterative, bark, bitters, absorbents, opiates, the subnitrate of Bismuth, the Prussic acid, alkalies fixed and volatile, and blisters; each in their turn had been applied without effect. It occurred to me that food entirely of one kind, and in small quantities, persevered in for a considerable time, and aided only by such remedies as would allay the increased sensibility of the nerves which supply the secreting surface of the stomach, were most likely to effect a cure. I ordered her the following medicine for the latter indication:—*R.* Extr. conii, gr. iv.; aq. cinnamom. ℥ss.; liquor. calcis, ℥j.; syrupi, ℥ss. *M.* Fiat haustus, ter in die sumendus.

For the article of food I recommended abstinence from every thing except bread and milk, which was never to exceed in quantity at one time what might be contained in a breakfast cup.

The patient had the good sense and resolution to persevere in this regimen for more than a month, at the end of which time she was free from complaint.

She has experienced no relapse.

*Case 3.*—*Mrs. W.* æt. 43, the wife of a small tradesman in poor circumstances, was admitted into the Asylum for Recovery of Health, under my care, Jan. 11, 1827.

She complained of extreme debility, with constant eructations of wind from the stomach, and pain at the scrobiculus cordis. Tongue clean, no vomiting after food, but she is constantly bringing up from the stomach small quantities of clear, tasteless fluid. This fluid occasionally passes through her nose, from the rapidity with which it is discharged from the stomach. The pain is not increased after taking food.

There is great nervous distress, augmented by the least noise: she has some headach, but it is not constant; occasional urticaria. Appetite impaired; catamenia have been absent since July, slight leucorrhœa; bowels regular. She has been ill several weeks.—*R.* Subnitr. bismuth. ℥ss.; magnes. carbon. ℥ss. *M.* Fiat. pulvis, bis in die sumend. *R.* Sp. ammon. arom. ℥xxv.; mist. camphoræ, ℥x. *M.* Fiat. haust. p. o. n. sumend. Applic. Emplast. canthar. epigastrio.

These medicines were continued, with regulated animal diet, until the 24th, when the report is as follows:

Great pain and uneasiness about the scrobiculus cordis, with constant rising of fluid through the nose. Pulse 76, weak; tongue clean; bowels open.

She was ordered a pill of half a grain of calomel and six grains of extr. conii twice in the day, instead of the bismuth powder.

This plan was attended with no advantage, and on the 1st Feb. I was determined to have recourse to the following medicine, and strict milk diet.—*R.* Pulv. rhæi, gr. v. fit. pulvis bis in die sumend. cum. haustu sequent. *R.* Magnes. ℥ss.; potassæ carbon, ℥j.; aq. fontan. ℥j.; sacchari, ℥j. *M.* Fiat haustus, cum. coch. j. max. succi recentis limonum sumend.

On March 5, the report is, by living only on milk, with the medicine last prescribed, she has gained flesh and strength, and is free from pain.

The strict regimen distressed the patient so excessively that I yielded to her entreaty to be permitted a slice of meat daily. Three days afterwards her symptoms returned, and it was not until after a perseverance in the exclusive use of milk diet for three weeks longer that she entirely recovered.

She left the Asylum on the 27th of March, free from ailment, having gained flesh very considerably.

I proceed to a most important part of the subject; viz. that watery secretion from the stomach is often a symptom, and sometimes the only symptom, of commencing organic disease of the stomach or adjacent viscera.\*

*Case 4.*—In November, 1826, a gentleman, æt. 59, of sedentary habits, and much oppressed by the fatigue of business, but who had enjoyed good health for many years, consulted me for pyrosis; which he described as the occasional rising into his mouth of a tasteless fluid, without any pain or uneasiness whatever; and he never vomited his food. The whole quantity of tasteless fluid evacuated on these occasions might amount to an ounce and a half at each time, occurring sometimes twice in one day, but often not more than once in several days.

The patient's diet was regulated to small quantities of animal food, and he was ordered 20 minims of liquor. potassæ in lime-water thrice daily. The inconvenience, however, was so slight, that he does not appear to have complied with the prescription.

A few months afterwards a large tumour was observed in the left iliac region, which ultimately proved to be fungoid disease of the whole of the inferior half of the cavity of the stomach. No symptom except the pyrosis could call the attention of the physician to disease of this organ.

In Dr. Parry's Works, vol. ii., a case of ulceration of the stomach is related; the proper symptoms of which, at all times obscure, were preceded, for several months, by attacks of pyrosis.

There is also a case now under my own care, of confirmed organic disease of the stomach, where I was first led to examine the state of the abdomen, from the discharge of watery fluid from that organ.

Pyrosis occasionally arises where there is disorganization of neighbouring viscera, the stomach itself remaining healthy in structure.

*Case 5.*—A very remarkable case of this kind occurred recently at the Middlesex Hospital, under the care of Dr. Macmichael, to whose kindness I was indebted for the oppor-

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\* I must by no means be understood to say, that organic diseases of the stomach, or adjacent viscera, do not occur without pyrosis; but that pyrosis is often a leading symptom of such diseases.

tunity of seeing it. As the general statement of the case has already appeared in the Gazette, I shall only allude to it here in support of my position.

The case was that of Mary Perry, admitted Dec. 1827. There was a tumour in her abdomen, which occupied the whole of the epigastric, umbilical, and part of the hypogastric region, very tender to the touch. She vomited her food frequently; but, in the intervals of vomiting, she brought up, without effort, much transparent watery fluid, generally without taste, occasionally saltish. This amounted to three pints in the twenty-four hours; and had, in less quantity, been observed from the earliest stage of the disease.

On her death, the tumour appeared to be formed by the liver, greatly enlarged from the presence of those tubercles which have received from Dr. Farre the name of "*Tubera diffusa*," and have been classed by the French writers with other cancerous and malignant tumours, under the name of "*Encephaloides*." The stomach was quite healthy in structure, but lay collapsed, its greatest breadth being not more than two inches, its greatest length about seven.

A case not very dissimilar to the foregoing occurred to my observation at Guy's Hospital, under the care of Dr. Richard Bright, in the autumn of 1823. Unfortunately the notes of this case are mislaid; but it was remarkable for the constant secretion of fluid from the stomach, (here, however, it was acid.) The vomiting which accompanied it; the rapid loss of strength, and pain, gave rise to the idea of cancer pylori being present. After death the stomach was found to be healthy, but subject to great pressure from the liver and spleen, much enlarged by chronic inflammation.

Another condition in which the secretion of watery fluid from the stomach appears in conjunction with or symptomatic of disease in other parts, is where it precedes either the formation of renal calculi, or their passage from the kidney to the bladder. My limits will not allow me to detail cases of this kind: I must therefore refer to Dr. Parry's works, in which he describes his own frequent attacks of pyrosis, preceding, as they often did, very severe paroxysms of nephritic colic.

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From the Bulletin des Sciences Medicales.

#### DR. HEIBERG ON THE SPEDALSKHED; *an Eruptive Disease endemical in Norway.*

This disease, which is observed in the environs of Bergen, and the adjacent part of the province of Drontheim, and improperly designated by Fabricius under the name of *Lepra*, must not be confounded with the *Radesyge*, so well described by Prof. Holst, of Christiana. It is a species of elephantiasis, affecting a great number of individuals, who, considering it as incurable, rarely resort to medical assistance.

It occurs under three varieties, namely, the

*squamous*, the *tuberculous*, and the *glabrous*. The disease ordinarily commences by a sensation of lassitude, heaviness, and internally cold, especially in the inferior extremities; the patient is affected with insomnia, and a troublesome pruritus on the surface, quickly followed by complete insensibility of the members. These symptoms continue a longer or shorter time, in different cases, but generally at the expiration of a year, others, more characteristic make their appearance, while those above mentioned still continue.

In the *squamous* variety, there is first perceived a dryness and corrugation of the skin of the hands and feet, which gradually extending, particularly on their internal surface, attain, in some instances, the thorax and abdomen; at a later period, a dry herpetic eruption is observed, attended with a furfuraceous desquamation of the epidermis. This eruption disappears and returns alternately, and is particularly troublesome by reason of the violent pruritus by which it is accompanied; it ultimately becomes fixed, and forms large incrustations nearly an inch in thickness, which greatly impede the motions of the extremities. The axillary and inguinal glands swell, a slight chronic inflammation attacks the conjunctiva, and eventually the cornea becomes opaque. In some instances, the hairs of the eyebrows are detached. The physiognomy of the patient is so peculiar, that the disease once seen, is readily recognised. The articulations of the feet and hands swell, the bones become carious, the fingers and toes are sometimes detached, and the patient cannot walk without great difficulty. He rarely, however, experiences acute pain; cannot endure the rays of the sun, and is generally better in winter than in summer.

The *tuberculous spedalskhed* is distinguished by grayish, violaceous, and shining stains, at first small like petechiæ, but increasing in size and becoming more numerous, both on the face and extremities. These stains are succeeded by tubercles, or rather, they pass into the suppurative state. Occasionally, the tubercles are developed without these precedents; they commence on the face, and extend sometimes to the tongue and ears,—the hairy scalp and trunk are always unaffected by them. Presenting a similar hue with the stains, the size of the tubercles varies from that of a large nut, to a hen's egg. The lips are tumefied, and often covered with a brownish incrustation. The extremities swell enormously, particularly the arm and thigh; the diseased parts are insensible, and deep incisions may be made through the skin without occasioning the slightest pain. The nose is sometimes totally destroyed, the senses of smell and taste are abolished, vision is lost as in the other variety, the eyebrows fall off, deglutition is difficult, and respiration constricted, so much so, frequently, as to threaten suffocation; the voice becomes hoarse, speech unintelligible, a violent cough supervenes, the tumefaction and tubercles attack the trachea, and death is caused either by suffocation

or marasmus. In relation to the ulcers which frequently form on the extremities, they have a rounded form, and are not very deep, with margins elevated, whitish, and absolutely insensible; their base is of a red or reddish brown colour, generally without inequalities; there is an abundant secretion of sanguinolent serum, rarely or never a laudable pus. The contour of the ulcers is shining and unequal.

The *glabrous* variety is more rare; there are neither eruption, stains, nor tubercles; the skin is smooth and dry, but has an indurated feel; ulcers form on the extremities, and the joints become carious; the countenance assumes an aspect of senility, the eyebrows fall off, and the eyelids suffer from a singular disease, producing what has been called lagophthalmia. The upper eyelid is unaffected, but the inferior is shortened, immoveable, and turned outwards so as to form a complete ectropion. When the patient sleeps, the globe of the eye is directed upwards beneath the upper palpebra, and he supposes that his eyes are closed, which however he is unable to effect. Vision is soon entirely destroyed.

All the varieties of the *spedalskhed* have moreover, a very repulsive odour, which exhales from the whole body, and especially from the ulcers and breath.

Such are the principal symptoms of this hideous disease. Dr. Heiberg intends shortly to publish a more complete work on the subject. He observes that he has been successful in his attempts to cure it.

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From the *Lancet*.

## DIABETES.

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"EXPERTO CREDE."

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To the Editor of the *Lancet*.

SIR—In your admirable columns, (*Lancet*, vol. vi. p. 408,) a case of diabetes is detailed, from Magendie's *Journal de Physiologie*, illustrative of the efficacy of venesection.

More recent discussions on that interesting and vitally important subject, by Drs. Ayre and Barry of London; the former advocating its local abstraction, (cupping,) and the latter an exclusively animal diet, induce me, at this distance, to take up my pen, to offer a few practical remarks and observations, on the pathology and therapeutics of diabetes.

That diabetes, generally speaking, is a functional, (constitutional,) and by no means an organic affection of the kidneys, is tolerably obvious from the deranged, not to say vitiated, state of the alimentary passages, which both precede and accompany it; the emaciation and debility consequent thereon, and the dissections, such at least as I have myself witnessed after death. Chylification, it is well known, is the commencement, and excretion (intestinal, urinary, and cuticular) the completion of digestion, the latter proving de-

fective or redundant, as one or the other of these valued excretions fails to perform its proper function in the economy. Heightened by predisposition or habit, perchance the kidneys, from a sympathy peculiar to them, are prone to deficient or excessive secretion, as dysuria and diabetes amply testify. The intestines and skin also have these predispositions—as constipation and diarrhoea, (dysentery being somewhat of an organic affection,) and a parched skin and profuse sweatings, no less testify. These important passages, neither elegantly nor classically expressed, "outlets" (bearing organic affection) prove obedient to the mighty work of digestion; hence an imperfect chylification is wont, as might be expected, to derange and vitiate them. Fermentation, depravity of secretion, and a want of tone, constituting dyspepsia, are common to the alimentary passages, and a fertile source, primarily, of diabetes. In conjunction with my friend, the late amiable and scientific Dr. Watt of Glasgow, many years ago, I first witnessed the effects of venesection, to any extent at least, in diabetes; and it is but due to science to say, that I remarked at the time, and have had it amply confirmed since, that without marked attention to the digestive organs, and fæcal excretion afterwards, the patients were addicted to relapse; which, ultimately, in some instances, proved fatal. The human fabric, be it impressed upon the practitioner's mind, will seem to rally, at times, under venesection, when its powers manifestly are verging to destruction. In the diabetes of extreme youth, or old age, little benefit, if any, will be derived from local or general blood-letting; for the vital powers, in the one case, are not established, and, in the other, perhaps are too feeble to carry on the great business of circulation. Nor have I found "exclusively a diet of animal food" at all suitable on such occasions, from its acknowledged disposition to generate a species of repletion, (both sanguineous and excrementitious,) fertile of cerebral, and hepatic affection in the aged, (as apoplexy and palsy;) and of pulmonary affection, on approaches to manhood, which phthisis confirms beyond the possibility of a doubt. In the cases of diabetes alluded to, a want of tone is apparent in the kidneys, and alimentary passages throughout, which the undue distention of, by fermentation, has tended not a little to establish. From no inconsiderable experience, I can affirm, that here the cortex cinchonæ lancifoliæ, fresh, and finely powdered, will be found to prove most eminently efficacious. In confirmed cases, however, it is needful, at the same time, to blister the sacrum, with a view to counteract the morbid action of the kidneys, and even essential to correct and secure a regular action of the intestinal passages. The quantity of bark should not exceed ʒss. or ʒij. in a sufficiency of any simple vehicle, (say water or skim-milk,) three or four times a day; and the subsequent form of pill, I have found to prove the best accompaniment. R. Pil. colocynth. c. — hydrarg. pulv. jacob. aa

gr. xv. ad ʒi m. ft. pil. duodecim: one, two, or three of which, may be had recourse to at bed time daily.

Regard must be had to temperate regimen, thorough ventilation, cold ablutions, and exercise in the open air daily. If morbid secretion (rather than mere fermentation, and want of tone in the stomach and alimentary passages) prove a chief source of diabetes, which a bad digestion and vitiated intestinal excretion had long foretold, characterised, as they had been, by a foul coated tongue, pyrosis, gastrodynia, constipation, or occasional diarrhœa; we must be careful, under such circumstances, to prepare the patient for the specific (i. e. tonic) operation of the cinchona. The preparation to which I allude, is to correct a morbid state of secretion, whether of the stomach, liver, or alimentary passages, by mild mercurial, antimonial, aperient remedies, (such, for example, as pil. hydrarg., pulv. jacob., magnes. sulph., soda carbonat., &c. ;) aided, it is important to remark, by judicious regimen, a tranquil tone of mind, (for mind proves a fertile source, primarily, of dyspepsia,) frequent cold spongings of the chest and abdomen, sufficient ventilation, and exercise in the open air daily. Heat being a powerfully exciting cause of fermentation throughout the alimentary passages, and fermentation a fertile source of indigestion, I have ever found these cold ablutions of the chest and abdomen beneficial, to say the least of them, under dyspepsia. I am moreover firmly of belief, that a deteriorated atmosphere, the result of insufficient ventilation, tends greatly to derange and vitiate the secretions and excretions of the body, on the perfection of which, we depend for health and longevity.

Another, and by no means unimportant consideration in diabetes, and I conclude. I allude to the temperature,—heat, more properly speaking, of the patient. Such patients, from time immemorial, have been kept warm, when, in fact, night or day, they can scarcely be kept too cool, if we study at all their convalescence. Reposing on beds of down, and wrapping in flannels, have proved most prejudicial, not to say fatal, to them. The same might be said of warm bathings, of tepid spirituous potations, and of forced, as I would define it, rather than excessive, venery. That inordinate thirst which prevails in diabetes and dropsy under opposite states of the kidneys, is fostered greatly by habit, and ought not to be given way to, if we expect to restore either healthy action or secretion of the alimentary passages, the skin, or the kidneys. Our errors in practice arise, very many of them at least, from insufficiently distinguishing in our modes of treatment, at any rate, between functional and organic affection, which require, not unfrequently, a marked and opposite mode of practice, and this I would impress upon the practitioner's mind, in his treatment of diabetes.

In a word, it is plain that venesection, cupping, and “an exclusively animal diet,” are but ill adapted, either for the prevention or

cure of diabetes; consequently, for mere relief's sake, we must not wield such doubtful, not to say dangerous, weapons—seeing that we have at command more temperate, efficacious, and permanent measures.

I remain, sir,

Respectfully yours,

W. HORSLEY, M. D.

*North Shields, April 15, 1823.*

From the Transactions of the Edinburgh Medico-Chirurgical Society.

# OBSERVATIONS ON THE NATURAL OR SPONTANEOUS CURE OF SYPHILIS. By JOHN WILSON, M. D., Hull.

From the time of the celebrated John Hunter, the various symptoms of syphilis have been so well described, and the appropriate treatment of each so accurately pointed out, by many distinguished authors, that there seemed to be no room left either for improvement or farther discussion.

The practice of the medical officers of the army, has, indeed, within the last few years, introduced a new era in the history of this disease; but as the inquiries they have so meritoriously instituted, and so candidly related, may not convince every mind of the propriety of their practice, and of the deductions which naturally result from its success, I shall offer a few observations on the causes most likely to impede its adoption, with some facts and inferences which seem to confirm the principles on which their practice is founded: for, notwithstanding the apparent advancement in our knowledge of this disease, there is perhaps no other which still produces more discordant opinions among practitioners, or whose effects are contemplated with more anxiety by the patient.

It has always appeared to me, that much of this uncertainty may be attributed to the division of lues venerea by some of the later writers, into syphilis, and pseudo-syphilis. I shall therefore begin with inquiring into the propriety of this division.

The primary symptoms of syphilis are well known to vary much in their nature and extent, from a simple abrasion of the skin, to the true syphilitic chancre, or phagedenic ulcer, as described by authors; they have often been observed to heal spontaneously, or under the application of topical remedies only, without being succeeded by secondary symptoms; and the mildness or severity of the latter has no apparent relation to that of the former; for secondary symptoms of various kinds and degrees, sometimes supervene even in cases where no primary symptom had existed, or in which it had been so slight as to pass unobserved. The secondary symptoms have also long been known to disappear spontaneously sometimes, and many well authenticated cases of their removal, by remedies of little or no acknowledged virtue in the present day, are

on record: I allude to a period preceding the late cures by the medical officers of the army, by simple topical applications, and a rigid observance of the antiphlogistic regimen. The nitric acid, for example, about twenty-five years ago, had a higher reputation than any other remedy ever acquired except mercury; and this was founded on as indisputable evidence as medical facts will admit of. Even the success of many inert quack remedies, well known to contain no mercury, may be fairly admitted as a confirmation of the tendency which this disease has towards a natural cure.

The belief, however, of the undeviating progress of the syphilitic virus, and the hopelessness of combating it by any other remedy but mercury, were so firmly established, that these favourable terminations were either regarded with indifference, or the disease was deemed to have been not truly syphilitic.

With those extensively engaged in the treatment of lues venerea, the great difference in the facility with which it is cured, in different cases, could not fail to arrest the attention; for although the disease, in some instances, completely answers the worst descriptions given of it by authors, and even mercury itself fails to remove it; yet all will agree, that the great majority of cases yield to this remedy, and that it often happens, that bad cases are cured by apparently a very inadequate portion of it.

These discrepancies no doubt suggested the opinion, that there were several kinds of the syphilitic virus; but the grounds of distinction among these seem to me unsatisfactory.

Mr. Abernethy, to whom medical science is so much indebted, defines syphilis by observing, that "the constitutional symptoms of syphilis are generally progressive, and never disappear unless medicine be employed;" and, "that they are generally relieved under an adequate effect of mercury on the constitution."

If the symptoms of true syphilis could be distinguished from those of the spurious disease or diseases, by any well defined difference in their external character, and not solely as they appear generally to have been, by their constant progression and resistance to every other treatment but the mercurial one, the necessity of such a division would be obvious, even at the present time, when both of these last statements are opposed by the non-mercurial cures by the army medical officers, and other facts in the history of the disease to be immediately adduced.

Mr. Abernethy himself, however, who has adopted the distinction of syphilis and pseudo-syphilis, explicitly acknowledges, that it is impossible to fix upon any diagnostic symptom which can enable us to mark a difference. It is therefore obvious, that the distinction has been formed solely for the purpose of separating diseases that may be cured without mercury, from such as cannot; and that the nature of the affection cannot be ascertained,

until that remedy be employed, and its effects observed.

In treating of pseudo-syphilis, this author remarks, that "the fictitious disease in appearance so exactly resembles syphilis, that no observation, however acute, seems to be capable of deciding on its nature."

Mr. Abernethy, indeed, attempts to distinguish the true from the spurious disease; but it is evident that the only ground on which he relies, is the effect produced by mercury, and later authors might be quoted to the same purpose. If this remedy was innocuous, such a mode of forming a diagnosis might be safely admitted; but as it not unfrequently has deleterious effects on the constitution, such practice must often be attended with unnecessary suffering to the patient, and much perplexity to the practitioner.

As we thus see an author of distinguished eminence, and of extensive experience in venereal diseases, acknowledging the impossibility of distinguishing between syphilis and pseudo-syphilis, by any other mode than the future contingency of a mercurial course, the salutary termination of which is by no means certain, and which, on all hands, must be allowed to be a severe test for the patient; it may be presumed that this division is neither founded in nature, nor accompanied by any practical advantage.

There are other diseases arising from specific contagions, in which as great a diversity of symptoms occurs as in the cases called syphilis and pseudo-syphilis, yet it is known that they arise from the same virus, however it may vary in its effects; thus, there is as much difference between a mild case of small-pox or scarlatina, and a severe one, as can occur in syphilis; and the effects, local and constitutional, of the animal poison absorbed in dissections, are at least equally various, although of much shorter duration; and these examples are exactly in point in another respect, inasmuch as their mildness or severity do not depend on the kind of virus or contagion applied, but on some peculiarity of constitution, the nature of which, it must be confessed, often cannot be discovered, although it is sometimes sufficiently apparent.

The phlogistic diathesis, and especially that state of the system induced by a frequent and liberal use of intoxicating liquors, certainly contributes both to a greater liability to the contraction of the venereal poison, and to render its effects more obstinate and severe; and, accordingly, in naval practice, where the source of infection can be more easily traced, and the habits of individuals more exactly ascertained, than in most other situations, I have often had occasion to contrast the effects of the same virus in different subjects, as very generally confirming this. I have also repeatedly ascertained the same woman to have infected several individuals in quick succession, in some of whom, the primary and secondary symptoms of syphilis followed; while others had gonorrhœa with swelled testes;

and the severity of the disease very exactly corresponding to the habits of the patients.

The scrofulous diathesis also, is generally supposed to have an influence in many diseases, and although sometimes obscure and of difficult cognizance, its existence may often be ascertained by the well known external characters; and all the writers on venereal diseases concur in stating their obstinacy when it is present; indeed this might *a priori* be expected, as these diseases have considerable analogy in some respects to each other. In the few fatal terminations of syphilis which I have witnessed, this diathesis was well marked: but it was doubtful in those cases, whether the event was more to be attributed to the disease, than to the mercury with which the patients had been largely and repeatedly imbued.

The pernicious effect of mercury, described by some authors, in superinducing a peculiar irritation of the system, sometimes attended by the erythismus mercurialis, during the continuance of which it possesses no power to arrest the progress of the disease, but, on the contrary, is said to convert the existing symptoms into other equally dangerous affections, not to be distinguished from the original ones, is another source of painful uncertainty. It chiefly occurs in those cases where repeated courses of mercury have been used, and of course the first step in the cure is the omission of the remedy.

An examination of the historical evidence, on which the American origin of lues venerea has been founded, would exceed the limits I have assigned to these observations; but that the disease became very prevalent in Europe, and much attracted the attention of medical and other writers, soon after the arrival of the companions of Columbus from Hispaniola in 1493, and was then, for the first time, imputed to infection from impure coitus; seem to me to be the amount of all the evidence of its importation from that quarter.

The belief of the foreign origin of the disease, has had much influence on the opinions entertained of its nature, and particularly as occasioning the effects of ordinary causes on the genital organs to be overlooked. It cannot be denied that these organs are as susceptible of disease as other parts of the body, and that inflammatory action should produce in them a *virus sui generis*, and capable of producing disease when applied to another, is perfectly analogous to what is known of some other morbid poisons of spontaneous origin.

The vaginal discharges of women, in whom no venereal poison can be suspected, are said to be sometimes so acrid as to cause ulceration of the parts with which they come in contact; and that this matter applied to the glands or prepuce of a healthy person, should inflame or ulcerate these parts, or be absorbed, and induce a peculiar train of symptoms, is quite consistent with what is known of morbid poisons in general.

When, however, an author of Mr. Abernethy's acknowledged accuracy and expe-

rience asserts, that he has known "both gonorrhœa and ulcers occasioned by connexion with persons who had no syphilitic disease," and that "gonorrhœa arising under the same circumstances, is so common as to need no exemplification;" it seems almost superfluous to adduce other proofs of the disease arising spontaneously. In treating of pseudo-syphilis, he farther observes, "it may be right, however, to remark, that there are cases which would induce the belief, that ulcerated sore throats, eruptions and nodes on the bones, similar to those described, may occur from a general disturbance of the constitution, without the absorption of any infectious matter;" and, that "some species of animal matter, which are not the products of disease, are, nevertheless, capable of exciting it in others."

If, indeed, the structure and functions of the organs, with the circumstances so often preceding and accompanying the origin of venereal affections are duly considered, it becomes rather a matter of wonder, that diseases of the genitals do not occur more frequently, and terminate in ulceration, independently of the application of any morbid secretion; and if the latter, however formed, is applied, the nature and condition of the parts, under these circumstances, are well adapted for its absorption.

In support of the opinion that venereal diseases are indigenous in every country, I may be permitted to observe, that I have witnessed gonorrhœa in a native of New Holland, at a great distance from any British settlement, and in a situation entirely precluding the possibility of his having either directly or indirectly acquired it from any European or other foreign source; for the population of that country is extremely thin, and each tribe lives so completely isolated, that they often have no knowledge of the language, or even existence of their nearest neighbours.

A difference of opinion still exists as to the identity of the virus of syphilis and gonorrhœa; but the latter is, I believe, every where of much more frequent occurrence than syphilis.

The history of the disease at the island of Otaheite, where it has undergone a remarkable revolution, disproves, on a large scale, the opinion of the undeviating progress of syphilis, unless opposed by a specific remedy; and also shows, that gonorrhœa may exist in a large community without inducing it.

This island, as is well known, was discovered by Captain Wallis in the year 1767; but, as the natives were hostile, and he remained only for a short time, it is probable that his ship's crew had too little intercourse with them, for ascertaining whether the disease existed in the island at that time; his people, however, did not contract it. It was subsequently visited at different periods, two or three years afterwards, by the celebrated Captain Cook, and the French circumnavigator Bougainville, both of whom found the disease to be very prevalent; but as their relations are confined to a general statement of

the fact, it is impossible to decide by what form of it their crews were affected. During a succeeding period of about thirty years, the island was visited by few ships; all of those, however, whose voyages have been published, report that their crews were infected, and that the disease continued to make great ravages among the natives.

His majesty's ship *Porpoise*, of which I was surgeon, visited the island in 1801, and remained for three months. From the accounts in voyages, and the personal reports of some missionaries, who had resided there for several years, there was much reason to fear that our people would suffer from the disease, and, accordingly, precautionary measures were recommended to be adopted by the captain, which were, however, soon found to be unavailing, and were abandoned after the first week of our visit.

The connexion of our people with the native women immediately became unlimited, and probably two-thirds of the whole of the young ones, at that time on the island, were at various times on board.

During the first month, none of the crew contracted any venereal disease whatever; and in the remaining part of our stay, only three of them were affected with gonorrhœa.

In a second visit, in the following year, after remaining two months, and one at the neighbouring island *Eimeo*, thirty of the crew contracted gonorrhœa, but no other form of the venereal disease appeared among them, nor did any secondary symptom supervene. Only one case of swelled testis appeared, in a person much addicted to inebriety.

It is certain, that in neither of our voyages, did our ship import any venereal disease, as the people submitted to a minute inspection ten days before our arrival; but in the interval between our voyages, a small vessel had arrived, and was wrecked on the island; whose crew, amounting to twenty-one, we found living on shore, and one of whom brought a severe gonorrhœa with him. This sufficiently accounts for the great increase of that infection since our first voyage.

The prevalence of scrofulous tumours and ulcers seemed to me to have misled former reporters respecting the ravages of syphilis; as, at first sight, these appear to be uncommonly frequent, although a minute examination would probably show that they are not much disproportionate to the numbers affected in Europe; for the scanty dress usually worn by the natives is ill calculated for concealing whatever external disease they are affected with.

The total exemption of the natives from any other form of the disease, and the opinion at that time entertained by the best writers, of the nature of syphilis and gonorrhœa, the first of which was universally believed incurable by any remedy but mercury, and the last to be only a topical affection, incapable of inducing the constitutional disease, seemed to me to prove, that syphilis had never been in-

troduced into the island, and that former navigators had met with gonorrhœa only, which (so fluctuating are medical opinions) was in their time deemed capable of inducing all the symptoms of syphilis.

Accordingly, in some observations on this subject, which were published in the *Edinburgh Medical and Surgical Journal*, I was at some pains to maintain this opinion; but, since that time, having fortunately met with the late Captain Colnet of the Royal Navy, an officer of accurate observation, and very extensive information, who accompanied Cook in two of his voyages, and who decidedly testified, that he had witnessed chancres and buboes, as well as gonorrhœa, in many of their crew at *Otaheite*, that opinion becomes no longer tenable; and the only admissible conclusion now is, that syphilis, in the space of about thirty years, had disappeared from the island.

It cannot be believed that the crew of the *Porpoise* could have escaped the infection of syphilis, had it existed in the country, after such unlimited intercourse with the women as has been stated; nor that the latter, who have no regard for chastity, would be restrained from such, on account of their being infected.

After a careful inquiry, in many excursions throughout the island, accompanied by a missionary well acquainted with the language, I was satisfied that the inhabitants had never used any remedies except salt water and cocoa-nut milk, occasionally as a purgative, and that the great diminution of the population which had taken place, was to be ascribed, not to any infection from Europeans, but to a fever of an intermittent or remittent type, and dysentery, which had been long prevalent, and very fatal, and to their own practices, of which I have given some account in the paper alluded to.

The mode of cure so successfully adopted by the medical officers of the army, by simple external applications, a rigid antiphlogistic regimen, including a complete state of rest in both the primary and secondary stages of syphilis, have been so fully detailed by them, that it is not necessary to repeat them here. The character of these gentlemen, and the publicity of their practice, entitle their reports to the utmost confidence; and it will be principally to their exertions that the profession and the public will owe the benefits that must ultimately accrue from a pursuit of the inquiry they have so zealously carried on.

Whether the use of mercury may be safely superseded altogether in this disease, will require a large accumulation of experience to determine; in the mean time, where the antiphlogistic treatment can be safely adopted, and effectually followed, there can be no doubt that it will be equally successful in private practice; but as the same rigid observation of rules cannot always be expected, it is probable that the general results will be less favourable than in hospitals, or in military and naval practice.

From the London Medical Gazette.

ON THE APPLICATION OF MERCURY  
TO VENEREAL COMPLAINTS. By S.  
D. BROUGHTON, Esq.

To the Editor of the London Medical Gazette.

SIR,

If the following very small and humble addition to the mass of evidence and argument already before the public, in various works, can be considered in any way tending to correct an erroneous principle of practice, singularly prevalent in the present improved state of surgical science, it cannot be more advantageously placed than in the columns of the Medical Gazette, the weekly receptacle of reports and criticisms of a strictly fair, sound, and authentic character, and on which the profession and the public may safely rely.

Usually a mine of prejudice has been ready to explode when any *anti-mercurial* doctrines have been broached; and in attempting to disturb theories sanctioned by age, and rendered orthodox from ancestral experience, the curse of modern heresy often attaches itself to the avower of that which many regard rather in the light of rash innovation than improvement.

I will not here dwell upon the often-told tale of the origin and progress of lues venerea, and the *blessing* conferred upon suffering humanity by the introduction of a supposed *specific* check to its ravages, in the form of mercury. Both bane and antidote have gone hand in hand together, during more than three centuries; nor is it my purpose to repeat the several well-known efforts made, from time to time, to lay the offspring of impurity, by the substitution of milder measures than such as are afforded by extensive and long-continued mercurial applications. These efforts have, indeed, generally fallen in time into disrepute, and the old remedy has been fondly and pertinaciously adhered to; the public has been taught to distrust the one, and to consider caution, safety, and security, as the sure commendation of the other.

I cannot glance at these successive efforts without offering a remark (with which, I believe, well-informed, experienced, and judicious practitioners, will generally coincide,) that Mr. Rose's late introduction of an anti-mercurial treatment (though some, perhaps, may consider it as a failure) is one of the greatest improvements in modern surgery, has made the most general impression, and imparted the most useful direction to practice, of which the pathology of the present century can boast. I also consider Mr. Rose's doctrines to be further valuable, upon the ground that they do not inculcate the necessity of hunting after one specific remedy as a substitute for a discarded specific; but that they tend, on the contrary, to lessen the estimation so long cherished of a dogmatical practice, introducing in its place a pathological theory and remedial measures, built on a far more rational foundation.

These doctrines have guided my practice some years, and experience has taught me not to distrust their truth, safety, and value. I have not allowed myself to be scared by imaginary dangers and terrors into the adoption of an ancient dogma, too often followed upon the ground merely of *suspicion* and *probabilities*, when the alternative was the use of a powerful mineral—at the best more or less a positive evil—the ultimate mischief of which, past and present history show to have been, and still is daily being, confounded with the effects of venereal poison, sometimes extending beyond calculation and the chance of recovery, from its destructive ravages upon the constitution.

I aver that mercurial saturation tends to the destruction of mucous membranes, the removal, by morbid absorption, of soft parts, and the disorganization of the osseous substance.\* The *mind*† itself has suffered, the constitution received a baneful impression, and pulmonary consumption‡ has not unfrequently terminated the patient's earthly career, when the system has undergone mercurial saturation; while the wretched victim of imbecility, or imprudent rashness, has been commonly reported (with ill-timed levity) to have died of "*the breeches fever*." Is it not better that we should

"———rather bear those ills we have,  
Than fly to others that we know not of?"

I do not, however, contend for the entire abolition of mercury in venereal complaints; like the abolition of slavery, it must be done with care and caution, from regard both to the moral and physical constitution.

Though "custom be the plague of wise men, and the idol of fools," the deep-rooted prejudices which it implants require gradual

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\* A young gentleman went through a full course of mercury: symptoms of phthisis pulmonalis followed repeated inflammatory attacks of the chest, and several deep holes were formed in the forehead, in the manner called *corona veneris*, but more properly should be *corona mercurii*. This patient did not die, but his head is marked, his constitution debilitated, and he is liable to sore throat and acute catarrhal affections, and has been obliged to pass much time in the South of Europe, to the detriment of his professional advancement in his own country.

† The brother of a fellow-student of the author, constitutionally strumous, went through a full course of mercury, and in a few weeks he was placed in a private madhouse.

‡ A respectable unmarried tradesman, in the city, 30 years of age, went through a full mercurial course. Impaired digestion, irregular bowels, symptoms of phthisis pulmonalis, and diarrhoea, followed. He was removed to the sea-side, and there died of hydrothorax, &c.

A young nobleman used mercury to a very considerable extent. Consumptive symptoms, &c. ensued; he was sent to the South of Europe, and there died.

steps to eradicate them; and there may occur certain cases wherein a *judicious* application of mercury will be serviceable and inoffensive. But Mr. Rose's mass of evidence has shown how easily we may wear ourselves of the practice, and that the suspension of the mercurial plan is a bug-bear, the fear of which has intimidated too much, and been carried unnecessarily far. At the same time it appears to me, that the adoption of mercurial remedies is more frequently the act of temerity than its rejection, at least as it is frequently applied. And I trust it is not extravagant to believe that *Mercury* is often more to blame than *Venus*, or that the destroying sword of the *god* does more execution than the insidious blandishments of the *goddess*.

These remarks have been elicited, not only from having repeatedly observed the safety of an antimercurial practice, but also the frequent terrible consequences of the contrary method; consequences which I do not hesitate to declare sometimes far, very far, exceed what it is possible to conceive from the progressive steps of venereal taint in the constitution. Then is mercury so mild and safe a remedy in its action that it may be indiscriminately used in *all cases of suspicious sexual intercourse*? Independently of otherwise removing the complaint possibly, even if suspicion be justly formed, is no time to be allowed to give the disease fair play, and declare its nature and character? Is no account to be taken of the many little constitutional and local circumstances which aggravate an excoriation, ulcer, or bubo,—or produce some cutaneous eruption, easily removed by a little patient care and judicious treatment, without saturating the system with mercurial ointment and pills? Yet such is a very common practice, especially with *general practitioners*; so that amongst high and low, rich and poor, a *sufficient course of mercury* is generally recommended to the patient; and he must get out of the scrape as well as he can—if matters go wrong, by aid of sarsaparilla, the sea-side, mild climate, &c. &c. Then, sometimes, it is thought, the *poison lurks in his bones*, or his throat (perhaps extensively ulcerated) shows that he has not had a sufficient quantity to destroy the venom, and therefore a little more mercury will do for him (and quickly, too, in a scrofulous habit,) when *Nice or Naples* will, probably, set him to rights again, and restore his constitution; and if it does, he may esteem himself very lucky to return with a sound skin and whole bones, &c.

Now these are not imaginary cases, and were it necessary, which it is not, a multitude of instances might be cited in proof, many of which have passed under my personal observation. Many cases might be shown to have done very well without mercury; many in which this remedy was totally unnecessary, though not, perhaps, injurious to the health; and many in which the abuse of mercury has produced distressing and protracted mischief; and, in some instances, ultimate dissolution, from a train of superinduced disorders.

On the other hand, few, I believe, if any cases can be authenticated, in which the venereal disease (as it is termed,) when left to itself, has produced any such consequences as those to which I have alluded above, without the aid of mercury.

The common *cant* is to attribute cases which get well without mercury, to what is called *pseudo-syphilis*, a term which is perfectly gratuitous.

Those whose prejudices are invincible, believe that all cases healed without mercury cannot have been venereal. With the example of Portugal before us, and some other countries, and the long trial which Mr. Rose gave the anti-mercurial system, I conceive such inferences to be wholly unwarranted. I have taken about *three hundred and fifty* recorded cases of ulcers of the penis, admitted and treated in the regimental hospital of the 2d Life Guards, *one hundred and fifteen* of which appear to have used mercury in different forms and proportions, and for different periods of time; so that about *two hundred and thirty-five* cases of primary symptoms, following sexual intercourse, have been healed by other means than mercurial remedies, as well as many not in the list.

The number of secondary cases of symptoms following the primary venereal disorder during the same period, amounts to about *twenty-two out of three hundred and fifty*. And, upon following up the narratives of these, it appears, that the majority were generally simple cases of *lichen*,\* which got well without mercury, and in no long time. The greater part of those cases which were protracted, and attended with ulcers of the throat, pains of the limbs, nodes, &c. were originally treated with mercury in the hospital, and the rest showed that mercury had been clandestinely procured at some period or other during the progress of the complaints. The simple cases of lichen, &c. were chiefly found amongst the men *not* treated with mercury, *while the most protracted and troublesome cases occurred with those who had been fully subjected to its*

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\* The following case may serve as an example of these in a healthy constitution, wherein no morbid diathesis or casualty modifies the symptoms. E. M. a private soldier, was admitted, August 10, 1827, with a deep sore on one side of the glans penis, following recent connexion. He was put upon low diet, and saline aperients; and the sore dressed with lint, dipped in the *lolio nigra*. In three weeks the sore was completely healed. Three weeks afterwards, an eruption (like *lichen*) covered his body, without preceding fever. This was September 9. He took *sarsaparilla*, and on the 27th he was discharged to his duty perfectly cured. In some of these cases small doses of the oxymuriate of mercury have been combined with the sarsaparilla, when the case assumed a chronic form, and did not become quickly cured; but in most cases it was not used, and in none but in a very small degree.

operation.\* Since the use of mercurial saturation has been suspended, no cases have occurred to throw any distrust upon the propriety of the practice; and the few cases of secondary symptoms were generally mild and trifling compared with those which followed mercurial treatment, and readily yielded without mercury.

I do not pretend to enter upon any nice discriminations of practice, or to draw a line between cases requiring and cases not requiring mercury, nor to describe such as mercury will aggravate. Indeed, I am aware of no satisfactory rules to guide the practitioner in this respect, but those which he himself derives from a sound judgment and experience. I wish merely to assist in establishing the fact, that venereal sores admit of treatment without mercury and without cause of alarm; the secondary evils of mercury being usually far more destructive than those which arise from venereal taint, and that the one case is often mistaken for the other. Consequently, it appears to me, that there is more security in omitting to push a mercurial course than in adopting it; that comparatively few cases occur requiring mercury; that the perils of mercury are sometimes manifold and terrible; that at all times it entails more or less personal inconvenience and annoyance, and frequently leads to a train of ultimate symptoms, from which erroneous inferences are made, and an useless, if not mischievous practice adopted, the effects of which cannot be foreseen, and their limits no man can calculate upon.

In my Dispensary practice I have had frequent occasion to observe the great danger of pushing a course of mercury, when the patient is not under the surgeon's control as to diet, temperature, &c. An error made in the treatment of sores on the penis with persons going about, and exposed to sudden changes of temperature, to cold winds, or wet, is too often irretrievable. The labouring classes in London, perhaps not living on the best diet, nor possessing sound constitutions, exhibit frightful examples of the imprudent use of mercury; the effects of which, from some cause or other best known to themselves, there are practitioners (chiefly in private practice) who are constantly disposed to attribute to syphilitic action and deficient mercurial saturation. This propensity, indeed, I remember once to have heard very satisfactorily accounted for in a medical debating society, by a candid avowal, that if the anti-mercurial mania continued to spread, "*it would be ruin to the apothecaries and general practitioners.*" Therefore, the inference drawn from this liberal sentiment was, that it is better to be on the safe side, and not to hazard the adoption of modern heretical opinions against the "*wisdom and experience of our ancestors.*"†

\* And such I have always found to be the case elsewhere.

† It is, however, to be hoped, that the

My attention has been particularly drawn to the subject of this paper lately, from the circumstances of the following case, affording a tolerable sample of the effects which the *blessing* handed down to us by our forefathers is capable of producing.

Case 1.—B. R. æt. 25, had gonorrhœa, with phymosis, in February 1824, and slight enlargement of the inguinal glands occurred on one side of the body. Extensive superficial ulceration was discovered when the prepuce could be slipped back; the sores healed in less than a month. Six weeks from his first presenting himself were devoted to taking twenty grains of blue pill daily, and mercurial frictions were employed for three weeks. Violent salivation took place occasionally, and he was long under its influence. In three months his throat was ulcerated, and before the expiration of this period the mercury had been resumed, and only spared from excessive salivation. The palate, tonsils, and pharynx, were involved in ulceration. Subsequently he went into a general hospital, and remained there nearly a year, mercury being again tried. When discharged, the ulceration had ceased; but soon afterwards violent pains took place in the limbs, and in about a month he was re-admitted into the hospital. Again mercury was administered, salivation being produced, and the teeth getting loose. Hot baths and sarsaparilla were used latterly in another hospital, and in seven months he was discharged from it, relieved by this treatment. Subsequently he came under my care at the St. George's and St. James's Dispensary, complaining of pains in the limbs, a node on one shin, and much debility and emaciation. His breath was highly tainted, his voice snuffling and inarticulate, the ala nasi partly drawn on one side from ulceration within, and the integuments covering the nostrils looked of a livid reddish colour. He expectorated a profusion of saliva, with thick mucus and purulent matter. The tonsils were large, red, and firm; and the back of the pharynx was in a state of ulceration. A quantity of loose bone rattled in the nose interiorly, but could not be removed with the forceps; and the uvular processes were somewhat carious. He had great difficulty in swallowing, and his bowels were very costive.

On the 15th of January last, I commenced a course of sarsaparilla with him. Diluted muriatic acid was directed to be thrown up the nostrils with a syringe, and the throat gargled with infusion of rose leaves, myrrh, and honey; and the compound rhubarb pill was taken twice a day. His present situation is greatly ameliorated, though sufficiently wretched. Large masses of bone have come through the nostrils, and his throat is better.

The following case may be considered interesting, as one often attributed to the remains

gentleman who avowed this motive was as much in joke as the wag who declared that the first standing toast at the College of Physicians dinners was always, "*a slow fever,*" with three times three!

of venereal taint in the system, or deficient use of mercury, and which many would probably have treated with a full course.

*Case 2.*—In Sept. 1827, a military quartermaster put himself under my care, complaining of having had shooting pains in the head from May preceding, and sores upon the scalp, which first appeared in April. Ulceration also extended over the soft palate, uvula, and tonsils. His body was much emaciated, his tongue covered with thick fur, his pulse quick, his appetite bad, and deglutition very difficult. A copious discharge of phlegm, purulent and mucous matter, was constantly going on. About two years previously, he was treated for an eruption of reddish brown patches, slightly elevated, to which he had of late been subject periodically; the eruptions sometimes dying away, and then returning at subsequent periods, and shifting its place; the head, shoulders, and limbs, being principally affected. Latterly, some of these patches ulcerated, a portion of them drying up in crusts, and a few extending into open sores, from the size of sixpence to that of half a crown, and preceded by pains in the arms and shoulders. Just before the eruption first appeared, a superficial sore on the penis, inside the foreskin, healed under the application of an astringent wash, after three weeks' duration, leaving no vestige behind. The eruption was unaccompanied by fever, nor were his appetite and appearance bad;—his bowels were regular. The eruption was treated with the decoction and extract of sarsaparilla, moderate doses of the oxymuriate of mercury being given daily, and five grains of the blue pill twice a day. The red precipitate ointment was applied to the sores until they assumed a clean granulating appearance, and then a solution of blue vitriol in camphor julap, healed them without difficulty. The oxymuriate of mercury, and the blue pill, were discontinued after one month's use; and in a fortnight he was perfectly relieved and well, his mouth being affected. The sarsaparilla had been taken at intervals from the commencement of the eruption. For some months during the progress of this complaint, he neglected himself, and was exposed to vicissitudes of weather and temperature; and there was reason to believe he had taken mercurial pills abundantly from a druggist. When under my control, from the 9th September, 1827, suffering from fever, and the state of ulceration in the throat (as described,) together with the sore on the scalp, and with costive bowels, he was put upon a milk diet and broth, and the following draught given every four hours:—*R. Mixture salinæ* ℥iiss.; *liquor. ant. tart.* ℥j.; *magnesia sulphatis* ℥j.; *spirit. ætheris nitrosi* ℥ss. ft. haust.

The throat was gargled frequently with solution of nitre in barley water, sweetened with honey, to which a little tincture of myrrh was added, and fumigated by an inhaler; and the hot bath was occasionally used. The compound colocynth pill was also taken at bed-time when required. The sores on the head were dressed with red precipitate ointment; and

one large ulcer, in a very sloughy state, upon the crown, was poulticed with linseed meal. The pharynx exhibited a state of inflammation and tumefaction round the ulcerated parts, which were covered with a white viscid substance; and his voice was snuffling and inarticulate. In ten days the velum palati exhibited a circular orifice, about the size of a sixpence, from a slough which had been gradually formed and cast off just above the uvula. The ulceration then extended in a straight line, right and left, from this central point, removing the uvula and velum palati.

On the 19th of Sept. (ten days from his admission,) the febrile symptoms being subdued, the decoction of sarsaparilla and extract were given, with a small dose of the liquor hydrargyri oxymuriatis, thrice a day. The decoction and oxymuriate were, however, omitted in about a week, as they disagreed with the stomach, and were difficult to swallow.

As the sores got into good healing condition, the shooting pains of the head subsided. The mistura quininæ, in small quantities, was substituted for the sarsaparilla; and as the sloughiness of the throat assumed a healthier appearance, the decoction of bark, with sulphuric acid and tincture of capsicum, replaced a gargle of the mel æruginis and lime water. On the 4th of Oct. the quinine mixture agreeing with the stomach, was strengthened from four to five grains in each dose, with twenty drops of the diluted sulphuric acid and sirup. In the mean time, though the appetite and digestion had but little improved, and he was still very weak, the ulcerations were every where improving in appearance, and cicatrising at their edges. By Nov. they were healing very fast, but he complained of being choked with phlegm. The sarsaparilla was again resumed, in less quantities, and gradually augmented; and the quinine mixture left off. The sores on the scalp were nearly healed, under the application of solution of blue vitriol in camphor julap.

On the 21st of November he returned to his duty, when all ulceration was perfectly healed, the tongue clean, the appetite restored, the bowels regular, the countenance healthier, and the bodily condition generally improved.

I supposed that the loss of the velum palati and uvula would greatly impede the speech; but as soon as the throat healed, the voice became again articulate; so that these parts may be presumed to be more subservient to deglutition, by guiding and directing the passage of morsels of food and liquids, than to speech, although the latter office has been assigned to the uvula.

The following case shows the occasional influence of excessive mercurial treatment upon the system, and the removal of an ulcerated state of the throat consequent to it without mercury.

*Case 3.*—S. S. æt. 23, a dispensary patient, under my care in February, 1823, with pains in the joints, superficial and extensive ulceration of the pharynx, marks of an eruption over the body, a strong mercurial odour in the

breath, and the gums evincing mercurial influence. He had also a furred tongue, emaciated appearance, and bad appetite. A recent sore appeared beneath the foreskin of the penis, which spread quickly, but not deeply.

It appeared that somewhat more than a year ago he had gonorrhœa, and a bubo in each groin, with a sore on the side opposite to that of the last mentioned, which healed in a few weeks, and left no mark. He took mercurial pills daily, and suffered excessive salivation for some weeks. Between three and four months afterwards an eruption spread over the body and limbs, and the throat became ulcerated. He went into an hospital, where he passed through a short course of mercurial friction, and he quitted the hospital with the throat healed. It was shortly, however, brought into the same state of ulceration as before, and at this period he came under my care, at the St. George's and St. James's Dispensary, with inflammation round the ulcerated portions of the pharynx, and in the state above described. The *lotio nigra* was applied to the recent sore of the penis, and it healed in less than three weeks. The decoction of sarsaparilla and extract were given freely every day, and the throat was gargled with astringent and stimulating solutions. Under this treatment he recovered his health and strength, and was finally discharged with the throat healed, and otherwise well.

The effects of mercury upon a naturally delicate constitution, and the removal of primary and secondary symptoms, without mercury, are shown in the subjoined example of two distinct venereal complaints in one person, and at different periods.

*Case 4.*—J. H. æt. 20, a dragoon, went through five weeks' course of mercurial friction, attended with severe ptyalism, during which period a sore on the penis healed. Eight months afterwards he had violent pains in his limbs, increased at night, with thickening and tenderness of the periosteum in both legs, shortly succeeded by a foul ulcer in both tonsils; and he was emaciated and out of health. These symptoms continued two or three months, during which he used hot baths, gentle aperients, astringent and stimulating gargles, with fumigation from the inhaler, and the decoction of sarsaparilla and extract. His diet was light, and principally farinaceous at first; and he finally quitted the hospital in restored health.

After his discharge, he again contracted sores on the penis, from impure connexion, and these healed in about five weeks, with astringent lotions. The sores were not deep, and their edges were nearly even with their centres. Pains of the limbs, but less violent than the last, together with a slight appearance of *lichen*, followed in a few weeks; and recourse was again had to hot baths and sarsaparilla; and he was shortly discharged cured, and went to his duty. In the winter, however, pains like rheumatism returned, and soon yielded to hot baths, and occasionally a few small doses of calomel and opium at night.

The following case exhibits secondary symptoms, following sores on the penis, which were treated with mercury, cured without mercury.

*Case 5.*—W. B. æt. 23, a dragoon, contracted sores on the penis from connexion, which he concealed, taking mercurial pills largely every day, from an empiric. In three weeks the sores healed, and he was much salivated. Shortly afterwards, he was admitted into the regimental hospital with fever, extensive ulceration of the palate and uvula, difficult deglutition, disordered health, and emaciation. An eruption of distinct elevated tubercles spread over the body and limbs, and upon the scalp, some of which died away, and others, in appearance like *rupia*, advanced. Saline mixture, with full doses of tartarised antimonal wine, and hot baths, subdued the febrile symptoms. The throat was washed with acidulated astringent gargles, and fumigated by means of an inhaler with cinnabar. The voice was snuffling and inarticulate. Subsequently to the subjection of the fever, he was prescribed the decoction of sarsaparilla and extract, with small doses of the liquor arsenicalis, three times a day. The sores were poulticed where they were large and foul, and others dressed with red precipitate ointment, and, finally, with solution of blue vitriol in camphor julap. Under this treatment, and a suitable change of diet, he recovered his health. Having been admitted on the 16th of June, 1821, he returned to his duty on the 25th of October following.

The next case which I shall adduce, is one of secondary symptoms aggravated by mercury.

*Case 6.*—E. C. æt. 24, a military musician, had a superficial sore on the frenum, which healed in less than a month, with astringent lotions. About five or six weeks after, he was exposed to wet weather, with inflammation of the throat, and an ulcer formed in each tonsil. He procured gargles and mercurial pills from a druggist, and became very much salivated. On the 21st of June, 1824, he was admitted into the regimental hospital, with an eruption of red patches all over the body, preceded by fever, and the throat extensively ulcerated. He was much emaciated, had a furred tongue and bad appetite. The ulcers in the pharynx appeared sloughy and spreading, involving the uvula and tonsils. The fever was treated with saline mixture, and tartarised antimony, and Epsom salts. The throat was gargled with bark and alum, and as there was pain, a plaster of soap and opium was applied to the throat externally, and the inhaler was used to fumigate the ulcers. He occasionally used hot baths. In about a week, the febrile state gave way, and left much debility. The fever diet was then changed for one of good broth, milk, &c. and the saline mixture was changed for the decoction of sarsaparilla and extract. About the first week in July, the throat was perfectly healed, and the eruption gone off, and, in a few days, he returned to his duty. On the 11th of November following, he was readmitted, with an ulcer on the velum palati, which looked healthy and very superficial. A

few scattered reddish papulæ were observed on different parts of the body, which died off with the desquamation of the cuticle covering them. A gargle of bark, myrrh, and capsicum, was prescribed, and the decoction of sarsaparilla with extract, and small doses of oxy-muriate of mercury given thrice a day. On the 8th of December he returned to his duty, quite free from ulceration and eruption.

The above six cases are selected from a stock containing many similar; but it is not so much my object to accumulate examples of the points to which I have referred, as to illustrate some of my positions. Instances of the above description have often fallen under my notice in hospital and dispensary practice, and some could be produced from among private patients; and such are doubtless to be found in the records of cases, which must occur to the remembrance of other practitioners. I am induced to believe, both from the foregoing facts and observations, and those which are put forth by Dr. Thompson, of Edinburgh, Dr. Ferguson (late Inspector General of the Portuguese army,) Mr. Guthrie, and others—

1. That all the forms in which venereal complaints present themselves, are to be removed without the aid of mercury; and this is more especially and remarkably the case in regard to the secondary symptoms of the disease.

2. That mercury has formerly been, and frequently is still, used in an unnecessary, indiscreet, and highly dangerous manner.

3. That mercury, *judiciously* and *alteratively* used, is not only an excellent, but perhaps the best remedy in many venereal complaints; nevertheless, a tithe of the quantity anciently administered is generally sufficient, and more than sufficient, probably, to eradicate the primary symptoms; while, again, a tithe of that tithe, or a centime, has been found competent to eradicate the secondary stage of the disease.\*

4. That mercury is very far from being a certain preventive of the secondary train of symptoms in any form or quantity.

5. That mercury, when pushed far, induces ulceration of the mucous secreting surfaces, more especially of the inner palate, throat, and fauces, as well as affections of the bones, so exactly resembling those ascribed to true syphilis, that the most experienced surgeon cannot detect any difference. In the hands of the members of the old school, mercury, in fact, creates its own work, by establishing diseases which have too often been confounded with venereal poison, and thereby led to a most dangerous and destructive practice.

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\* Vide Abernethy on his imaginary *pseudo-syphilis*, who cured so many of the worst cases of the disease with so little mercury, that he actually distrusted the work of his own hands, and therefore summoned the trustiest of his brethren of the old school, to vouch for the impossibility of the diseases so treated being truly syphilitic.

6. That the train of symptoms following mercurial treatment, has been found more severe and difficult to remove than that which follows primary venereal sores *not* treated with mercury; and that repeated relapses into secondary symptoms are removed with increased facility every time they occur (as if the disease wore itself out) in cases wherein no mercury has been given.

7. That while mercury, through its *accumulative* power, is the best and most powerful *alterative* ever discovered in numerous inflammations—such as the itritic, the hepatic, the dysenteric, the rheumatic, &c.—and is singularly powerful against that resulting from venereal poison; yet, if given incautiously, it tends to undermine the healthy state of the constitution, to establish, in some instances, and in others to aggravate, constitutional diseases—to increase constitutional irritability—to excite inflammation and ulceration in, and to destroy the mucous textures of the body—to promote morbid absorption and removal of the fatty, fibrinous, and osseous substances of the system—and to induce synovial, albuminous, and serous accumulations in the respective cavities lined with the membranes producing such secretions; although, in moderate doses, mercury tends to remove such accumulations.

8. That the extent and injury to the soft and bony parts of the system, arising from the action of mercury, is far more dreadful than any primary or secondary effects of venereal poison.

9. That mercury never was a *specific* against the venereal poison, for relapses were constantly occurring during its fullest operation; nor possessed any virtue in the cure of the disease beyond being the most powerful *alterative* in the hands of the medical practitioner; and that the creed so long believed in (to the ruin of the health of multitudes, through mercurial salivation,) of its indispensability towards the cure, and the destruction of the patient if omitted, is utterly false and groundless; facts which can admit of immediate every-day demonstration in the many thousands of the healthiest British soldiers, *who have been easily, effectually, and permanently cured of every stage of the venereal disease, without ever having taken one particle of mercury.*

The bigoted adherence to a belief so false, and so universal, in which the wisest and most philosophic of our profession blindly participated, will be quoted by after ages as a national reproach; and, as it has indeed already done, will, it is to be feared, go far in destroying our confidence in all medical dogmata, or even doctrines, whatever.

10. That mercury is wholly inadmissible in cases of *sloughing* sores of the penis, wherein there is preceding high inflammation and tumefaction of the parts affected, attended with fever; as it aggravates the local symptoms, and increases continual irritation; *and that mercury is inadequate to the cure in such cases, specific contagion being superseded by violent inflammatory action, which is too rapid in its course*

to be overtaken by the accumulative power of mercury, or by any remedies but *those which act immediately and directly upon the symptoms of danger.*\*

The two following cases may render the 10th and last position intelligible, and otherwise illustrate the treatment of such cases.

*Case 1.*—Dr. Ferguson, of Windsor, (who first drew the attention of the public to the non-mercurial treatment of the venereal disease in Portugal, when he was Inspector General of Portuguese Hospitals,†) attended an officer, in Lisbon, four days after connexion, the whole penis being enormously swelled, of a deep red colour, with malignant looking sores on different parts of the prepuce, and two on the glans penis. He was young, robust, plethoric, and of a sanguine temperament. The skin was hot, the pulse sharp and quick, tongue white, and eyes red, although the patient was living temperately. The catastrophe, Dr. F. well observes, if left to nature, was at hand; or if a few doses of bark, wine, and opium, had been given, his fate would have been inevitable. But he was, most judiciously, treated very differently: *he was blooded largely, the coldest lotions were applied to the penis, he was purged actively with neutral salts, and placed on spare diluent diet.* Under the then almost universal impression among English surgeons, the forbearance of mercury in the above described state, appears to have been as judicious and creditable as it was bold;—perfect success was the reward.

*Case 2.*—R. S. æt. 22, a dragoon, of robust and healthy appearance, although he had suffered, about a year previously, from typhoid fever, was under the author's care in June, 1826, the weather being very sultry. The penis was much tumefied and inflamed, a large sore on one side of the glans, producing great pain, three or four days after connexion. His pulse was strong and full, amounting to 120, his tongue white, and the skin dry and hot. Copious bleeding was immediately practised, full doses of neutral salts and senna were given, together with one drachm of the liquor. antim. tartar. in a saline draught every four hours, and ten grains of James's powder at night. The penis was kept cool with the saturnine lotion. Next day after his admission on the 11th of the month, the pain and fever were diminished. The sore appeared to be sloughed deeply, and the slough was partly separated on the 17th. The day after, all pain and tumefaction ceased. On the 20th, the slough came away, and left a deep excavation, looking florid and healthy. By this time he had no fever, and was in good health. A poultice of poppy-head decoction and conium had

been substituted for the cold lotion, and when the slough came off, the *lotio cupri sulphatis* was applied to the sore. Granulation went on quickly, and early in July the ulcer was perfectly cicatrized. On the 8th of July he returned to his duty.

From the Lancet.

SOME OBSERVATIONS ON THE COMPARATIVE MERITS OF THE DIFFERENT METHODS OF PERFORMING AMPUTATIONS. By GEORGE BUSHE, M. D., of the Royal College of Surgeons in Ireland, and Assistant Surgeon to the Forces.

It is not my intention to enter into a historical inquiry of the progress and improvements that have been made in this branch of surgery, or to mention the accidents and diseases which require amputation; it is merely my desire to show the comparative merits of circular and flap operations, and the advantages and disadvantages of the different modes of executing them, which, as I shall hereafter explain, will principally depend on the state of the limb, and nature of the malady requiring amputation.\*

If we view amputation in a proper light, we shall soon learn that it is an operation of considerable magnitude to the patient, involving him in dreadful suffering, and if not well executed, consigning him to certain misery for the rest of his life. Although these are facts well known to the most humble practitioner, it is by no means uncommon to see a man, who cannot decently despatch the most trivial surgical operation, undertake the performance of amputation. His first object is to girt the limb firmly with a tourniquet, (by which immediate death from hemorrhage is avoided,) and

\* Nothing can be more empirical, than making disease subservient to remedies; but though truly unscientific, we every day see men who should know better, degrade themselves by following this line of practice. For example: one oculist will bleed, &c., in all cases of ophthalmia; form all artificial pupils on the principles of Gibson, and, in every case of cataract, operate with a needle; on the contrary, another man will use solutions, and ointments, as the only remedies for conjunctival ophthalmia; make pupils as recommended by Cheselden, and even attempt to extract membranous cataracts. In like manner, one surgeon is so devoted to the double circular incision, that he performs no other, though his coadjutor, in the same hospital, is bigoted to the double flap operation, and never amputates but after this manner. But, the unprejudiced practitioner will look to the nature of the case, and adjust means accordingly; for only by thus acting can he be of public utility, and honestly beneficial to himself.

\* It has occurred to the author to notice two distinct examples of destruction of the penis—in one case entirely, and in the other reducing it to a short stump—following the application of mercury to sloughing sores on the penis, consequent to inflammation and fever.

† Vide Med. Ch. Trans. vol. iv.

the next, to dismember the unfortunate patient, well bearing in mind, that anatomical knowledge can no more serve him than the best operator, as his great object is to divide every tissue. The vessels being secured, and the wound dressed, it matters not whether the patient has suffered from an ill conducted and protracted operation, or whether he recovers with a sadly formed stump; at all events, the surgeon, who may have thus tampered with human blood, and deserves the scorn of all honest men, obtains amongst his patients a celebrity of the very first rank, for having achieved what to them appears the most serious of all surgical operations. Hence it is, that we every day see men destitute of the slightest surgical dexterity, anxiously looking out for cases upon which they may work a character, thus sacrificing every honourable feeling, and violating all the bonds of humanity; nay, becoming the very workers of cruelty, and stigmatising a most laudable profession, for what? nothing less than sordid ambition. This picture is but too truly delineated, and it is much to be desired that such men would rather employ themselves in obtaining information, which could not fail to excite emulation amongst their cotemporaries, lead the better informed part of the profession to respect them, and compel the public to seek at their hands, that aid which can only be obtained from the well-informed practitioner.

The circulation in the limb about to be removed is commonly restrained by the tourniquet; and to whatever slight deviation has taken place from this established custom, we are principally indebted to Mr. Liston, of Edinburgh, who has written a concise and useful paper on amputation in the *Edinburgh Journal*; and although I cannot altogether agree with that justly celebrated operator, who, when speaking of the tourniquet, writes as follows: "It is, in my opinion, of no use, and, in many cases, worse than useless;" still I wish to give him every credit for vast improvements in this department of surgery; however, to the former part of his statement I cannot at all acquiesce, but the latter is consonant with my own views. It was inculcated by Mr. J. Bell, that it required considerable pressure to restrain the current of blood through arteries of any considerable magnitude; but this opinion has met with its desert, and has been totally annihilated, particularly by those army surgeons, who had the most abundant opportunities of putting it to the test of experiment; and Mr. Liston has not failed to prove to us, that the pressure of an assistant's finger is sufficient to prevent a column of blood passing through an artery, no matter what caliber it may possess. But whilst in hospital practice, or private life, we can procure proper assistants, and personal safety, during the operation, it must not be forgotten, that the army surgeon on the field of battle, can frequently neither obtain a proper assistant, nor personal safety, during the execution of many hazardous operations,

therefore to him the tourniquet becomes absolutely necessary; and, on the same principle, I think Larrey acted judiciously, in securing the inguinal artery with a ligature, before he proceeded to the amputation at the hip joint. In this light, therefore, I look upon the tourniquet as a most useful instrument to the military surgeon; but though it cannot be doubted, that where it is employed, more blood is lost during the operation, than when the vessel is compressed by an assistant; this, instead of being detrimental to the soldier in rude health, must rather serve to prevent that excitement, which often succeeds to primary amputation; so that, on this score, the naval or army surgeon can raise no objections to it. But, as I before stated, I entirely agree with Mr. Liston, when he says, "that in many cases it is worse than useless," because the greater part of the returning blood is lost; and as many patients upon whom secondary amputation becomes necessary, are greatly exhausted by suffering, and the duration of disease; the loss of even a small quantity of blood is an object that should be attended to, and avoided as far as possible. Luckily, such cases are the subjects of operation in hospitals and private life, where suitable assistants can be procured, therefore, I would lay it down as a maxim, that, save in the field of battle, the tourniquet should be dispensed with in all cases of amputation. It may not be useless here, to caution those to whose lot the compression of the greater arteries may fall, not to be over desirous to use great force; for it is well understood that a moderate degree of pressure is sufficient to check the sanguineous current; and that nothing can be more painful to the patient, or tiresome to the assistant, than strong force long continued; and further, there is no doubt but that moderate pressure will more certainly curb the arterial circulation; for, from the great elasticity of the larger vessels, they have a tendency to elude force, though directly applied; this fact was fully demonstrated to me two years since, when the brachial artery was consigned to an over anxious assistant, whilst I amputated the arm. In this case the vessel rolled from beneath his fingers, so that I found it necessary to desist, until he again (with more caution) resumed his duty. Finally, the operator should only be satisfied that his assistant perfectly understands how to compress the vessel, but also that he possesses sufficient presence of mind to continue his calling during the operation. I am particular in this statement, from a circumstance that occurred some time since in my own practice, viz. when about to remove a lower extremity, above the knee, I charged a person with the femoral artery, whom I supposed equal to the task; but when I transfixed the limb, he suddenly let go his hold, and the consequence was, an alarming flow of arterial blood; so that the expedition and evenness of the operation were considerably retarded, by want of capacity in my coadjutor, to assist in a common amputation of the thigh.

The double circular insection is that now generally adopted by European surgeons; and though a few years since, considerable exertions were made in France and Germany, and still more lately in Edinburgh, by Messrs. Liston and Syme, to establish the double flap operation, yet, with a very few exceptions, a strong prejudice exists against it, and it is not common, even now, to see hospital surgeons adopt this line of proceeding.\* This, at first, might lead the inexperienced to entertain but a low idea of its utility, and to practise the circular operation; but I firmly assert, that this prejudice has not arisen from the defects of the flap operation, but an unheeding attachment to the established mode, or circular incision. The natural feelings of man lead him to form prejudiced opinions on matters to which habit has long accustomed him; but it is to be hoped, that the rising race of surgeons will, in some degree, deviate from the principles of their fathers, and, by strict experimental inquiry, seek improvement in this most essential branch of operative surgery. Thus actuated, I commenced; and it has since been my lot to perform many flap operations in public hospitals and private life, in the presence of men fully competent to judge of their advantages and disadvantages; and the very favourable termination of the majority of these cases induces me now to say, that the double flap operation appears possessed of the important advantages laid down by Messrs. Liston and Syme, and that it is adaptive to the greater number of cases. So deeply am I impressed with these sentiments, that it seems to me a matter of surprise, that before now it has not broken the chains of prejudice, and taken its proper place.

As above stated, the advantages of double flap amputations have been clearly and fully explained by Liston and Syme, therefore I consider it quite unnecessary to enter into a detail of them in this place; save that I feel myself called upon to refute an objection that has been started, since the papers above alluded to were published, viz. that from the great extent of surface exposed, union, by the first intention, is not so likely to take place as after the double circular insection; and hence sinuses and tedious suppuration more commonly follow it. To me this objection appears so ill founded, and to be the offspring of such shallow reasoning, that I deem it unnecessary to occupy more time in opposing it than whilst I write,—that the surface of a well formed flap stump, is less than that of a circular one equally well performed; by the diameter of the angles above and below (or before and behind) sacrificed for the rounding of the flaps; however, I do not hesitate to say, that tedious suppuration, and even sinuses, too frequently follow flap amputations; but these consequences are only apt to ensue when the operator, desirous of making a good stump,

has made the flaps too long and pendulous, an error of serious magnitude, but of very common occurrence,\* and it is my belief, that adhesion will more frequently follow well executed flap than circular operations, for the cut surfaces can be kept more easily in contact, whereas, after the circular incision, (no matter how well performed,) the soft parts must be more or less stretched, and a cavity must necessarily exist anterior to the extremity of the sawn bone, which will become a reservoir for exuded fluids, that can obtain no depending outlet.

With respect to the best method of performing flap amputations, there has been a considerable variety of opinion, but the differences may be classed under two heads, viz., the propriety of cutting from within outwards, or from without inwards; the former being principally advocated by some French surgeons, especially Lisfranc; and in this country it has met with strenuous supporters in Messrs. Liston and Syme.—The latter method has had its advocates chiefly in Germany, amongst whom may be mentioned the celebrated Langenbeck, but it has also been practised by some of our military surgeons in the peninsular war. These differences require much consideration, and, in opposition to high authority, I fearlessly say, that there are cases where one plan is preferable to the other; and I cannot help thinking, that their misapplication, added to the difficulty of their performance, constitute the chief and true objections to their more frequent adoption. In proof of this statement, I do not hesitate to say, that those who have witnessed amputation at the hip joint, performed in both ways, will side with me when I write, that the limb can be removed with less danger of hemorrhage, where the incisions are made from without inwards, as then the operator has it in his power to secure the vessels before the operation is performed, should the loss of blood demand it. Again, I assert, that the advocates for Langenbeck's method in the thick part of the thigh, have preferred it, because, where they have seen Lisfranc's mode adopted, the integuments have been too short to cover the divided muscles, and this consequence of transfixing, I have observed myself in primary amputations, especially where the limb has been muscular. I have also witnessed a still greater defect in this respect in two cases, (one of which occurred to myself,) where, from untoward circumstances, primary amputation had been postponed beyond the usual time, so

\* At Guy's Hospital, it is most commonly adopted in respect to the thigh.—ED. L.

\* When I perform the double flap operation, I make the flaps shorter than I have observed in the operations of others; but I always retract the soft parts at their bases, as far as possible, by which expedient the extent of surface is diminished, the stump rendered more plump, and the cut extremity of the bone, or bones, more completely imbedded in muscle.

that the tissues had become tumefied from serous and lymphatic effusions; but, in all such cases, by first dividing the integuments and fascia with a scalpel, they can easily be retracted by an assistant for the extent of an inch or more; then the transfixing knife may be employed for the division of the muscles, &c., with safety, expedition, and success; but in cases where the bulk of the extremity is not great, nor, on the contrary, emaciated or flaccid, (which is oftentimes the case in old wornout persons,) and the parts to be divided are in a healthy condition, transfixing has, in my hands, been most successful; and, on looking to my notes, I find that I have performed eight amputations after this manner.

I have now said much concerning flap operations, but I would not wish to be understood as appearing altogether to condemn the circular incision; for within the last month I have amputated the arm of a delicate female, after the manner of Dupuytren, viz. with a single circular incision, because it was much emaciated, and very flaccid. And from the success of this and another case, that I operated on in the same way, I have but little doubt as to the superiority of this method in flaccid and spent members; for when the soft parts have thus degenerated, union by the first intention rarely takes place. The double circular incision is generally abortive; I have seen more than one case where bad suppuration has followed it, the stump open, and the bone protrude, though here has been abundance of skin, hanging loosely over the suppurating surface. After the double flap operation, the consequences are nearly as unfavourable, and should the wound heal, the stump will be pendulous and flabby. When this operation by the single incision is determined on, an assistant should retract the soft parts to their greatest extent, before the knife is applied; and after the bone is exposed, the muscles should be retracted still farther, before the saw is employed. After the removal of the limb, the operator will find it almost impossible to bring the edges of the divided integuments into coaptation; but this is by no means necessary; "it is directly hurtful," and, indeed in general it will be found sufficient, through the medium of one or more adhesive straps, to give the lips of the wound a proper direction for future coaptation, which, as the tension is reduced by moderate suppuration, and the muscles surrounding the bone begin to decrease in bulk, will be rapidly effected.

With respect to the method of amputating by the double circular incision, there is but one part where I would recommend it in preference to any other; and this is below the knee, in the upper third of the leg, for here there is no necessity for a full cushion to support the weight of the body, and I can fairly say, that I never saw a case where a flap was formed from the calf of the leg, in which considerable retraction of the remaining muscles did not ensue, attended with great induration of the flap, separation of its edge from the

skin on the front of the tibia, sometimes exfoliation of the bone, and generally tedious suppuration. Nor can I speak much in favour of the method recommended by Mr. Syme, viz. of forming an anterior and posterior flap; for before I saw his paper, I once performed this operation in the presence of staff-surgeon Hume, and regret to say, that my success was so indifferent, that I have not since repeated it.

After saying so much on amputation, it may not be irrelevant to offer a few concise remarks on the dressing of stumps.

It is not a little curious, that even now, in some of the first hospitals in Europe, surgeons should continue to envelop stumps in weighty bandages, compresses, dressings, &c.; however it is gratifying to observe that this practice is on the decline, and that a new light dawns on the treatment of wounds after operation; for it is known to every pathologist, that a high degree of inflammation has a tendency to prevent adhesion; and those dressings which must have the effect of preventing evaporation, cannot fail to excite a high degree of inflammation, thus directly operating against union by the first intention. In former times, sutures were employed after amputation, but they are now, with a very few exceptions, abandoned; and indeed most practitioners dispense with them in all wounds where they find it possible, asserting that they produce tension, erythema, and ulceration. This opinion is, in part, correct, and it is my belief, that when sutures are employed to bring parts into contact, they generally operate as above described; but when used only to keep parts in coaptation which easily adjust themselves (as flaps after amputation,) they create much less irritation, and by their retaining the parts more accurately in position, answer the intended end better than adhesive straps, which but too frequently give way in consequence of the exudation from the wound, by their bulk prevent evaporation, and, from the resin they contain, produce cutaneous irritation. Moreover the very act of applying these straps, bears the soft parts back against the bone; and I will venture to say, that those surgeons who may form good stumps, and instead of adhesive straps, employ three or four points of suture, will find the statement I have made in their favour correct. After amputation, I merely place a broad strap of adhesive plaster round the limb, above the wound, with a view of supporting the muscles, and preventing their retraction; then I insert a few points of suture, and place a fold of old linen, moistened with cold water, on the face of the stump. Many surgeons who have seen me thus act, have remarked that the lips of the wound between the sutures were not sufficiently in contact; but by exposure of the stump after ten hours, I have proved to them, that the succeeding inflammation produces the perfect apposition of the intermediate edges, so that, should the number of sutures at first appear insufficient, the experience of one case will

convince any one to the contrary. From the third to the fifth day, I generally remove the sutures, and in their place apply a few narrow adhesive straps; and if any considerable degree of suppuration should ensue, a light compress and roller, wetted with a lotion of camphorated spirit, have appeared to me very desirable.

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# ESSAY ON INSANITY, PRINCIPALLY IN REFERENCE TO THE SEAT AND PATHOLOGY OF EROTIC MONOMANIA.

By Dr. Epps, Lecturer on Materia Medica and Chemistry, &c.

If any matter be an object of interest to the mass of mankind, it is insanity. This is one among many diseases that peculiarly claims the attention of every well-wisher to humanity, as it subjects many of his fellow-creatures to circumstances the most appalling and afflictive. Without casting any poetic imagery upon the matter, it is a malady which occasions the individual suffering under it to be deprived of his liberty, to be placed under the power of strangers, after being torn from the social circle, the endearing objects of home; and finally, and alas, too frequently, to be associated with those

"Whose laugh would make the wisest mad!"

and whose only point of relationship consists in similarity of circumstances. He is not the only sufferer. His family groan under the severest anguish, in contemplating their parent or brother deprived of his reason, and sorrow becomes the inhabitant of an abode where joy previously reigned. And what adds to the horrors of this disease, is the circumstance that persons of the most cultivated understandings are those individuals most frequently attacked; a truth recognised by the common observation, that genius and madness are nearly allied.

But if insanity be an object of general interest, it is one also of particular attraction to the medical practitioner, as he is often required to place himself in a relation to insane individuals the most responsible that can possibly be conceived. He can, on his affidavit, commit a fellow creature to confinement, can separate him from every object held dear; and this upon the conviction that the individual concerned is insane. When this responsibility is considered, the importance of the medical practitioner being well acquainted with the subject of insanity, of his being able to define where insanity begins, and where sanity ends; of being able to point out what kinds of insanity require coercive measures, and what lenient treatment; and of having it in his power to suit his measures most beneficially to the individuals affected, must be clearly seen. In gaining such a state of knowledge, it seems absolutely necessary, that the *morbid state*, constituting insanity, should be perfectly known. Its *proximate* cause should be clear; the varied forms under which insanity appears

must be quite familiar, and a thorough acquaintance with the human mind should be possessed.

To ascertain the proximate cause, pathology and physiology must be diligently studied. Both are necessary; healthy structure must be studied with healthy function; and then diseased structure will be more easily discovered whenever there is diseased function. Beclard truly remarks:—"Il n'y a plus de phénomènes morbides ou de symptômes sans organes altérés, que de fonctions sans organes réguliers, que de phénomènes sans corps, que de mouvement sans matière."\*

Both the pathology and physiology of mental diseases have been lamentably neglected, and in the attempts that have been made towards bringing the same to a state of progression much opposition has been raised, from the influence of the erroneous views at present possessing many minds, exciting individuals biassed by them to cast a veil over fresh facts and new explanations of these facts. Indeed, it is true, in more senses than one, that "light is come into the world, and men loved darkness rather than light." Insanity these opponents have considered as *incurable*, or if not going to this extreme, success in its treatment is merely a game of chance, in which the odds are much against the practitioner. Both these erroneous notions have originated in the metaphysical doctrine of the *separate existence and total independence of mind and matter*: which doctrine is contradicted by every day's experience. The doctrine has, however, had a potent influence; and this being associated with the fact of the mind's *immortality*, has, by this association, gained that influence which it would otherwise never have attained. The mind being thus considered as a distinct and unconnected entity, it was deemed unreasonable to suppose that means acting upon the body simply, could be curative to this essence, this immaterial principle, mind. The poor insane individual was therefore left to the will of Providence, as was plainly said. But this was a child that lazy ignorance begat upon piety.

The mind, according to those who candidly examine all sides of the question, must be looked upon as *associated in its exhibitions with matter*; in fact, it is never manifested but through material organs. This indeed is an opposite opinion; but it seems to be the true one, and it is that which this Essay (the first of a series) has been written to establish, by proving that insanity is an *organic* disease.

In establishing this view, several points will be considered. Definitions and illustrations of some terms will first be given: general remarks, showing the probability of the truth of the position, that insanity is an organic disease; some remarks on the best theory of the facts and the general treatment of insanity; and then some pathological proofs of one particular part of the cerebral system being affected in a particular species of monomania.

\* *Elémens d'Anatomie Générale*, page 121.

1. *Definitions and Illustrations.*

In relation to insanity, many terms have come into use. To have a perfect understanding of the subject treated of, it will be necessary that an explanation of these be offered; for Cicero rightly observes: "Omnis enim, quæ a ratione suscipitur de aliqua re institutio, debet a definitione proficisci, ut intelligatur quid sit id de quo disputetur."

The terms referred to are *mania*, *melancholia*, *idiotism*, *fatuity*, *monomania*, *polymania*, and *insanity* itself.

To the explanation of these it will be necessary to make some general observations.

The mind is that to which insanity has reference. This principle, whatever it be, is known to us only by its phenomena, and these are what are commonly named *feelings*, *desires*, and *intellect*. The two former have been arranged under the *affective*, and the latter under the *intellectual* faculties. This division is important, because in cases of insanity, sometimes the affective faculties are diseased, while the intellectual are not; sometimes the converse, and sometimes both are affected. Instances of these will be given, as they may be useful as references.

First, a case may be mentioned of an individual whose desires were diseased, but whose intellectual faculties were sound. It is related by Pinel, that a patient had periodical fits of insanity, in which he was seized with an "uncontrollable fury, which inspired him with an irresistible propensity to seize any instrument or offensive weapon, to knock on the head the first person who presented himself to his view. He experienced a sort of internal combat between this ferocious impulse to destroy, and the profound horror which rose in his mind at the very idea of such a crime. There was no mark of wandering of memory, imagination, or judgment. He avowed to me, during his strict seclusion, that his propensity to commit a murder was absolutely forced and involuntary—that his wife, whom he tenderly loved, had nearly become his victim, he having scarcely had time to bid her fly to avoid his fury. All his lucid intervals were marked by melancholy reflections and expressions of remorse; and so great did his disgust of life become, that he had several times attempted, by an act of suicide, to bring it to a close. 'What reason have I,' said he, 'to cut the throat of the superintendent of the hospital, who treats us with so much kindness, and yet in my moments of fury I am tempted to rush upon him, as well as others, and plunge a dagger in his bosom. It is this unhappy and irresistible propensity which reduces me to despair, and makes me attempt my own life.' " *Sur l'Aliénation Mentale*. Edition deuxième, p. 102 et 103, Sect. 117. This one case is quite sufficient to show that the affective faculty may be diseased, while the intellectual is healthy.

An example may now be given where the intellectual faculties are diseased, while the affective remain in a sane state. Such is the madness of some individuals, who, having dip-

ped too deeply into metaphysical speculations, have lost their reason so far as to reason most absurdly; also such is that of those who have been engaged in very extensive calculations, and have imagined themselves, though as poor as possible, capable of paying the national debt.

However, the cases of diseased intellectual faculties are much more rare than those of diseased affective faculties.

Perhaps those which occur most frequently are cases in which both the intellectual and affective faculties are diseased. Instances in abundance occur in most works on insanity.

Frequently all the affective faculties are sound, with the exception of one: the same occurs with respect to the intellectual faculties. Such cases come under the head of *mania rai-sonnante*.

These illustrations being given, insanity may now be defined; and the definition, with the substance of which I was favoured by Dr. Wright, of the Bethlem, seems the best, viz. "Insanity is a disease of one or more faculties of the mind, of the diseased manifestations of which the patient is not conscious, or if conscious, is not able to control." Taking this as the *genus*, there are three *species*; one, in which the affective faculties only are injured; a second, when the intellectual; and the third, when both. Of these species we have varieties, *monomania* and *polymania*, the former being applied to a case of insanity when only one of either or both classes of faculties is affected; and the latter when more than one.

*Mania* and *melancholia* seem to be only different *modes* of any one of the species; *mania*, indicating *fury* combined with the insanity; *melancholia*, fear and anxiety.

*Idiotism* and *fatuity* remain to be explained: idiotism is when there is a *deficiency* in the usual number of faculties (this deficiency being *connate*, or born with the individual;) and *fatuity*, when this deficiency arises from the *loss of power* in certain existent faculties, from disease or some injuring cause. So that idiotism must be incurable; fatuity may, in some cases, perhaps, be cured.

#### *General Remarks on the Probability that Insanity is an Organic Disease.*

Some general facts favour the doctrine that insanity is an organic disease. They are the following:—

First, the intimate connexion which subsists between mind and body. The mind, every one is aware, does not attain its maximum of power at once, but, like the body, passes through its different stages successively. In infancy it is infantile; in childhood, childish; in youth juvenile; in manhood, vigorous; and at middle age, at its perfection. Hence again its power diminishes; its activity gradually becomes less, until matter and soul cease to be the companions of each other. Now, it cannot be supposed, that the mind, an immortal principle, can experience such changes; it must, like Hebe, be ever young. It is the corporeal organization, the brain, through which

the mind exhibits itself, that undergoes these changes; a circumstance favouring the view that insanity consists in an organic affection of the brain.

The second probability is dependent upon the fact, that insanity is a *hereditary* disease. It is well known that weak digestions, deranged hepatic systems, and other peculiar organizations, descend from parent to children, and no one hesitates to refer these to organization. Insanity is transmitted in a similar way, and why should not this circumstance be referred to a peculiar state of some organ? and, if any, why not of the brain?

The third probability rests upon the period at which insanity most frequently occurs. The period is between thirty and forty years of age, when all the mental powers are in the highest activity, when organization is in full activity, and when it may readily be conceived, the brain, like any other instrument, will be in a state of peculiar liability to be disorganized, to be put out of order.

The fourth probability is derived from the circumstance that *bodily* injuries and changes often induce insanity. Thus, among the insane, how often do we find those who have been engaged early in dissipation, who have studied too hard, or laboured without repose! In addition, in the case of women, insanity occurs frequently during menstruation, pregnancy, and after delivery, when powerful and peculiar actions are going on in the system, and on account of the sympathy between different parts of the body. It thus happens, that any diseased *state* then exhibits itself, and insanity, appearing at these periods and under such circumstances, can easily be understood as being an organic disease, which previously lurked in the system, but which now, having an opportunity for its exhibition, breaks forth in its most distressing features.

The circumstance of the influence of the *season* and *weather* in inducing insanity, may be referred to the same point. From an erroneous opinion, as has been proved by the observations of Haslam, of the influence of the moon, we have the term *lunacy* (luna, the moon.) Suicide is more common at some seasons than others. Pinel thinks a high temperature excites most readily and frequently the maniacal paroxysms: and mentions cases of remission and exacerbation, corresponding to the changes of the temperature of the atmosphere, from mildness to severe cold.

Another probability may be drawn from the periodical nature of the attacks of insanity. Several organs of the body have periodical activity; and this is ascribed by all to a peculiar condition of the organs themselves. The same may happen, we may conclude, with the brain, and consequently, taking the view that insanity is an organic disease, the peculiar nature of its attacks can be explained.

Other circumstances might be mentioned upon this point, but the foregoing seems sufficient to show the probability of the proposition, and, therefore, the next point of inquiry may be entered upon.

### *Pathological and Physiological Proofs that Insanity is an Organic Disease.*

There is an axiom, held dear by minds constituted in a certain way, which is, "Scire vere est scire per causas." This, as a general principle, is highly important: it distinguishes the philosophic from the common observer; it is the source of all scientific inquiry; the birth-place of analytic examination; the friend to physiology and pathology, and the foe to empiricism.

In bringing forward the proofs that insanity is an organic disease, the following points will be noticed; first, some testimonies as to the brain, generally considered, being diseased in cases of insanity; second, some objections replied to; third, pathological cases referring to a particular species of mania, called *erotic monomania*; the statement of these cases being premised by some physiological facts, showing the intimate connexion between the parts found diseased in the cases cited, where the diseased manifestations existed, which, in the healthy condition, are found to lead to the healthy manifestation of those feelings exhibited morbidly in the erotic monomania.

With respect to the testimonies as to the brain, generally considered, being diseased in cases of insanity, the following remarks may be quoted from Lawrence: "I have examined, after death, the heads of many insane persons, and have hardly seen a single brain which did not exhibit obvious marks of disease; loaded vessels, increased serous secretions, in recent cases: in all instances of longer duration, unequivocal signs of present or past increased action; blood-vessels apparently more numerous, membranes thickened and opaque, depositions of coagulable lymph, forming adhesions, or adventitious membranes, watery effusions, and even abscesses: add to this that the insane often become paralytic, or are suddenly cut off by apoplexy."

"Dr. Haslam states that insanity, in every instance, is accompanied by organic alterations of the brain, and in these alterations, he places the proximate cause of the disease."—*Medico-Chirurgical Review*, July, 1827.

Spurzheim maintains that he *always* found organic alterations in the heads of insane people. M. Georget maintains the existence of these organic alterations.

Dr. Wright, of the Bethlem, stated, "that in a hundred cases of insane individuals, whose brains he had examined, ninety showed evident marks of disease; and the others some signs, such as the bloody points on cutting through the hemispheres."—*Medico-Chirurgical Review*, p. 192, April 26, 1827.

Many such testimonies might be brought forward. These seem sufficient for the purpose: and therefore the objections, consisting of counter-statements, may now be noticed.

Some individuals, who, generally speaking, have not had the same opportunities of observation as those whose names have been mentioned, have asserted that they have examined the brains of insane individuals, and have not

perceived any organic disease. Here, three points present themselves for consideration; first, whether they are capable of making observations; second, whether the persons examined died insane, or had previously recovered and died of some other malady; and, third, whether some diseased states of the brain may exist without any perceptible disorganization or diseased structure.

An individual, in order to be able to make observations with accurate correctness respecting diseased structure, must be well acquainted with healthy structure; so well, indeed, as to be able to discover the *least* variation from the state of health. He must know the structure of the brain. Richerand rejects the observations of Geeding upon the brains of insane individuals, because this observer, according to him, did not sufficiently know the organization of the brain. To the observations of many medical practitioners, the same objection, and, on the same ground, may be made. Another circumstance, totally neglected till of late, tends to render null the objection of those observers who state that they have seen no diseased structure. It is thus that they, thinking the mind acts through the brain as an *individual whole*, look, in cases of insanity, for disease in the whole cerebral mass: whereas, if the views hereafter to be brought forward are correct, disease may exist in one part of the brain and not in another; and the attention not having been directed to the particular part diseased, and the other parts being found healthy, the whole was said to be so.\* This may be said by some medical logicians to be *begging the question*. To such an observation, the reply may be made in the words of one who has done much to deliver the pathology of the brain from that state of backwardness in which it has been for many ages. In Bouillaud's work on Encephalite are the following observations: "A la verité, l'observation des maladies cérébrales est hérissée d'innombrables difficultés. Si le cerveau n'était qu'un seul organe, ou bien si chacun des organes secondaires dont il est composé était affecté isolément, rien ne serait plus aisé que déterminer les rapports entre les symptômes et les lésions organiques: on aurait, pour ainsi dire, un problème à une seule inconnue, et partant, d'une solution aussi simple que possible. Mais puisque le cerveau est composé de plusieurs organes, dont chacun joue un rôle particulier; puisque plusieurs de ces organes

peuvent être affectés à la fois et différemment, et que les phénomènes varient suivant le siège, l'étendue, la nature, les complications de la maladie cérébrale, vous voyez que l'opération devient singulièrement compliquée et que la difficulté de la solution du problème augmente avec le nombre des inconnues." P. xxii. Preface.

Again, it sometimes happens that an insane individual recovers, and dies of some other disease. We may not find in the brain of such an individual any perceptible change in organization: indeed, the organ may have recovered its healthy action; and hence, nothing can be inferred from such cases.

In addition, some diseased states of the brain may exist without a possibility of our perceiving them: that is, as our knowledge is at present. In the same manner, functional disorder of the stomach, liver, kidney, and other organs, exists without our being able to perceive any thing in the structure of those organs corresponding thereto. In amaurosis no disorganization very frequently can be perceived: but who doubts the existence of some diseased state as the cause of the functional derangement?

All these matters must be borne in mind and attended to, before an individual can say that, in cases of insanity, there is no morbid change of structure.

These objections being replied to, the more particular subject of this essay may now be brought forward, namely, *erotic monomania*, which will afford us the most striking proofs of insanity being an organic disease in certain cases to be brought under the notice of the reader. These cases, however, will be better understood, and their force, in establishing the proposition stated, more clearly manifest, when premised by some physiological facts, showing the intimate connexion between the parts found diseased in the cases cited, and the diseased manifestations therewith exhibited, by proving that, with the same parts when healthy, the same manifestations, only in a sane condition, are given rise to. The facts are now to be noticed; previously stating that in *erotic monomania*, the *cerebellum*, particularly the *vermiform process*, is found diseased, and the *sexual organs* are often in a state of activity. It need hardly be mentioned, that excessive sexual desire is the characteristic of *erotic monomania*: this excessive sexual desire, as exhibited by men, called *satyriasis*; by women, *nymphomania*; and being in some cases connected with excessive *priapism*, often with *masturbation*.

The sexual organs have been, and still are, imagined by many to be the source of sexual desire. This opinion, however, is erroneous; the genitals being merely as organs through which the desire, dependent upon the cerebellum, exhibits itself, and fulfils its destination; just in the same manner as the muscles are the ministers of the will, not the source of volition.

That the cerebellum is the source of the sexual desire, is proved by the circumstance

\* Lately an individual, well versed in the examination of the pathology of the brain, was engaged in examining the brain of a person who died of excessive masturbation. He perceived, slicing the brain in the usual way, no organic disease. A gentleman present, who considered the cerebellum to be the organ of the sexual desire, turned the attention of others present to this part of the cerebral mass, which was found to be diseased. No doubt the first gentleman would say the brain was healthy.

that whenever it is large the sexual desire is proportionably strong; when small, is proportionably weak. Some have the organ very large, and such persons are recommended in scripture to marry, for it is better "to marry than to burn;" in other words, it is better to give an outlet by marriage to the impulses of this faculty than continually to have its impulses exciting the mind, and inflaming the individual. Others, we are told, are "eunuchs from the mother's womb;" that is, not that they have no genital organs, but that their sexual desire is so weak, from the cerebellum being small, that they may indeed be said to be eunuchs.

Again, it is a well known fact, that a violent desire for sexual intercourse is often preceded by a pain in the cerebellum, which is relieved by sexual gratification. Many young people complain of pain at the back of the head about the period of puberty. When the catamenia are either excessive or deficient, and even when coming on, females often complain of pain in the back of the head; which is relieved by the natural evacuations taking place, being moderated when excessive, increased when deficient. The sexual desires have existed when the genital parts have not been developed, as in children of two, three, four and six years of age.—Gall, *Sur les Fonctions du Cerveau*. Tome iii. Again, old men, whose genital organs are paralysed or inefficient, are often tormented with desires. And what is still more conclusive, individuals having no genital organs at all, have had the sexual desire most powerfully active; as women, who have no wombs.—Gall, lib. cit. *Memoirs de la Société de Paris*, par Professeur Caillot. Case in Guy's Hospital recorded in the *Lancet*. And the converse has occurred, that those who have had the genital organs perfect have not had the sexual desire: "The mother of one of Napoleon's generals, as well as of other children, told a friend of mine, 'Qu'elle n'avoit au que le douleur d'enfanter.'" And the late Dr. Heberden has the following passage: "Duo mariti mihi narrarunt uxores suas in venerem fuisse frigidas, omni ejus cupiditate et voluptate carentes: sæpe tamen gravidas factas esse, et recte peperisse." Gall has known similar cases.—Elliotson's *Blumenbach*, p. 44.

Many other circumstances might be cited to show that the sexual desire does not depend upon the genital organs. Still, however, though not dependent, there exists an intimate connexion between these organs and the cerebellum, held to be the source of the sexual desire. A few facts illustrative of this connexion may be mentioned; and first, with respect to the dependence of the genital organs on the cerebellum.

Voluptuous dreams, which certainly are mental actions, often excite an ejaculation of semen. Spirituous liquors, which excite the energies of the mind, have a powerful effect in inducing venereal desires. Baron Larrey mentions the case of a soldier who had received a sabre cut on his nucha, and who lost thereby the venereal impulse. Hippocrates as-

serts, that the Scythians rendered themselves impotent by cutting the veins which are behind the ears. Brantome, in his memoirs, (vol. ii. p. 182,) mentions the case of a person of the name of Burie, who, from a blow on the nucha, in battle, lost all power of seminal emission. Dr. Formey had a case, where the patient, from a wound on the nucha, had excessive erections, followed by complete want of power, which he recovered after six months. Larrey gave Dr. Gall several similar cases. Some facts may now be mentioned showing the connexion between the genital organs and the cerebellum, the genital organs being the parts affected.

Castration has a very powerful effect upon the cerebellum; so much so, that if the animal be castrated early in life, the cerebellum does not increase in size, and is never troubled by the sexual desire; but if the castration be performed late in life, the cerebellum having attained its full size, the desire exists without the ability to gratify the same. Any one to perceive the effect of castration, need only examine the skull of a cock and a capon; of a bull and of an ox, and the difference of size in the region of the cerebellum will be immediately apparent.

But what is still more particular in showing the connexion between the genital organs and the cerebellum, is the influence of *unilateral* castration. Several cases are related by Dr. Gall, in his work already cited, of individuals, who, having lost the testicle of one side, had the opposite lobe of the cerebellum diminished, indicated by one side of the head in that part being flat, while the other was bossy. This celebrated individual performed, in addition, numerous experiments on animals, in which he found, that when an animal was castrated on one side, the opposite lobe of the cerebellum diminished; that is, if the right testicle was taken away, the left lobe of the cerebellum was affected; if the left testicle, the right lobe.

Cases have occurred, moreover, where, from sabre cuts in the region of the cerebellum, atrophy and softening of the testicles took place.

These general remarks being made, the cases showing that, in erotic monomania, the cerebellum is diseased, and that therefore insanity is an organic disease, may be brought forward.

The symptoms of erotic monomania are merely diseased manifestations of the feeling of the sexual desire. Excessive lustfulness of action, look and word indicate it. Persons under this disease often talk of building temples to Love, &c.

CASE 1.—A boy, aged 13, gave himself over to onanism. This induced paralysis of the lower extremities, loss of sight, and, finally, death. On dissection, the lobes of his cerebellum were found to be filled with pus: the left lobe being affected more than the right; both testicles were small; that of the right side was almost gone, and of a very soft consistence. The left lobe of the cerebellum was most affected.

CASE 2.—A man, aged 32, brought at night

to the Hôtel Dieu, having been found by the national guard on the quay with some prostitutes, one of whom stated, that in the act of coition, having drank liberally before, he fell down in the state to be described. His face was extremely red; heat of the head and the neck very considerable; pulse 90; respiration interrupted; occasionally convulsed; genitals hot; extremities cold. Thirty leeches were applied; he was bled from the feet. The patient died. It was imagined that he had taken cantharides, from the state of the genital organs; but none of this substance was detected. All present at the dissection were struck at the *inflamed appearance of the cerebellum*. The hemispheres of the brain presented no trace of organic alteration; the anterior corpora quadragemina were slightly inflamed; the posterior much more so; the *processus cerebelli ad testes* were of the same colour; the *hemispheres of the cerebellum* were much injected, but much less than the *superior vermicular process*.

This case is given by M. Serres. This extraordinary pathological condition, as he calls it, brought to his remembrance the doctrine of Gall, that the cerebellum is the organ of the sexual desire; and he straightway proceeded to examine whether Dr. Gall's opinion was established by analogous cases. He states, that he remembered two cases of cerebellic inflammation, where erections and abundant ejaculations continued during their duration; and adds, fresh facts have confirmed the view originally taken; namely, that the cerebellum is the seat of the sexual desire. He affords no other cases.

CASE 3.—Thomas Marianne, aged 55, sanguineous temperament, very much addicted to venery, was brought to the hospital, having all the symptoms of cerebellic apoplexy: as concomitants of which, considerable *heat of the neck* and abundant ejaculation at the end of the paroxysm, swelling and vivid redness of the genitals, may be mentioned. He died. The substance of the brain presented nothing particular; the *cerebellum* was redder than natural. When the *superior vermicular process* was cut, the action of the air gave it immediately a lively red colour, and all the white substance seemed as if macerated in blood. Thick, black blood was found in the process.

CASE 4.—Nicolas Bourgoïn, aged 46, was brought into the hospital, May 17, 1818. Nothing was known of his previous condition: but the symptoms indicated violent cerebral apoplexy. He had erections, tension, and swelling of the genital parts, abundant ejaculation of semen during the night, apparently at the end of the paroxysm. M. Serres predicted that the cerebellum would, at death, be found injured. Satyriasis became very violent, and the patient died; the volume of the cerebellum was found to be larger than ordinary; the *cerebellum* was very red; clots of blood were found in the *superior vermiform process*. Drs. Edwards and Lisfranc were present at this dissection.

CASE 5.—A man, of the name of Gembar, aged 52, received into the hospital of la Pitié, March 5, 1819. He was insensible; paralys-

ed; the penis in a state of constant turgescence, and occasionally erected. From these and other symptoms, it was concluded, that there would be disease of the cerebellum. This was found to be the case; the left hemisphere of the cerebellum was diseased, and the *superior vermicular process*.

Taking a view of these cases, M. Serres concludes, that the priapism had been produced by the lesion of the cerebellum; and that if these lesions have not been observed, it has happened from the want of attention on the part of physicians.

CASE 6.—This is related by Dr. Falret, in testimony of the truth and accuracy of M. Serres's diagnostic. The symptoms of this case indicated cerebral apoplexy; the penis, in addition, was swelled, stretched, red, and in a state of erection. The cerebellum, on dissection, was found to be injected; the *superior vermicular process* was diseased.

CASE 7.—John Baptist Francis Guardier, aged 42 years, very much addicted to venery, was brought, on the 29th of January, to the hospital, insensible; face red and swelled. His wife stated, that his penis had been in a state of erection all the night; there had been an abundant emission of semen: the neck was swelled, and there was great heat in the occipital region. In the evening, a paroxysm occurred, the penis became erect, and he applied his hand to this organ. Though ice was applied to the penis, it still retained its erection. He died.

On dissection, the cerebellum was found larger than usual, and the vertebral artery of a large caliber; the anterior part of the *cerebellum* was corroded, and the superior vermiform process seems to have been implicated.

CASE 8.—John Charles Montagnon, aged 50, was seized with the symptoms of cerebellic apoplexy; in him the *cerebellum* was found diseased.

CASE 9.—Mary Jane Josephine Doubourg, aged 33, early addicted to venery. Not satisfied with the common sources of gratification, she delivered herself up to the vice of masturbation. She, to be relieved from the state which was induced, submitted to have the clitoris removed: she became imbecile at 32; died at la Pitié. On dissection, the superior and inferior vermicular process were found diseased and hardened; little ulcers were here and there; the cerebellum was softened, and had lessened in size. The cerebellic arteries were extraordinarily developed, as were the hypogastric, the uterine, the vaginal, the urinary, and the hæmorrhoidal.

CASE 10.—This is a case with which the Phrenological Society was favoured by Dr. Elliotson, communicated to him by Dr. Clarke, who sent a drawing, wherein the inflamed appearance of the *superior vermicular process* was very apparent.

CASE 11.—This is a case of chorea; of which disease an insane individual died in Bethlem Hospital. He was completely bestial: the *superior vermiform process* was in him found diseased.

CASE 12.—This is the case of a young man, coming under my own notice, who died of satyriasis; in him the *cerebellum* was found diseased.

CASE 13.—This case is one of a young lady, coming under my notice, who died of nymphomania, and in whom the *cerebellum* was, in some parts, converted into pus.

CASE 14.—This, the last that need be mentioned, is given by Magendie in one of his last Journals, in which case the *cerebellum* was found diseased.

It will be perceived, that these cases are extremely curious. They convey much information, and give much room for conception. The paralysis generally attendant upon the disease; the ejaculations of semen; the heated condition of the neck; and the disease of the superior vermiform process and of the *cerebellum*, are matters which justifiably claim the attention.

In conclusion, it may be added, that Dr. Monro, speaking of the cerebral mass, says, "Abscesses are sometimes found in the brain; more frequently in the *cerebellum*;" (Anatomy, vol. iii. p. 218.) a fact, which corresponds to the powerful and injurious predominance of the sexual desire.

Such are some cases which can, at present, be presented to the reader, as proofs that in erotic monomania, the *cerebellum*, particularly that part called the superior vermiform process, is the part morbidly affected. Such cases will accumulate, and I have no doubt that I shall have the privilege, ere long, to present many such cases to the notice of the reader.

Thus it seems to us to be proved, that insanity is an *organic* disease; and that in this particular disease, a particular part of the cerebral system is affected. We shall hereafter find, that other diseases are connected with certain other parts of the cerebral system; and of this, as a fact, the essayist has no doubt, and he trusts that the same will be perfectly clear to others, when the facts are brought forward. We now therefore leave the *pathology* of erotic monomania in order to consider its *treatment*.

*Treatment, &c.*—In the treatment of erotic monomania, there are two points to be considered; first, whether the disease be *simple*, that is, uncombined with any other disease; and second, whether it be combined. If simple, the treatment will be different from that when the erotic monomania is complicated. To illustrate these views, it may be stated, that, in many cases of nymphomania, we have, as an attendant, in females, a suppression of the menstrual discharge. This is an effect produced, and often, by removing this effect, we remove the erotic monomania. In such complications, I have found the most happy effects produced by the application of from twenty to thirty leeches to the labia. These have acted, as it is vulgarly said, like a charm. And this, as a fact, be it remarked, has been urged against erotic monomania being a *cerebellic* disease, since the disease being removed by this application to the genital parts, proves, according to the objectors, the malady to be

dependent upon these parts. The fallacy, however, of this objection is easily seen. Often from a running sore being healed apoplexy takes place; and often threatened apoplexy is prevented by opening a sore that has been closed. Now who would say, that the state of the parts inducing apoplexy is that of the sore? The application is quite clear.

The attention may now be turned more particularly to the disease of erotic monomania, viewed as simple. When it is said, viewed as simple, it is to be understood not as conveying the meaning that there is no other affection of the system, but no one so predominating as to form an object in the treatment any higher than a mere accessory symptom.

In many of the cases above related, the means are stated by which the monomania was attempted to be relieved; and the effect which leeches, applied to the upper part of the neck, produced, shows that we may justly have some dependence upon such means so applied. It has also been seen that often the termination of a paroxysm was produced by an ejaculation of semen. May not this deed of nature be taken as an indication? And might we not, justifiably, in such cases, have recourse to means exciting the seminal discharge, and then in the intervals have recourse to those means which might stop the increased and diseased action in the *cerebellum*? I have known, in some cases, the malady removed by the application of leeches, and the use of those means by which a determination of action is induced to other parts. In support of this practice of local bleeding is the fact already noticed, respecting the Scythians rendering their people impotent by bleeding from the veins at the back of the head.

I have never found any benefit from the use of blisters immediately to the part. Indeed, blisters, setons, frictions with volatile substances, often excite increased action in the genital parts. Whereas, on the other hand, priapisms, erections, and seminal ejaculations have been relieved by cold lotions applied to the nucha.

Such are some views respecting the treatment of this species, which is too common, and which I look upon as curable in the generality of cases.

Other treatment is also useful; but that noticed has been brought forward principally because it has reference to the particular views respecting the *seat* of the disease.

Such is a brief outline of the subject of *erotic monomania*; and the remarks made, it is hoped, may have the effect of turning the attention of medical men more particularly to the pathology of insanity, on which subject the essayist will be most happy to receive any communications.

It was proposed at the outset to give some views on the treatment of insanity generally, and on the *theory* of that treatment. These subjects may, perhaps, be advantageously postponed until the other facts respecting other diseases, as dependent upon *cerebral disease*, are brought forward. A conclusion may there-

fore be made by observing, that the treatment of insanity which is most beneficial, is that which acts upon the malady as an organic disease, and also as founded upon the opinion that the mind exists in different states, and exhibits itself through a plurality of organs.

\* \* Some cases of epilepsy were mentioned at the late discussion on insanity at the Westminster Medical Society, wherein the cerebellum was diseased. In this I can readily believe; and the circumstance is not at all opposed to our proposition, inasmuch as the cerebellum evidently has some general functions in addition to the peculiar function which we assign to it. This appears likely, because the *lobes of the cerebellum* exist in the mammalia only; in fishes, amphibia, many insects, and in birds, the *processus vermiciformis* is the only part existing; and as this exists in all animals having the sexual desire, it is said to be the *primitive position*. I am not at all satisfied as yet with respect to the general functions which the addition of the two cerebellic lobes confers. But, that motion is in some measure concerned, especially the motions taking place in coition, I am inclined to believe from the facts, that Dr. Gall has traced fibres from the part of the spinal marrow supplying the parts of generation with nerves, to the cerebellum; and, lately, that a gentleman, who has been excited by artificial means to excessive venery, has become epileptic. May not then epilepsy be somewhat connected with the cerebellum? And will not these views afford some explanation of the fact above mentioned?

From the Transactions of the Medico-Chirurgical Society of Edinburgh.

**CASE OF ANEURISM OF THE AORTA, WITH DISEASE OF THE SPINAL CORD.** By THOMAS MOLISON, M. D. Fellow of the Royal College of Surgeons, and one of the Surgeons of the New Town Dispensary.

Stewart, a chairman, æt. 34. Since the middle of January last has complained of frequent uneasiness and pain in both sides, more particularly the left; has also, for three or four months, been troubled with difficulty of breathing, upon any considerable exercise; and lately has had much difficulty in swallowing solids. Since the middle of April he has not been able to carry any considerable load. Yesterday he carried to Newhaven (distance one mile and a half) a bag of about twenty pounds weight, and bathed in the sea whilst warm; he had been in the water about a quarter of an hour, when he became very confused, and on coming out, fell amongst some stones; he then bled profusely at the mouth and nose, and, from that moment, lost all voluntary motion and sensation of his upper extremities. He was taken up and carried to an adjoining house; was afterwards put into a warm-bath, and, in the evening, was bled at the arm, and blistered on the nape of the

neck; but passed a very restless night, with frequent shivering fits, which were reported to resemble convulsions.

16th July.—To-day he was brought to town; had vomited fluid and coagulated blood several times since last night: and when I saw him, at 5 P. M., he had just brought up a large quantity of fluid blood, which afterwards coagulated. He answered questions distinctly, although previously he had talked incoherently; he seemed somewhat lethargic; I therefore spoke loud, and repeated my questions until answered. He said that he was sore all over; but added, that the pains were most severe in the head, chest, and bowels. His features were expressive of much anxiety, and he almost constantly tossed about his head and legs, while his arms were cold and motionless, having a death-like pallid appearance. When he moves his extremities, he invariably contracts the muscles of the mouth, as if making some great exertion. I requested him to squeeze my hand; he then threw the muscles of his face into action, and said he squeezed with his whole force, while not a muscle of his arm moved, nor was he sensible there when pinched. Pulse 120; weak at the wrist and ankles, but much stronger proportionally in the carotids, where they could be seen beating at the distance of five or six feet. No palpitation at the heart; breathing natural; heat of the head augmented, but greatly diminished in all the extremities; considerable thirst; tongue very white; pupil natural; skin dry; complains of passing his urine involuntarily.

8 P. M. Seven or eight ounces of fluid blood have been discharged by the mouth.

11 P. M. Says he is still very unwell, and tosses about his head and legs. On introducing the catheter, he used great force with his legs.

17th, 8 A. M. Has slept none, retains his urine, and passes it voluntarily.

11 A. M. Has passed two very dark bloody alvine evacuations.

9 P. M. Does not move his head and legs so often.

11 P. M. Breathes softly, and seems in a quiet sleep; he received an injection with fifty drops of laudanum two hours ago.

18th. Slept several hours last night; about four o'clock he talked quite rationally; in a few minutes after, hemorrhage occurred at the mouth, and during this he expired.

20th July, 12 noon.—*Dissection*.—The body is rather muscular, with little fat. Heart and lungs natural; some slight adhesions between the pleuræ. An aneurismal tumour, of the size of a small orange, was observed on the left side of the spine, at the upper part of the root of the left lung. It freely communicated with the aorta and œsophagus, opposite to the fourth and fifth vertebres; indeed, in this part, almost the whole caliber of the œsophagus was destroyed. The left side of the bodies of the third, fourth, fifth and sixth vertebres were quite bare, and absorption had proceeded in many parts to the depth of a

third of an inch. The stomach contained a large, firm coagulum of blood, with prolongations of several inches into both œsophagus and duodenum, of which cavities it had taken an exact shape. The colon was greatly distended with air, and was of a deep dark colour, from the blood it contained. The small intestines were natural, with only some dark spots from the same cause.

Having removed the posterior arches of the three inferior cervical and of the whole of the dorsal vertebres, we observed the cellular substance on the outside of the dura mater of a much darker red than usual, and slightly infiltrated with a bloody purulent looking matter. Between the third dorsal vertebre and the sixth, there existed some extravasated blood immediately underneath the dura mater. Above the second dorsal vertebre the cord itself was firm, and appeared perfectly healthy; but below this, it gradually became soft and yellowish; and opposite to the fifth, sixth, seventh, and eighth, its substance assumed both the consistence and colour of thick cream, with no remains of organization. The cord at this part was so carefully inspected, that, if any medullary band had existed amongst the soft matter, it must have been seen. Three or four of the vertebres were in many parts denuded of their soft covering, where they form the anterior bony parietes of the cord: in some parts they were merely rough; in others, the absorption of the osseous matter was considerable. Nowhere was there any sensible deposition of lymph between the dura mater and arachnoid. The head was not opened; and we had much difficulty in obtaining liberty to open so much of the spine, which was done fifty-eight hours after death.

The treatment was sinapisms to the legs, which caused some heat and pain; the head was shaved; and cold water was frequently applied at the request of the patient, who said it relieved the pain. Turpentine and laudanum enemata were likewise employed: two of the former remained twelve hours, but did not seem to assist the evacuation of the dark bloody matter.

We have no hesitation in attributing the disease of the spinal cord to the aneurism; for, although the absorption was most extensive on the anterior surface of the bodies of the vertebres, yet the disease seems to have passed through these spongy parts, and to have showed itself in the same process of absorption on the posterior surface, although there could be here no pressure from the aneurism, which is generally considered as the cause of the absorption of the osseous matter. It is more than probable that the disease of the spinal cord had existed for some considerable time previous to the bursting of the aneurism, and, of course, that all the sudden serious symptoms were owing to the great loss of blood.

Softening and disorganization of the dorsal portion of the spinal cord are commonly attended with convulsions of the lower extremities, and followed by paralysis: but still some few cases are on record where this portion of

the cord has been found destroyed, and yet no paralysis has supervened. I may mention the case of a gun-shot wound, which occurred in the practice of Desault, where the spinal cord was completely divided opposite to the tenth dorsal vertebre. M. Janson, of Lyons, also mentions a case, where the inferior dorsal portion of the cord was converted into a pulpy matter. Olivier, in his work on the Diseases of the Spinal Cord, records a case of M. Van-de-Keere, where there was extensive caries of the bodies of the dorsal vertebres, with complete absorption of the cord, between the ninth dorsal vertebre and first lumbar. The termination of the superior end was bulbous, whilst that of the inferior was compressed backwards. The colour and consistence of the cord remained unaltered. - In Desault's and Van-de-Keere's case, there was not only no paralysis, but perfect sensation remained in the lower extremities until death. From the impossibility of explaining the above cases, M. Andral *junior* is inclined to doubt their accuracy; but if medical men were to draw such inferences, I need scarcely say how injurious it would be to the future improvement of our profession.

Magendie, who is exceedingly ingenious in many of his observations, endeavours to explain the difficulty, by considering the pia mater as one of the organs or conductors of sensation. Dreading that our patient Stewart might have hurt the spine in his fall on coming out of the water, for he could give no very distinct account how he fell, I carefully felt all the spinous processes, and tapped them firmly with the points of my fingers, but he said he had no pain there, nor ever had. M. Olivier states, that the most constant symptom of this disease is an excessively acute deep seated pain, accompanied with a feeling of burning heat along the spine, greatly exasperated by motion; and adds, that all authors who have written on this disease, with the exception of M. Pinel *junior*, who has mentioned two cases, have noticed this pain in the back. M. Andral\* however says, that this pain has been remarked in a very few instances only, and adds, that it still remains for further experience to point out how far this local pain indicates the existence and seat of the inflammation of the cord. Certainly Stewart never complained of this local pain in the spine, not even when he was freely moved.

That general convulsions may have occurred in this case is possible, as the friends declare, that, during the first night after bathing in the sea, his face was frequently twisted to one side, with fixed staring eyes, and that then it required several persons to hold him in bed; but certainly at no time when I saw him did any thing resembling convulsions occur. He frequently threw the muscles of his face into action on turning over his legs, or drawing them up, but this was evidently a voluntary motion; and at no time afterwards were his lower extremities paralysed.

\* Dict. de Médecine, tome 18.

Regarding the paralysis and loss of sensation of the upper extremities, I can give no explanation, nor am I aware of a similar case on record.

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From the Edinburgh Medical and Surgical Journal.

**CASE OF OBSTRUCTION IN THE ARTERIES FROM AN INTERNAL CAUSE.**

By JAMES SYME, Esq., Fellow of the Royal College of Surgeons in London and Edinburgh, and Lecturer on Surgery in Edinburgh.

On the evening of Tuesday the 22d January, I was asked to meet Mr. Lizars in consultation, on the case of Alexander Young, a farm-servant on the estate of Riccarton.

I found the patient a thin, rather unhealthy looking man, aged 58. His pulse was 90, soft, and moderately full. His tongue was moist and clean. His mind, though easily agitated, was free from delirium or confusion. His countenance expressed much anxiety and pain, which he referred to the left foot and leg.

About the middle of this leg, on its outer side, there was a large aperture in the integuments, which exposed to view the anterior tibial and peroneal muscles in a state of mortification. A bright and diffused redness bounded this part superiorly and laterally; while all the limb below on the same side, as far as the ankle, was cold and livid. There was a large slough in the centre of the sole of the foot, and almost all the toes were black either on their sides or extremities. On several parts of the instep and inner side of the foot we also observed those discoloured marks so well described by Mr. Pott as precursors of the mortification which occurs in old people. The spots in question seemed to be owing to death of the cutis and separation of the cuticle, without any effusion of serum to raise it into a vesicle.

I was told that in the beginning of December he had suffered from pain in the left side, together with oppression in breathing, for which complaints he had been repeatedly blooded and blistered. He was much relieved by these measures; but on the 30th of the same month he had a relapse,—complaining of pain in the right side under the false ribs, shivering, and uneasiness in both legs, but particularly the left one. On the following day he was again blooded from the arm and leeches on the right side. A blister was then applied to the chest, and repeated sinapisms to the foot. The left leg being then painful, was subjected to warm bathing, friction with liniments, sinapisms, leeches, and, lastly, a blister, which was soon followed by sloughing of the skin over which it had been placed. About the same time a blister was applied to the sole of the foot, which also was succeeded by death of the part concerned. An incision was afterwards made through the dead skin of the leg, and a *seton* introduced. The mor-

tification had thus existed about a fortnight previous to the time I saw him, and had not extended upwards.

Having fully considered all the circumstances now related, we were unanimously of opinion (*viz.* the practitioner who had treated the case from its commencement, another gentleman who had been consulted after the gangrene appeared, Mr. Lizars, and myself) that the disease, if allowed to proceed, must inevitably and speedily destroy the patient; that there was reason to think the mortification might in part be owing to the severe treatment which the limb had received in the first instance; and therefore that amputation afforded, though certainly very little, nevertheless, some chance of escape from destruction otherwise certain.

This opinion being distinctly explained to the patient and his friends, it was agreed that he should submit to the operation, which, with the assistance of Mr. Lizars, I accordingly performed above the knee.

On examining the limb, which we did very carefully the following day, we found the artery, where cut across, quite pervious and apparently sound in its coats. On entering the popliteal cavity it became firmly obstructed by a dense white coagulum, like that lining old aneurisms, and continued to be so all the way to its division into the peroneal and tibial arteries, which also were obstructed to the extent of an inch and a half from their origin; beyond this they were pervious and apparently healthy. The obstructed vessel seemed somewhat contracted, particularly about the middle of the popliteal portion; its coats were here also thicker than usual, and adhered to the clot. We could not discern the smallest speck of ossification or other morbid degeneration in any part of the artery. The vein was much thickened in its coats, so as to resemble an artery, but was not obstructed, or in other respects different from its usual condition.

As the patient bore the operation well—was freed from the distracting pain which he previously suffered—and continued to do well for several days, I flattered myself with having struck at the root of the evil. On Sunday the 25th I saw him for the first time after the operation. His appearance was then much improved, and nearly half of the stump had healed by the first intention; but his pulse and respiration were very unsatisfactory; the former being 120 and undulating, the latter also much hurried. On Monday he complained of pain in the chest. On Wednesday night he died.

The dissection was performed on Friday, in the presence of all the gentlemen who had been concerned in the case.

The remaining inferior extremity was free from any appearance of disease.

On the outer side of the stump, all the way up to the hip, the cuticle was detached, and the muscles separated from each other by a purulent effusion.

The external appearance of the lungs was

quite healthy, excepting a little emphysema at the upper part, but their texture was remarkably softened, as if they had recently suffered from inflammatory action. This change was particularly observed at the posterior and lower part.

In the substance of the spleen there was a large abscess, the parietes of which were strengthened by strong adhesions to the diaphragm, stomach, &c.

The heart was extremely soft, so that the finger could be pushed through its substance with great ease. The arch of the aorta was not at all dilated, nor altered in the textures of its coats, but its inner surface was very red, and seemed as if not so smooth as usual, having, a velvety sort of aspect, and appeared, moreover, somewhat softened. The abdominal portion of the aorta was filled with dark half-coagulated blood, but retained its usual colour. The right internal iliac artery was firmly obstructed from its origin, as were the right and left internal iliac veins. The left femoral artery was pervious all the way to its extremity, where the ligature remained, but the profunda and corresponding vein were firmly obstructed, as were also the same vessels of the right side. The right femoral was pervious, but the popliteal was obstructed precisely as I have already described the left one, which was amputated.

We searched carefully for traces of chronic disease in the arterial system, but could find none; not the smallest trace of osseous or other degeneration. I have since found a small scale at the origin of each internal iliac, but how inconsiderable, and how little affecting the inner surface of the artery, will appear from the fact of its having been overlooked at the dissection, when some of the parties present were very diligent in searching for such morbid changes.

I do not recollect of ever dissecting a subject so far advanced in life, whose arteries were in a more healthy condition as to texture.

My opinion of the case is, that it was one of acute inflammation of the arteries, similar to the one recorded in Mr. Hodgson's Treatise on the Blood-vessels, and similar to those very rare cases in which the vessels were obstructed from effusion consequent on inflammation of their coats.

The different obstructions found at the dissection must either have existed at the time of the operation or taken place after it. If they did not exist then, there was nothing to prevent the operation from being successful, and if they existed when it was performed, then it must be allowed that the collateral circulation which had sufficed so long might have nourished the limb permanently; so that, in either view of the case, the operation afforded a chance of recovery.

As to the general question respecting amputation on account of gangrene, I certainly think that the operation is not proper when the disease commences without any external cause. Thus a few days before the case

above related happened, I was consulted respecting a patient about 40, whose foot and leg had mortified, without any apparent cause whatever; but though the friends were very anxious to have the limb removed, I would not consent to do so, as there could be no doubt that the disease would continue to extend after the operation.

In conclusion, I venture to suggest that obstruction of arteries, as a consequence of effusion from their own coats, may not be so rare an occurrence as it would appear to be, from the extreme rarity of such cases on record. It has often been remarked that the vessels leading to or from a mortified part were rendered impervious by coagulum; and this circumstance has hitherto been explained loosely as a consequence of the mortification, as an effect of nature to prevent hemorrhage. But it is well known, as I can testify from repeated observation, that the vessels are not always obstructed, which they ought to be if the obstruction were the effect of mortification. And, therefore, I think it may reasonably be asked, is not the obstruction rather the cause of mortification in those cases where it exists? The coagulum certainly does not look like one resulting from mere stagnation of blood in the vessels; since, instead of being soft, bloody, and *smaller* than the caliber of the vessel, it is firm, white, and closely impacted, so as to render it difficult to lay open the vessel without injury to its contents.

I have long been satisfied that ossification alone is not sufficient to occasion mortification, since, were it so, seven people out of ten who have passed the age of sixty ought to suffer from mortification of the toes; and I have no doubt that, in addition to the ossification, there is some farther obstruction, as from contraction of the vessel, of which there is a specimen in my possession, the aorta hardly admitting the little finger; or from coagulation, of which also I have a specimen, in the femoral artery of an old man who died of this miserable disease. The vessel is completely plugged for the extent of five or six inches, by an exceedingly firm white fibrinous coagulum.

From the Archives Generales de Medecine.

**SPINA-VENTOSA DU CUBITUS ENVELOPPE PAR UNE ENORME TUMEUR SQUIRRHEUSE ET ENCEPHALOIDE; double altération développée dans l'espace de six mois à l'avant-bras droit.** Par le docteur OLLIVIER.

Jean Morille, æt. 24, a weaver by trade, entered the hospital at Angers March 11, 1819. He possessed a robust constitution, and had always enjoyed perfect health until the preceding July, when he began to complain of fugitive pains in the right forearm. In a short time the pains became fixed at the upper and back part of the forearm, near the elbow, and continued without mitigation till September, when a slight swelling was per-

ceptible in the part, which from that time occasioned constant and distressing insomnia. The tumour sensibly augmented in volume, extending along the posterior part of the ulna; it was hard, the integuments presented their natural colour, and the motions of the ring and little fingers were impeded. In January, the surgeon in attendance, perceiving a fluctuation in the spot where the tumour had first made its appearance, made an incision into the part about an inch in length, which bled freely, and the hemorrhage could with difficulty be arrested by strong compression. The wound readily healed, but the progress of the tumour was subsequently more rapid than before. Cataplasms of green wheat were applied upon a part of the surface of the tumour, and the skin irritated thereby, lost its ordinary whiteness, and assumed a livid hue.

When the patient entered the hospital, the tumour had existed six months; its form was that of an ovoid, flattened before and behind; transversely its greatest circumference was twenty-one inches, and twenty-six, measured longitudinally; it extended from the inferior fifth of the arm to within three fingers' breadth of the wrist, occupying more particularly the anterior and inner part of the forearm, so that the radial side was not covered by it. The elbow joint appeared to be enveloped by the tumour; the skin possessing a violet tinge, was shining, greatly distended, and slightly ulcerated at the spot where the incision had been made, but was unaltered in every other part. A pretty strong degree of compression occasioned only a slight increase of pain; the temperature was not greater than that of other parts of the body; the pulsations of the radial artery were somewhat accelerated. There was a slight œdema of the hand; the fingers were constantly bent, and every attempt to straighten them occasioned acute pain in the tumour; the forearm was flexed to a right angle upon the arm, and the motions of supination and pronation were impossible.

Notwithstanding the general pallor of the patient, the softness and flaccidity of his muscular system, his health did not appear to be much affected; his appetite and digestion were good, and he had retained his *embon-point*; he complained only of extreme weariness, arising from the insomnia which had continued for several months, and of the inconvenience arising from the weight and enormous magnitude of the tumour, which augmented the pain, and rendered every motion irksome. The arm was amputated on the 18th of March, and towards the latter part of April the patient was discharged cured.

Upon examination the tumour was found to weigh nine pounds; the violet tint of the skin disappeared after the operation, and on the posterior surface of the tumour, this membrane was observed to be sensibly thicker and denser than in other parts; the corresponding cellular tissue was lardaceous and infiltrated with serum. The subcutaneous veins were enlarged, flattened, and their parietes thicken-

ed; between the borders of the incision, which has been mentioned as having been made some months before, there was a small, scirrhous, papilliform, and slightly projecting body; the surface of the tumour was regularly rounded, except on the inner side of the forearm, where it was unequally lobulated. The muscles arising from the internal condyle, such as the pronator teres, flexor carpi radialis, &c. were spread out upon the anterior surface of the tumour, as were likewise the extensor digitorum communis, extensor carpi ulnaris, &c. posteriorly. The tumour, separated from its investments, extended from the humero-cubital articulation to the upper margin of the pronator quadratus muscle. The fleshy fibres of the flexor digitorum profundus, and the flexor pollicis longus had entirely disappeared, and were replaced by the lardaceous and cartilaginous substance which composed the tumour; the same alteration extended in part to their tendons. Posteriorly, the anconeus, the long abductor, the long and short extensor of the thumb, the extensor primi digiti, and the supinator radii brevis, had undergone, some altogether, and others in part, the same degenerescence. The change from muscular tissue to the lardaceous state, could readily be traced, the fibres were observed colourless and infiltrated with serum, forming a mass which preserved the form of the muscle until its union with the tumour, when they could no longer be distinguished.

The substance of the tumour was hard, and presented a smooth surface when incised by the knife, displaying in great number the orifices of minute vessels; it consisted of scirrhous and encephaloid tissue, and excavations were observed in different parts, containing, some a sanguineous serum, and others a yellowish fluid; one, larger than the others, existed opposite the spot where the incision had been made, and gave rise to the fluctuation perceived there. The tumour adhered to both surfaces of the inter-osseous ligament, surrounding the artery of the same name, which had undergone no alteration; penetrating between the radius and the ulna, it had produced an incomplete luxation of the former, outwards upon the humerus. The cartilages and synovial membrane of the elbow joint were sound, the latter contained some flocculi like inspissated synovia, surrounded by a very delicate membrane, permeated by an infinite number of capillary ramifications. The density of the tumour increased in the vicinity of the ulna, which was evidently the centre round which it had been formed.

The humerus was sound; the radius was enlarged but not softened, covered with unequal vegetations, and separated from the ulna about an inch; its articulation with the latter was destroyed. The ulna, equally hypertrophied and firm, was bristled in its superior half by a multitude of osseous fibres, which formed upon its surface a very irregular, spongy, and cellular net-work, intermingled with a resisting cartilaginous tissue, which insinuated itself into the smallest interstices,

and in certain points adhered intimately to the bone. In proportion as the osseous vegetations separated from the ulna, they appeared to lose their continuity, and to form an alveolar tissue, with large plates, in the midst of the substance which composed the centre of the tumour. The most eccentric filaments of this osseous net-work, became gradually confounded with each other, and disappeared in the tissue of the tumour about two inches from its periphery. The cartilage investing the superior extremity of the ulna, was more pliant, but quite as firm as in its usual state; it had a more evidently fibrous appearance. The articulation of the wrist was sound, and all its motions free.

Dr. Ollivier refers to an analogous case described by Boyer,\* in this instance, however, the disease was evidently constitutional, and its development occupied a space of twenty years, which is strongly contrasted with the rapid march of the case just narrated. In both instances, the disease commenced in the extremity of the bones near the articulation, without implicating the latter; but in that related by Boyer, the osseous vegetations were accompanied by a dilatation of the canal of the femur, on which bone the disease was situated; in the present case nothing of the kind was observable; the ulna was enlarged, its consistence not sensibly changed, and the inequalities on its surface—the vegetations, and the areolar spongy tissue—appeared to be the result of a true hypertrophy. In both cases, the disease of the bone preceded the development of the surrounding scirrhus mass, so that it appeared to have been the exciting cause of this degenerescence. Dr. O. inquires whether this co-existence depended upon the osseous spiculæ, which diverging from the bone into the surrounding parts, induced a constantly increasing degree of irritation? or whether it is to be referred to the nature of the cause which primarily gave origin to the disease of the bone?

From the Transactions of the Medico-Chirurgical Society of Edinburgh.

#### CASE OF DISEASE OF THE HEART.

By J. H. WISHART, F. R. S. E., Surgeon to the King in Scotland.

Mrs. Edmonston, æt. 28, March 28, 1827, is affected with palpitation of the heart of several years' standing: the pulsation is very irregular and intermitting; it is not confined to the natural situation between the sixth and seventh ribs, but it may be felt nearly as high as the clavicle on the left side of the sternum. The beat is so strong, that it is often heard by her attendants; and, on approaching the ear, the sound is very loud. She has a constant, severe tickling cough, with great anxiety and

hurried breathing; the jugular veins have a distinct undulatory motion. The pulse at the wrist is so feeble and irregular, that it can scarcely be numbered. Her chest is very much emaciated, and the mammae shrunk. The lower extremities are œdematous; the swelling extends up to the pelvis, and there appears also to be slight effusion in the abdomen, but no fluctuation. Urine very scanty and high coloured; bowels open; appetite natural; considerable thirst.

This complaint of the heart is of about seven years' duration: she has during that time been unable to walk quick, or up stairs, without violent palpitation being brought on. She was always aware of it herself, and, of late, her sister, who used to sleep with her, was sensible of the sound produced by the beating of the heart. It has increased considerably within these nine months, since she was married, and has had more to do in going about in the management of her house. The dropsical swellings have supervened within these six weeks.

On visiting this patient, after clearing out the bowels, I directed her to take pills of a grain of squills, and half a grain of calomel, night and morning; to use the nitrous æther in juniper tea, acidulated with cream of tartar, as her ordinary drink. I was very desirous of trying the effect of small bleedings from the arm, but on its being hinted at to her, I found that both the patient and her friends had imbibed a prejudice against bleeding, in consequence of its having been once tried with no benefit. The case was now too far advanced to expect any thing from it except temporary relief, and, on that account, I did not urge its use.

In the course of a few days, I found, in addition to the other symptoms, that her whole body was tinged of a yellow colour. I examined the right side; but although there seemed some firmness, I could not perceive any enlargement of the liver. The medicines had had the effect of increasing the quantity of urine, but had procured no relief to the pectoral symptoms; the cough was very distressing. The digitalis was ordered, in addition to the former prescription, and small doses of an opiate were directed for the cough.

There was, from this time, a very rapid aggravation of all the symptoms. The œdema of the lower extremities increased to a great degree; the surface of the skin was shining, and at the sides of the thighs and on the fore part of the legs, there was a reddish brown mottled appearance, as if blood had been effused. The yellowness of the whole body increased to a greater pitch than I had ever seen it. For two days previous to her death, she seemed unable to cough; she lay in a continual slumber, the pulsations of the heart became sensibly feebler, and she expired on the 18th April.

*Dissection.*—The skin of the whole body was tinged of a deep yellow colour; the lower extremities œdematous, and nearly double their natural size. On opening the thorax, the

\* *Traité des Malad. Chir.*, tome iii. page 594, 1re. edition.

pericardium and heart occupied the space between the second and the seventh or eighth rib on the left side, but inclined more to the right than usual; the lungs were healthy; about twelve ounces of serum were contained in the cavities, rather more in the left than in the right. The pericardium contained six ounces of fluid of a deeper yellow colour than natural. The heart, also, was tinged of a yellow colour; it was a little enlarged in size. The right ventricle was occupied by a polypous concretion, extending into the auricle, which it filled so completely, as must have been a great impediment to the circulation from the veins into the auricle, and from the auricle into the ventricle. The polypous concretion adhered to the columnæ carneæ of the ventricle; it extended, also, for a very short space, into the pulmonary artery. On laying open the left ventricle, its walls were rather stronger than usual, but the difference was slight; its cavity was small, and contained very little blood. The mitral valves were cartilaginous, and the orifice so contracted, as scarcely to admit the point of the little finger. The opening of the aorta appeared at first sight completely closed; a caruncular excrescence, with fringed edges, projected into the ventricle. On examining it, it was found to originate on the aortal aspect of the valves, and passed through them into the heart; it filled up the space, so as scarcely to leave an opening sufficient to admit a split almond. The left auricle was much distended, and filled with a fleshy concretion mixed with blood, but of a quite different appearance from that in the right cavities of the heart, resembling the concretions that are frequently met with in aneurismal sacs. The two venæ cavæ were larger than natural.

About a pound and a half of very dark coloured serum was contained in the cavity of the abdomen. The omentum was of a dark colour: it did not extend, as usual, over the surface of the intestines, but was shrivelled up at the upper part. The right lobe of the liver was enlarged; very firm in its texture, and its surface covered with small white tubercles of the size of a pea. On cutting into it, similar tubercles were distributed through it. The gall bladder was of a whitish colour: it was quite collapsed, and did not contain more than a tea-spoonful of greenish bile. The duct leading into the duodenum was impervious, and did not admit the point of a small probe.

The pulsation of the jugular veins has been considered as a diagnostic mark of the dilations of the right cavities of the heart. In some cases, the pulsation has also been observed to be communicated to the veins of the arm to such a degree, that it was difficult to distinguish them from the arteries. In these cases, the retrocession of the blood by the superior cava, seems entirely ascribable to the quantity of blood that has penetrated into the right ventricle, being greater than can be received by the pulmonary artery, there being also some defect in the corresponding ventriculo-auricular orifice. The contraction of the ven-

tricle which projects the blood into the lungs, sends back a portion of it again into the auricle through which it has just passed, the blood returning again into the jugular veins, and meeting that which was flowing towards the heart, suddenly distends them. In this case, the inversion of the course of the blood was not owing to dilatation, but to the obstruction caused by the polypous concretion passing from the auricle into the ventricle, and the beginning of the pulmonary artery.

Diseases of the valves are among the most common affections of the heart, and occur as frequently as enlargements of its cavities. These affections are very often combined together. They occur most frequently in the left side of the heart, and, perhaps, more generally in the mitral valves; very often in the semilunar valves of the aorta, and are much rarer in the right side of the heart. Various are the forms which the valves assume in their changes. The venous, or mitral valves, frequently adhere to each other, and leave only a narrow fissure, or small hole, for the passage of the blood; or they are torn, rolled up, and compressed against the walls of the cavities; they are in various degrees of stiffness and induration, with swellings or irregularities on their surface. The arterial valves assume, also, a variety of forms; they adhere readily at their indurated edges, or an induration forms at their base, which unites with a bony ring; or they are separated into several portions, which contract and form irregular swellings; or they become rigid, and form a firm mass which prevents the flow of the blood from the heart.

The effect of these changes is a greater or smaller obstruction to the flow of the blood, and this may be either uniform and continued, or irregular and intermitting. The latter is produced by the soft excrescences, and such indurations of the valves as allow a certain degree of motion.

The symptoms characterising contraction of the openings of the heart are the same as denote every disease of that organ; such as anxiety, palpitations, faintings, &c.; but these symptoms are more strongly marked. In the state of rest, the patients often feel a slight, but always a certain, degree of contraction about the heart, marked by a superficial and unequal degree of difficulty of breathing; but as the disease increases, this becomes constant and often violent. The motion of the heart is sometimes, though rarely, regular, and corresponds with the pulse; but on the slightest motion it becomes in a moment irregular, and the pulse also varies from its previous correspondence with the pulsation of the heart. It becomes unusually quick, without necessarily becoming irregular; or when it does become irregular, its irregularity is different from that of the heart. The breathing becomes more affected, in proportion to the disturbance of the activity of the heart, and there is often a threatening of suffocation. Farther, these symptoms are accompanied with paleness and blueness of the countenance and of other

parts; frequent bleeding from the nose, startings in the sleep, and fainting fits.

In many cases we observe a whizzing of the heart, such as always takes place when the blood passes through a narrow aperture; but this appears more especially to take place when in consequence of the imperfect closure of the opening of the heart, a part of the wave of the blood is driven back through it. In contractions of the apertures of the heart, we find almost all the marks of other affections of the heart conjoined; they gradually supervene, such as œdematous swellings of the extremities: all the symptoms increase in the progress of the complaint, especially the want of correspondence of the beating of the heart with the pulse at the wrist, and the irregularity of both; as well as the sudden change from a state of comparative ease to that of urgent symptoms of suffocation.

*Edinburgh, July, 1827.*

A very short time after the preceding case was read to the Society, I stated at one of the meetings, that I had a case under my care which presented so precisely the same train of symptoms as the preceding, that I mentioned at the time that I had reason to anticipate the same morbid appearances, that is, the contraction of the semilunar and mitral valves. The symptoms particularly presenting, were the undulations of the jugular veins to a very great degree; indeed, it appeared as if there was the motion of *boiling* in the neck on both sides. The respiration was more difficult than in the former case; the patient could only breathe with her head leaning forward between her knees. There was great irregularity in the action of the heart; it generally intermitted every fourth beat, but the action was not so much increased in force. The pulse at the wrist was so feeble and irregular that it never could be counted. The patient had always been of a very sallow unhealthy complexion, but it had been ascribed to some affection of the abdominal viscera: she was frequently distressed with sickness and vomiting. The symptoms of affection of the heart had never been observed in any previous illness.

The usual medicines were tried without avail; œdema rapidly supervened, and she expired on the 17th September, about ten weeks from the commencement of her disease.

The body was opened in presence of Dr. Knox. It was œdematous to a very great degree; twenty-five ounces of serous fluid were found in each cavity of the thorax, and about ten ounces in the pericardium.

The heart was generally more muscular than usual, more especially the left auricle. The left auriculo-ventricular orifice was greatly contracted, in consequence of a thickened cartilaginous state of the mitral valve. The aortal valves were in a similar state, permanently thickened and cartilaginous; and the opening into the vessel scarcely admitted the little finger. It seemed even as if this state was about to extend to the right side of the

heart; for the tricuspid valve was beginning to partake of this thickening, and the pulmonary semilunar valves were stronger than usual.

One kidney presented the appearance of that disease by which this organ is ultimately converted into a number of sacs communicating one with another.

Both preparations have been deposited in the Museum of the Royal College of Surgeons of Edinburgh.

From the Transactions of the Medico-Chirurgical Society of Edinburgh.

**REMARKABLE CASE OF CRURAL HERNIA.** By GEORGE BALLINGALL, M. D., F. R. S. E. Surgeon Extraordinary to the King, and Regius Professor of Military Surgery in the University of Edinburgh.

On the 14th of March last, I was requested to visit Mrs. Hay, a patient of Mr. Young's, labouring under obstruction of the bowels; and who had previously been twice the subject of strangulated hernia. The first attack of this disease she had sustained six or seven and twenty years ago; and it was stated to have then terminated in ulceration at the groin, and the formation of an artificial anus, through which the feces were discharged for some time. The second attack took place, under Mr. Young's observation, about sixteen years ago, and, after considerable difficulty, he succeeded in reducing the tumour by the taxis, without operation.

On examining the groin, where she complained of pain and tenderness upon pressure, an irregular cicatrix, like that of a scrofulous ulcer, presented itself, and a small puffy tumour was observed under the crural arch, in the usual seat of femoral hernia. This tumour could be reduced with the utmost facility, and the finger could be made to follow it up under Poupart's ligament, the aperture being uncommonly free and pervious. The tumour could be reproduced by coughing; it apparently contained a fluid, and was repeatedly protruded by the patient's efforts, and again returned, with ease, by Dr. Abercrombie, Mr. Young, and myself, going up generally with a slight gurgling noise. The patient's bowels had been completely obstructed for upwards of two days; she had vomited repeatedly, and complained of pain and tenderness over the whole surface of the abdomen, although these last mentioned symptoms had been considerably alleviated, previous to my seeing her, by blood-letting, blistering, and the other measures adopted by Mr. Young and Dr. Abercrombie.

Being satisfied, from an examination of the parts, that no stricture existed in the crural arch, we were led to conjecture that a contraction of the caliber of the intestine had probably taken place, in consequence of the previous sphacelus, or ulceration of the gut, six and twenty years before; and, under these circumstances, I suggested the possibility of

saving the patient's life, by making an incision over the site of the tumour in the groin; and, should the bowel be found obstructed or adherent in the neighbourhood of the crural arch, perforating it, to give vent to the fæces, thus forming again an artificial anus. The symptoms were not, however, at this time, thought sufficiently urgent, to justify an operation, the utility or success of which depended so much upon contingencies, which could not be foreseen, and twenty-four hours more were employed in attempts to relieve the bowels by the exhibition of purgatives, particularly the croton oil, and by the administration of turpentine injections. These not proving successful, the other gentlemen in consultation with me became desirous that an attempt should be made to save the patient's life by operation. I therefore made an incision in the usual form through the integuments, and opened a hernial sac, from which a small quantity of serous fluid was discharged; towards the outer or iliac side of this sac a small portion of intestine was found lying adherent to the integuments and to the sac, but having no appearance of strangulation, nor in any degree compressed or strictured by the crural arch; this portion of the gut was slightly wounded in dissecting back the integuments, and was afterwards opened, freely, as seen in the preparation on the table; the finger was introduced through the aperture, and carried both upwards and downwards, but no feculent matter was reached, nor was any discharged during the patient's life. She gradually sunk after the operation, and survived only about eighteen hours.

On dissection, the aperture was found to be in the lower part of the ileum, which was somewhat contracted both above and below the orifice, and in the upper part of the canal a considerable quantity of liquid fæces was found, which had begun to pass through the opening in the gut during the dissection. No obstacle, indeed, appeared to exist to the free passage of the fæces through the aperture which I had made; but the upper part of the canal seemed to have become paralysed previous to the operation, to have lost the power of propulsion, or, perhaps, to have taken on an inverted action, as indicated by the stercoraceous vomiting which latterly took place.

It was my intention to have submitted to the society, at some length, the observations which have suggested themselves to me, in reflecting upon the foregoing case, but, at present, other avocations prevent me from doing so; and I shall only observe, that this, and many other cases, press strongly upon my mind the justice and importance of a remark made by Janson, the chief surgeon of the Hotel Dieu at Lyons: "Nous dirons à ce sujet, qu'on ne peut jamais préciser au juste ce qu'on rencontrera dans une tumeur herniaire." This uncertainty as to the contents of every hernial tumour, appears to me to justify and to encourage an early resort to operation, wherever any vestige or remains of hernia exist, in combination with obstinate obstruction of the bow-

els; and had the operation which was performed in Mrs. Hay's case been done at an earlier period, there is, I think, much reason to believe that her life would have been prolonged.

From the London Medical Gazette.

#### FACTS AND OBSERVATIONS ON THE PATHOLOGY OF SOME DISEASES RESULTING FROM MORBID POISONS. Communicated by JOHN ASHBURNER, formerly Physician to the Small-Pox Hospital, &c.

Twelve years ago I promulgated some curious facts relating to the pathology of hydrophobia, which, though very important, I was from various circumstances not then enabled to give to the world in a sufficiently detailed form. The authors of the article on this extraordinary malady, in the *Dictionnaire des Sciences Medicales*, regretted the shortness of my notice, and some doubts were consequently thrown upon the nature of the disease upon which the observations were stated to have been made. The kindness of my friend Mr. King, of Clifton, to whom I am indebted for extracts from his journal, has enabled me to remove these, and I shall hope that the recital of the circumstances connected with his experiments and observations will lead to further investigation.

A question of no inconsiderable importance in the pathology of diseases resulting from morbid poisons, suggested itself to the mind of Dr. Darwin, in the consideration of hydrophobia, as to the period after inoculation when excision of the inoculated part might be performed with a prospect of arresting the progress of the disease. He says, (*Zoonomia*, vol. iv. page 50,) "If the patient were bitten in a part which could be totally cut away, as a finger, even after the hydrophobia appears, it is probable it might cure it; as I suspect the cause still remains in the wounded tendon, and not in diffused infection tainting the blood. Hence there are generally uneasy sensations—as cold or numbness in the old cicatrix—before the hydrophobia commences."

Dr. Babington held the opinion that there is a specific period, (*Medical Records and Researches* of, &c. page 127,) prior to which the disorder may at any time be prevented by the removal of that part whence the matter was at first introduced.

It would be very desirable to ascertain the precise period of time at which the inoculated part in small-pox, or in the milder variety of the same disease, cow-pock, may be removed to prevent the accession of the constitutional affection; and to determine whether its removal after the disease had supervened would modify the subsequent train of symptoms. The tissues most particularly influenced by the application of certain morbid poisons are not yet sufficiently distinguished; and the changes of organization which these tissues undergo, form a subject of inquiry as interesting as it is important.

Inoculators of the vaccine virus are in the habit of observing the irregularities which take place in the progress of the pock, if at any stage its structure be materially injured. The object of vaccination may have been more often frustrated by accidental injury than would perhaps be generally allowed. Having watched with some attention the minute structure of the pock, in its various stages, I shall take this opportunity of illustrating the kind of investigation which is necessary in inquiries into the local changes of organization, induced by the inoculation of morbid poisons.

My friend, Dr. Macartney, whose well-known zeal for the improvement of science is beyond all praise, has made an admirable attempt to classify diseases of the skin according to a natural arrangement. In the diseases which he places under the term *varicodes*, the first specific change of structure is in the cellular tissue which surrounds the villi of the true skin; this texture interposes itself between the cutis and cuticle, and obtains the name of rete mucosum. If a pock be observed in its earliest stage, there is perceived a few blood-vessels determining to a central point, and a pimple results, which has a hard feeling under the finger. Mr. Cross observes, "that in twenty-four hours this pimple increases so as to prove an acuminated vesicle." (See Cross's Hist. of the Varioloid Epidemic, &c. page 135.) The acumination disappears, and about the fourth day the centre of the eruption is depressed, for a cellular arrangement obtains, which having been pointed out by Dr. Macartney to Mr. Cross, was by him likened to "the axis, spokes, and circumference of a wheel," (page 136.) These cells are formed by the attachment of the central portion of membrane to an inflamed part of the villous surface of the true skin, and they are filled by a fluid, which is at first limpid, but which in time becomes less transparent, distending the pock, and giving it somewhat the appearance of a portion of a white currant. On the sixth day the central indentation disappears, the fluid of the cells becomes inspissated and glutinous; the villi of the cutis, which had been embraced by the delicate cellular membrane, inflame, and about the eighth day pour out pus. The wheel-like organization is now gone, for the cells have been consolidated by the agglutination of their contained fluid, and they form the upper covering—the membranous dome of a small abscess, the floor of which is a highly inflamed tuft of cutis.

There is a tendency in severe cases of small-pox for this villous tuft to die; it may in time form the slough described as distinctive of the small-pox pustule. If it should not die, a recovery of the part takes place, and no cicatrix is left: when it dies, a slough is thrown off, the consequence of which is a puckering of the skin; the loss of substance always leaving a very irregular-shaped scar, having a tail-like extremity, a distinction by which the small-pox cicatrix is known.

In the small-pox which occurs after vaccination, it is not uncommon, about the fifth day,

to find the eruption converted into small horny buttons; for the villi have not been highly inflamed. They soon recover themselves, but the cellular structure, having been amply filled with a glutinous lymph, is left in a hard and darkened state; it is tough, and remains for days without much apparent change, for the diseased action having been mild, and not attended by energetic inflammation, the absorption of these hard buttons is very slow.

In the scabbing stage of the cow-pock the regularly agglutinating process is easily observed. The cells become gradually consolidated; for the fluid thickens, becomes hard, and at last a scab is formed, beneath which ulceration has removed a small portion of the villi of the cutis, and has left the slight mark peculiar to a mild and uninjured progress of the pock.

In the diseases of this nature, in which the constitutional irritation precedes the specific organization, there would seem to be no period at which its approach could be arrested. Considerable danger occasionally attends an attack of variolous fever, even in persons who have previously been perfectly vaccinated. I have known several cases in which death has taken place on the third day of the febrile action, the patient having been at that period covered with petechiæ, instead of the pimples which should have appeared.

In the inoculated cow-pock, the constitutional irritation cannot precede the specific change of structure; and in some cases it is very difficult to say when the constitutional influence has been established. Experiments have not yet decided whether the extirpation of the newly-organized structure would check the advance of the disease.

We know nothing clearly of the change of organization which takes place at various periods in the part inoculated with the hydrophobic poison. The following detail, put together from information afforded by Mr. King's journal, will show that the investigation of the point has not been wholly unattended to.

On the 12th of March, 1807, Dr. Beddoes and Dr. Stock accompanied Mr. King to the village of Weston, near Bath, for the purpose of seeing a rabid cow, that was shut up in a barn at Farmer Wintle's. They observed the animal from an open window above its reach, and opposite to them was a strong barricade, about seven feet high, which divided that part from the rest of the barn. The space of the floor thus partitioned, in which the cow was placed, was about 20 feet by 14. She was excessively furious, and bellowed very loud; frequently attempted to leap over the bar, and several times nearly succeeded. Sometimes she ran with violence against the wall, and fell down in convulsions. A wooden block was let down from the window, at which she ran with great fury, and tossed it about. A bucket of water was similarly let down, but before it came to the ground she plunged at it and overturned it. Upon the water being splashed about, she fell into violent convulsions. Her eyes were much blood-shot.

Mr. King observed her for about two hours,

and at one time she was seen attempting to eat a little hay; but it was soon vomited again, and she then voided a small quantity of black hard dung, to which she turned round, and in a short time ate it up.

The author of the article Dog, in Rees's Cyclopædia, mentions that some dogs affected with rabies will eat their own excrement, and lap their own urine. Rabid dogs are said not to evince any dread of water. It must be remarked that this cow exhibited the hydrophobic symptom; and within three or four months of the period at which she had this disease, in all that part of Gloucestershire lying northward between Bath and Bristol, the villages of Pucklechurch, Syston, &c. a very unusual number of such cases had occurred among cows, horses, and other animals.

Farmer Wintle informed Mr. King, that about two months before several rabid dogs had passed through the field in which his cow was placed, but they had not been observed to bite her. She had calved about a month before this time. It was on the 8th of March that she had been perceived to be different from her usual way. In the evening she was very unruly, and frightened the woman who went to milk her. On the following morning she quietly submitted to be milked: in the evening of that day she foamed at the mouth, ran about in a very wild manner, and rushed at people with her mouth open, as if to bite. She was then confined in the barn.

Mr. King was desirous of trying an experiment with the saliva of this cow, and, accordingly, in one of the attempts she made to leap over the barricade, her head projecting over it, he succeeded in obtaining on his hands an immense volume of foam which was discharged from her mouth.\* He had made two incisions under the wings of a common barn-door hen, quite through the integument, the lancet scratching the muscular part. The foam immediately taken from the cow's mouth was inserted, by rubbing, into these cuts. The fowl was

confined in a basket, her head projecting through an aperture; she was secured in such a manner as to prevent her from pecking at the incisions; and thus the bird was sent to Dowry Square, in Dr. Beddoes' carriage. The next day this fowl was let loose, among others, in a poultry-yard at Mr. King's residence. On examining it from time to time, the incisions were found to be soon healed, and their place could with difficulty be discovered. The habits of this fowl appeared to be exactly like those of the rest. On the 25th May, however, she was observed to run at the other fowls, and she refused her food. She had a wild, strange expression, and her eyes were blood-shot. Early in the following day, her legs became contracted, so that she very soon lost the power of standing upright. She remained sitting, with the legs rigid, a long time, refusing food and water, and appearing very irritable when touched. She died in the evening, immediately after readily drinking a large quantity of water, which had been offered to her.

Early on the morning of the 27th of May, this fowl was examined by Dr. Macartney. The inoculated parts appeared recently inflamed. There was great vascularity about the cicatrices, which exhibited three small tumours, each about the size and shape of a compressed pea, the vessels of which were turgid with blood. At the time of the inoculation, Mr. King observed no injected appearance about the parts, nor was such apparent when the bird had been examined at any subsequent period. The trachea and œsophagus were considerably inflamed, and the vessels of the brain were distended with blood.

A question which naturally occurs, upon the consideration of these tumours, is, can there be a specific character in the change of organization which appears at the cicatrix of inoculation with rabid poison?

In the year 1822, the sister of one of my servants having been attacked with hydrophobia, was sent by me to the Middlesex Hospital. She had been bitten by a little dog in the hand, about six weeks previously. After exhibiting the usual train of distressing symptoms, unrelieved by the exhibition of very large doses of opium, she died. I could not conveniently attend the examination of the body, but I requested my late friend Mr. Shaw, to pay particular attention to the state of the cicatrix in the hand; and his observation to me afterwards was, that there appeared to be much vascularity about the spot: not having, however, turned his attention to the post mortem state of scars left by lacerated wounds, he was unable to say whether the degree of vascularity was unusual, or whether there was present in the part a structure which could be called specific. Additional and more minute observations are, therefore, wanting, to establish whether hydrophobia be or be not accompanied by a specific organization of the inoculated part.

London, May, 1828.

\* This cow died on the night of the 12th March, and on the 14th was brought to Mr. King's house, in Dowry Square, where he dissected it on the 15th. The brain and spinal marrow were very soft: they were not at all inflamed: the tongue, as far as the root, was natural. The whole surface of the fauces, pharynx, larynx, and mucous membrane of the trachea, was of a dark colour, and in appearance resembled brownish crimson velvet. The pleura costalis, to within two ribs of the diaphragm, was of the same tint: in parts it was putrid. The pericardium was full of a dark-coloured fluid. The cavities of the heart were distended with dark grumous blood. The diaphragm and stomach were healthy. The liver was small, pale, and shrivelled. No bile was perceivable in the small intestines. The spleen was very dark, flabby, and full of grumous blood. The kidneys were in a putrid state. The rest of the abdominal viscera appeared healthy.

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# MEDICAL STATISTICS.

*Abstract of Lectures delivered at the College of Physicians, by Dr. Bisset Hawkins.*

Statistics, a science of modern origin, seems to have been first applied to observations on the public health, and to have derived its birth from bills of mortality. It has become the key to several branches of knowledge, opening, in a manner the most convincing, simple, and summary, their gradual progress, their actual condition, their relations to each other, the success which they have attained, or the deficiencies which remain to be supplied. Its application to the objects of government has created political economy; and there is reason to believe, that a careful cultivation of it, in reference to the natural history of man, in health and disease, would materially assist the completion of a philosophy of medicine, by pointing out to physicians of every part of the world, the comparative merits of various modes of practice, the history of disease in different ages and countries, the increase and decrease of particular maladies, the tendency of certain situations, professions, or modes of life, to protect or to expose; and, finally, by indicating, as the basis of prognosis, the extended tabular views of the duration and termination of illness, which are furnished at successive periods by the registers of hospitals.

It is more than probable, that if the doctrines of Brown had been universally subjected to the test of statistics, they would never have obtained so baneful an influence over the practice of continental Europe. Peter and Joseph Frank, two eminent German physicians of that period, eagerly adopted at first the ideas of Brown; but candidly recanted their error, after witnessing the results on a large scale, at the clinical institutions of which they were professors.

The same test may at present be advantageously applied to the opinions of M. Broussais, who is now the chief oracle of medical theory in France. It is well known that he imagines every where a gastro-enteritis, and that his principal remedy is the application of leeches. In the prospectus of his Journal he asserts that his practice has a prodigious success; and that in the hospitals, where it has been adopted, the mortality has fallen from one in five to one in thirty. But examination of the registers of the Val de Grace (the military hospital to which he is attached) has ascertained that, on an average of five years, he has been uniformly less fortunate than his brethren at the same establishment. The mortality has been 1 in 18 under his three colleagues, while that of his own patients has been 1 in 13; and so far is the mortality of his own cases from incurring a gradual diminution, that, from 1816 to 1819, it had rather increased, being, in 1817, 1 in 14; in 1818, 1 in 12; and in 1819, so much as 1

in 8, a very large sum for a military hospital.

Medical statistics afford the easiest proof of the efficacy of medicine in opposition to the vulgar notion (sometimes carelessly countenanced by medical men) that nature is generally alone sufficient for the cure of disease, and that art as frequently impedes as accelerates her course. If we form a statistical comparison of fever treated by art, with the results of fever consigned to the care of nature, we shall derive an indisputable argument in favour of our profession. Of 37 cases of fever treated by Hippocrates, (in only a few of which it seems that clysters and suppositories were alone employed,) 21 ended in death—above half of the whole. But at the Fever Hospital of London, in 1825, the total mortality was less than one in seven, although half the deaths occurred within 72 hours after admission. At the Dublin Fever Hospital, the average loss from 1804 to 1812, was only one in twelve; and in the Clinical Wards at Edinburgh, in 1818, the mortality of fever was also about one in twelve. This termination throws no shade over the skill of Hippocrates, but rather brings to light his love of truth: the mortality belonged to the age, and not to the physician; and it may be reasonably inferred, that under other practitioners of his time it was even more severe. We perceive that one out of two cases of fever may recover by the almost unaided efforts of nature; but that, under the medical protection of our own age and country, 6 out of 7, or even 11 out of 12, are likely to survive.

Medical statistics enable us to form the most correct estimate of the influence of certain mechanical improvements in promoting the salubrity of particular districts.

No documents remain to inform us of the rate of mortality, or of longevity, amongst the Greeks. A few facts on these points have descended respecting the Romans. The *expectation* of life calculated for the citizens, and not including the slaves, appears to have been in the third century thirty years. If we select subjects in England of a similar condition, an extension of life discloses itself remarkably in our favour, as the expectation of life for our middle classes is at least fifty years; and for the whole mass of Britain at least forty-five years. The mean duration of life among the easy classes of Paris is forty-two years. The probability of life to the *whole* population of Florence is the same in the present century as that of the *easy* classes of Rome in the third century.

The observances of the Christian religion appear to have revived in modern times the registry of births and burials. At Geneva good mortuary tables have been preserved since 1560, and the results are in the highest degree curious and satisfactory. It seems that, at the time of the reformation, half the children born did not reach six years of age; at present they attain to twenty-eight, so that in the course of about 300 years, the probable life of a native of Geneva is become nearly

five times greater than before. The mean life was thus in one century eighteen years; in the next, twenty-three; in the following one, thirty-two; and during the ten years from 1815 to 1826, the mean life amounts to thirty-six years.\*

Captain John Graunt, of London, has the honour of being the first writer who ever directed the attention of the world to the comparative births and deaths of different cities, years, seasons, sexes, diseases,—of the town and of the country,—and to the proportion of births to deaths. In his “*Natural and Political Observations upon Bills of Mortality*,” printed in 1661, he displays a singular genius for observation in a field where no footstep can be traced before his own. The most industrious labourer who followed him, in the same mine, was Süssmilch, who published at Berlin, in 1742, his “*Göttliche Ordnung*.” The gradual accumulation of registers in the principal states of Europe had prepared for him a copious stock of materials; but the fruit was not as yet sufficiently ripe to afford a valuable harvest. His object seems to have been rather to draw certain general conclusions which apply to the whole civilized mass of the globe, than to balance the comparative degrees in which various countries and cities enjoy, or are deficient in, health and longevity.

Süssmilch estimates the nearest average of mortality of all countries, (taking towns and villages together) as 1 in 36. Busching, a celebrated geographer, calculates it to be from 1 in 32 to 1 in 37. About eighty years have since elapsed, and a surprising improvement in the physical condition of man has progressively developed itself. In almost every civilized country of Europe, we find the annual proportion of deaths considerably diminished, and continuing actually to diminish, relatively to the particular circumstances in which each country is placed; and, in Britain, the value of life is nearly doubled, if we compare Busching’s rate of 1 in 32 with the rate afforded by the census (taken in 1821) of about 1 in 60.

Dr. Odier, of Geneva, in the 4th volume of the *Bibliothèque Britannique*; and Dr. Herberden, in his valuable “*Observations on the Increase and Decrease of different Diseases*,” published in 1801,—appear to be the earliest authors who had the merit of revealing this improvement of life in their respective countries.

Sir Gilbert Blane, Mr. Rickman, Mr. Milne, and Mr. Finlaison, in England; Dr. Villermé, in France; and Dr. Casper, in Germany, have subsequently pursued with zeal the same path of inquiry, and have obtained conclusions the most interesting to human nature, because almost uniformly agreeing in its tendency to amelioration.

The mean duration of life to the middle

classes of Britain appears to have been thirty-seven years at the close of the 17th century, and to have risen to fifty-two years at the expiration of the 18th century. A corresponding change in the health and duration of life of our entire population has equally arrived.

In 1780 the annual mortality of England and Wales was 1 in 40.

In 1790 1 in 45.

In 1801 1 in 47.

In 1811 1 in 50, or 1 in 52.

In 1821 1 in 58, or 1 in 60.

So that, on the whole, it has decreased from 1 in 40 to 1 in 58, in forty years.

The annual mortality of the several counties of England, ranges between 1 in 47 and 1 in 72;—Middlesex and Sussex are the two extremes. In Wales, Pembrokeshire and Anglesey have only one death yearly among 83 individuals—the lowest genuine rate of mortality that has been published in any part of Europe. Even in Middlesex, where the rate is higher than in any other county, let us remark the change which has supervened in only ten years;—in 1811 it was one in 36, but in 1821 it was only 1 in 47.

But the decline of mortality is even more remarkable in our cities than in the rural districts. It is well known, that, in any given country, the deaths of a city are more numerous than those of the rural districts. This difference is chiefly felt in the first five years of life, when many more die in London than in the country. From 5 years of age to 20, the deaths are fewer in London; from 20 to 50, more numerous, on account of the large annual influx from the country. In all cities, a portion of disease and death is to be assigned to the constant importation from the country of individuals who have attained to maturity—but, having been previously habituated to frequent exercise in a pure atmosphere, and to simple regular diet, are gradually sacrificed to confined air, to sedentary occupations, or to a capricious and over stimulating food. These causes are not equally fatal to those who have passed their early years within the walls of a city; and, after the age of 50, the proportion of deaths in London is smaller than in the country. Jenner, and, very recently, Dr. Baron, have made some experiments on animals, which indicate that a loss of open range, and accustomed nourishment, has with them also a tendency to disorganize and to destroy.

Not only the comparative mortality of London is greatly diminished during the last fifty years, but its absolute mortality in respect to preceding centuries. In 1697, the deaths were about 21,000; but in 1797, only 17,000. In 1826, the deaths were less numerous, by 3000, than in 1766; although, during all this time, the population has so rapidly multiplied. The annual mortality in 1700, was about 1 in 25. About 1720, it seems to have increased to 1 in 20; to have maintained that proportion to 1750, and, from that period to the present, to have exhibited a constant and gradual decline. In 1801, the decrease was to

\* Vide *Journal of Foreign Medicine*, vol. ii. p. 95.

1 in 35, (or, as corrected by Price, 1 in 30.) In 1811, we find 1 in 38; and finally, in 1821, so low an average as 1 in 40.

From the returns of ninety-nine parishes of Scotland, which alone were given in the Population Abstracts of 1801, it appears that the average mortality was 1 in 56. I have not been able to ascertain how nearly Scotland has since kept pace with England and Wales. During the ten years, from 1801 to 1810, the average annual mortality of Glasgow was 1 in 43. During the next ten years, from 1811 to 1820, it declined to 1 in 45.

It would be interesting to derive some information from Ireland on this point; and we must regret that no correct parochial registers have been kept to elucidate the condition of that country.

On the continent of Europe, changes in the duration of life have been experienced, similar in nature, and following the same laws, as those of our own country, but very inferior in degree. In France, for instance, the annual deaths were, in 1781, 1 in 29; in 1802, 1 in 30; in 1823, 1 in 40. In Paris, about the middle of last century, the mortality was 1 in 25; but, at present, it has decreased to 1 in 32.

In Sweden, from 1755 to 1775, the average was 1 in 35; from 1775 to 1795, 1 in 37; in 1823, 1 in 48.

The mortality of Great Britain, its cities and its hospitals, appears greatly inferior to that of any other country in Europe; and it seems incontestible that Great Britain is the most healthy country with which we are acquainted, and that it has been gradually tending towards that point during the last fifty years. It has been long the fashion, both abroad and at home, to exhaust every variety of reproach on the climate of our country, and particularly on the atmosphere of London; and yet we shall find that the most favoured spots in Europe, the places which have been long selected as the resort of invalids and the fountains of health, are far more fatal to life than even our great metropolis. The proportion of deaths at Montpellier was greater thirty years ago, and is greater at present, than in London; and, although it is usually much larger in cities than in provinces, yet it is exactly the same in London, and for the department of the Herault, the southern, fertile, and long supposed most salubrious district of France, of which Montpellier is the capital. The annual mortality of Nice is about 1 in 31; of Naples, 1 in 28; of Leghorn, 1 in 35.

At Berlin, it is 1 in 34; Paris, Lyons, Strasbourg, Barcelona, 1 in 32; Madrid, 1 in 29; Vienna, 1 in 26; Rome, 1 in 25; Amsterdam, 1 in 24.

If we compare country with country, our superiority is equally striking. The country which approaches most nearly to us is the Pays de Vaud, where the annual deaths are 1 in 49; in Sweden and Holland, 1 in 48; in Russia, about 1 in 41; in France, 1 in 40; in Prussia and Naples, 1 in 33 to 1 in 35; in Wirtemberg, 1 in 33.

The principal end of hospitals is the relief of the sick poor, but another benefit may be derived from them,—an abstract of the results of their multiplied experience, without which their instruction to our profession and their utility to the public, are considerably abridged.

Mr. Milne remarks, that in reading the writings of the physicians who have treated these subjects, it is impossible not to regret that they have been so little attended to by the profession in general; and that bills of mortality have not been more generally kept, in such a way as to throw the light, which they alone can do, on the causes of the increase and decrease of different diseases; and of the great differences that are found between the degrees of mortality in different situations, and among different classes of the people.

Some persons appear to have hastily concluded, that the mortality of an hospital affords little information as to the economy or practice prevailing in it; and have even ventured on the paradox of supposing that the deaths will become more numerous as the discipline improves, and as the skill of the officers increases; because, under such circumstances, the most severe cases alone will be selected, and will be speedily discharged, to afford room for new ones. This argument appears to be founded chiefly on a solitary fact, originally proposed by Joseph Frank, namely, that in the ninth year of the French Republic, the mortality at the Hôtel Dieu was 1 in 7, and yet in the next year rose to 1 in 6, although the interior economy was much ameliorated; and in the following year even to 1 in 4. But the real solution of this change appears to be due to the exclusion from the Hôtel Dieu, at that time, of all pregnant and insane cases, such as had been previously received there in abundance, whose mortality is much less than that of the common objects of a general hospital, and whose presence, accordingly, tended to diminish the annual amount. In respect to hospitals destined for particular complaints, as the Lock, or the St. Louis of Paris; or in regard to lying-in, or military hospitals, it would be unjust to form comparisons, except with others of a similar kind: but the general hospitals of the principal cities of Europe may be fairly approximated, with an occasional allowance for position, or for some accidental peculiarity, such as the larger number of violent injuries which occur in great commercial cities: it is probably on this last account that the mortality of St. George's hospital is greater than that of the Edinburgh Infirmary; in 1825, of 1025 in-patients admitted at the former, 664 were cases of accidents. On the whole we shall find, that in every city the mortality of the hospitals has usually declined in proportion to the increase of prosperity, and to the diffusion of knowledge; and that, wherever it maintains a high standard, the lower orders will generally correspond in their condition of want and degradation, and the profession of medicine will be seen to occupy a subordinate rank in public esteem.

It has been calculated that the 20th part of every population is labouring under illness, and that the 100th part suffers some severe disease. But the average of sickness and of recovery is constantly fluctuating under the influence of single seasons; of plenty and of scarcity, of the spirit of the times, and of political events.

The earliest notice of the mortality of our own hospitals, is contained in Sir Wm. Petty's work on Political Arithmetic; from which it appears, that in the year 1685, the proportion of deaths to cures at St. Bartholomew's and St. Thomas's hospitals was about 1 in 7. In the printed report of St. Thomas's hospital for 1689, the mortality is about 1 in 10. The first annual report of St. George's hospital for 1734, when patients were first received, yields a proportion of about 1 death in 8 patients.

The mortality of St. Thomas's hospital was in 1741 about 1 in 10; from 1773 to 1783 1 in 14; from 1783 to 1793 1 in 15; from 1803 to 1813 1 in 16.

The annual average of deaths at Christ's hospital, during the 40 years ending in 1799, was 1 in 150.

The mortality of the Edinburgh Infirmary appears to have ranged during the present century, between 1 in 14 and 1 in 20, or even 1 in 21.

The mortality of Heriot's hospital, at Edinburgh, has been only 1 in 235 annually, during the last 17 years. It is composed of children from the age of 7 to 14. The average mortality of some other public establishments at Edinburgh appears to be very low.

The average mortality of the Fever hospital, at Dublin, has been considerably diminished since the commencement of the present century.

The annual average from 1804 to 1812 was about 1 in 12; from 1812 to 1814 1 in 15; in 1815 1 in 20.

The mortality in France is much greater among the lower classes than the affluent ones. It even appears, that in the wealthy departments of France life is protracted 12½ years beyond its course in those which are poor. Precisely the same result is observed in the rich and poor quarters of Paris: where 50 deaths occur in the rich arrondissements, about 100 happen in the poorer ones.

The average mortality of all the hospitals of Paris, in 1822, was 1 in 8. 42; of all the hospitals, 1 in 6. 71.

The average stay of each patient in the hospitals was about 35 days.

The total mortality at the Hôtel Dieu, was, in 1822, 1 in  $6\frac{82}{100}$ ; and the average stay of each patient was about 25 days. If we examine the progress of several years at the Hotel Dieu, a slow, but gradual improvement will appear. Between 1770 and 1780 the mortality was about 1 in 4: at this period it would be difficult to imagine, as some have done, that high mortality is a proof of well-regulated hospitals, since Hunczowski declares that he often saw, on the same bed, a

dead body lying by the side of two dying patients, and one convalescent. From 1804 to 1814 the deaths were rather more than 1 in 5.

In the Charité hospital, at Paris, the total mortality was, in 1822, 1 in  $5\frac{53}{100}$ . The average stay of the patients was about 30 days.

The number of children abandoned by their parents is at Paris enormous: in 1826 these wretched beings amounted to above 8000. In 1818, 120 died out of 133 thus exposed. Of 1000 foundlings, at Paris, 251 have been ascertained to die during the first few days, and 235 more on their road to the country nurses, or before the end of the first year.

In the provincial hospitals of France the mortality is less than in those of the metropolis. At Lyons, for instance, the mortality of the Hôtel Dieu is about 1 in 11; and, at Montpelier, the average of all the medical institutions is about 1 in 10. We may remark, that, as the mortality of great cities is usually superior to that of towns, so the annual deaths of metropolitan hospitals will usually exceed the proportion of provincial ones;—and, generally, that in any large hospital the proportion of deaths will exceed that of a small one.

The mortality of Berlin has progressively diminished. From 1747 to 1755 the annual rate was about 1 in 28; from 1796 to 1799 1 in  $29\frac{1}{11}$ ; from 1802 to 1806 (suffering from war) about 1 in  $27\frac{1}{4}$ ; from 1816 to 1822 1 in  $34\frac{3}{11}$ .

The mortality of the Charité, the chief general hospital, on an average of 20 years, from 1796 to 1817, has been about 1 in 6. And yet, in this high mortality, must be included a large supply of lying-in women, and of the insane, whose presence should lower the total sum of death.

Dr. Casper, of Berlin, published in 1824 an interesting collection of statistical facts, illustrative of the influence of vaccination on that city. In 1789 one death out of every nine deaths in Berlin was occasioned by small-pox; but from 1820 to 1822, only 1 death in 1635 deaths was the result of small-pox. He combats, by authentic documents, the notion that the other diseases of infancy have become more fatal since the introduction of vaccination.

The annual mortality of Vienna was estimated in the middle of last century at about 1 in 20. In 1810 Wertheim brings it to about 1 in 24. Since that time, it has again undergone a slight improvement, and is about 1 in 26. The great hospital at Vienna includes a variety of clinical and other establishments; the annual mortality of the whole is about 1 in 6. In 1810 above half the foundlings received, annually, perished. Since that time their mortality has been considerably lessened by the sending them into the country to be nursed.

At Pesth, the present capital of Hungary, the annual deaths at the civil hospital were, in 1825, of exactly similar proportion to those at the hospital of the metropolis of Austria,

under whose mistaken policy it equally languishes.

The mortality of the small cities of Germany appears to be often half, or even one-third of that which prevails in the great ones.

The change in the duration of life which has taken place in the kingdom of the Netherlands has rendered the old tables of the probabilities of life, formed by Kerseboom, insufficient for actual use, and M. de Quetelet has recently constructed a new scale. His inquiries have brought to light many curious facts. The value of life in Holland seems to have doubled itself since the middle of last century. Sussmilch then estimated the annual mortality of 39 villages of Holland at about 1 in 23; and at present we find the average mortality of the whole kingdom to be 1 in 48. The mortality of the hospital St. Pierre, at Brussels, was, in 1823, about 1 in 9 among the adult patients, and about 1 in 6 among the children.

The population of Amsterdam has decreased, in consequence of declining commerce; and its mortality increases with the progress of decay. The mortality in 1777 was about 1 in 27; it was recently about 1 in 24. The average mortality of the chief hospital, (St. Pieter's Gasthuis) during the 20 years from 1798 to 1817, was about 1 in 8.

The returns formerly afforded by Russia presented such extraordinary results that a general belief prevails of their surcharges and colouring; as when, in the progress of the Empress Catherine, artificial edifices were created in the distance, to amuse with an image of prosperity. The annual average of deaths at the imperial hospital for the sick poor, at Petersburg, has been for the 20 years ending in 1817, so high as 1 in 4½.

The mortality of the two chief hospitals of Madrid was, in 1818, about 1 in 11. The report, however, is not very authentic, nor minute. At Barcelona, during 1823, the general hospital of Santa Cruz afforded about 1 death among 7 patients.

The annual mortality of Geneva is greater than that of Manchester, and is about the same as that of Glasgow and Birmingham. The mortality of its hospital in 1823, was about 1 in 11; but about a third part of the patients were soldiers, whose presence generally lowers the sum of fatality.

The proportion of deaths to recoveries at the general hospital of Genoa was, in 1821, 1 in 6. At the hospital St. Giovanni, at Turin, it was in the same year 1 in 7.

At Milan, the mortality of the great hospital, during the three years ending in 1814, was about 1 in 6. In 1823, it was about 1 in 7.

At Pavia, where the most minute records are preserved, the total mortality of the hospital San Matteo della Pietà was, in 1823, about 1 in 10. The mortality of Hildenbrand's Medical Clinic was less than 1 in 6.

The mortality of the Medical Clinic of Padua, at which Brera is Professor, was in 1821, about 1 in 11. At that of Bologna, where Tommasini is Professor, it was, about the same period, 1 in 10. We perceive the care

and skill of these eminent men evinced by the degree in which they have counteracted the ordinary course of mortality in the Italian hospitals. That of the Santa Maria Nuova, at Florence, is 1 in 7; of the hospitals at Leghorn 1 in 7½; of those at Rome about 1 in 7. At the Foundling hospital of Naples the annual loss is 1 in 5; about the same occurs at Stockholm.

Some details might be adduced, which prove that the deaths of the patients at a London Dispensary are proportionably less numerous than at similar institutions at Berlin and Geneva.

The most prominent fact afforded by medical statistics, next to the diminished mortality of infancy, is the beneficial change which has supervened within the last 100 years in the fate of lying-in women.

In 1750, at the British Lying-in Hospital of London, 1 woman died out of 42 admitted. In 1780, only 1 died out of 60. And, finally, the improvement became so great in the ten years between 1789 and 1798, that only 1 case was fatal out of 288.

Tenon assures us, that the mortality of lying-in women at the Hôtel Dieu (where they were formerly received) was so high as 1 in 15, at the very time when in the British Lying-in Hospital it was only 1 in 60; and the still-born were 1 in 13 at the former, when only 1 in 25 at the latter.

The mortality of the Lying-in Hospital at Paris was, in 1822, 1 in 30; but at the City of London Lying-in Hospital, the deaths, in 1826, were only 1 in 70; and at the Dublin Lying-in Hospital, the average deaths of 57 years have been only 1 in 93.

The deaths at the Lying-in Hospital of Stockholm were, in 1822, nearly the same as at Paris—1 in 29.

At Berlin an improvement has taken place in this respect. From 1796 to 1806, 1 woman died out of 32 admitted into the Charité; but in the ensuing ten years, only 1 in 45.

The varieties in the proportion of the still-born in different places are very difficult of explanation. In Prussia, 1 child in 32 is still-born; in Hanover, about 1 in 30. In Sweden and Finland, about 1 in 40. On the contrary, at Strasburg, on an average of 20 years, the proportion has been 1 in 11, and is at present 1 in 12½.

The still-born are generally more frequent in towns than in the country, and more common among the poorer classes than the affluent. At Stutgard it has been remarked, that the still-born increase nearly in the same degree in which the illegitimate births are augmented.

We have some curious details on the mortality of prisons, from France; but few from other countries. The highest mortality any where known among adults seems to arise at the Dépôt of Mendicity of St. Denis, where 1 individual dies annually out of 3 admitted. In all the other prisons of Paris, the annual deaths are about 1 in 23. On the contrary, those of the galley-slaves, who live much in the open air, are only 1 in 49.

So great was the care taken of prisoners of war in England, that in 1813, only 1 died out of 55, although labouring under most of the privations which embitter and enfeeble existence.

The superior health enjoyed by the British army and navy, when at a distance even from home, has often been a subject of surprise and exultation. Let us go back above half a century, and hear the opinion of a distinguished foreign historian. Alluding to the events of the seven years' war, Muller observes, that the resources of military talents were never more successfully applied than by the Britons during that contest: so much care was taken to provide for all the wants of the soldiery, that the ordinary mortality among the wounded was not more than 1 in 20; and out of 14,000 men who were employed in the year 1760 in cruising in the Bay of Biscay, scarcely 20 were attacked by disease.

If we follow the steps of the late war, we shall discover many results equally remarkable. Assuredly no ancient nor modern general has ever been so deeply indebted to his medical companions as the chief of our Peninsular troops.

Even on the barren rock of Gibraltar, the mortality of our garrison was only 1 annually in 48, according to a recent report, and exclusive of the years in which epidemic fever prevailed.

Very different was the fate of the disabled soldier in remoter times. It appears that each Roman legion, containing from 3 to 4000 men, had only one medical officer attached to it.

To mark the improvement of health in our navy, we may compare the fate of Commodore Anson's crew with a ship placed in similar circumstances about fifty years subsequently. Anson passed 143 days at sea, without touching at any place of refreshment. On his arrival at Juan Fernandez, half of his companions alone survived, and of the remaining 200, only 8 were efficient. But in 1794, the *Suffolk*, a 74 gun ship, during 162 days had no communication with land, and arrived in India without the loss of even one man, and with no case of severe disease at the time of disembarkation. The success which attended the efforts of Cook, and, subsequently, of Captain Parry, in checking the inroads of disease upon their crews, is universally known. The total mortality of our navy, in all parts of the world, including those who were lying in hospitals, was, in 1813, only 1 in 42.

A portion of the good health enjoyed by our army and navy must be ascribed to moral causes, such as national spirit, and general success. The operation of moral causes on the health of soldiers was strongly evinced in the French army during their disastrous campaigns of 1813 and 1814: the number of its diseases preserved a terrible proportion to its losses, and increased with every failure.

Suicide is so frequent a topic of allusion in medical writings, and so often depends on a deranged state of body or mind, that it seems

to merit a place in medical statistics. We have here a very pleasing conclusion to draw in respect to our own country, as, in spite of ancient prejudices entertained against our supposed propensity, it really appears that the English are *less* disposed to suicide than any other people who have attained a similar grade of civilization. If we should even quadruple the usual return in the bills of mortality, the suicides of London would still fall greatly short of those of other capitals.

The population of Westminster is above 182,000. The annual average of suicide during the 13 years ending in 1825, has been only 22. During the last eight years a reduction of nearly six suicides has taken place on the average of each year. In 1817 and 1821, the amount was only 17. Far from November being the most influential month, its average was only 2, while that of June was nearly 3. The proportion of male suicides to female was so much as 5 to 2. Nearly the same proportion of the sexes occurs also at Paris, where the suicides appear to be often five times more numerous than in London, in proportion to the population. In the first six months of 1819, the suicides at Paris were about 200. The persons taken out of the river alone amounted, in 1820, to 260; in 1821, to 309. In Prussia, the increase of suicide, and its present height, are very remarkable. In 1818, the total number was 650; but in 1822, so large as 859. At Berlin, from 1788 to 1797, one suicide appeared amongst 900 deaths; but from 1813 to 1822, there was one in 100 deaths. At Copenhagen the proportion has lately been 100 annually amongst 100,000 inhabitants. Even at Rouen, the number in a recent year is as large as that of London. In 1793, a stormy period for France, 1300 are said to have occurred at Versailles.—Amongst nearly 152,000 persons insured at the Equitable office, only 15 instances happened during 20 years.

We are compelled to attach a much lower importance to the influence of climate, both in health and disease, than was formerly assumed. In Europe, at least, the maladies of the individual seem to depend much more upon his habits, condition, and occasional local peculiarities, than upon the varieties of climate. Even in tropical climates, a large proportion of European mortality must be ascribed to the neglect of a congenial diet, and to a deficiency of self-control. Neibuhr, who saw all the companions of his travels perish around him, imputes their fate to their European mode of life. It has been thought that females suffer less from the change of climate than men; and their more regular and cautious habits may probably form an outwork.

In the epidemic cholera of India, the mortality of the European troops was greatly inferior to that of the natives; 27½ out of 100 of the former, were cut off; but so overwhelming a proportion as 80 among 100 of the latter.

Humboldt has furnished some details of the rate of mortality in New Spain: the average

of the whole kingdom was 1 in 30 annually. Mr. Bristed states the average of annual deaths throughout the United States to be 1 in 40.

The influence of climate in Europe is most visible in the various fatality of the same months to different cities. The axiom of Celsus on the seasons is not at present applicable to the northern capitals. In London, the autumn now appears the most healthy; after it, in succession, the summer and winter; and least of all seems the spring. Paris and Berlin correspond nearly with London; but Montpellier, Padua, and Milan, reverse the order.

When we speak of a healthy climate, it is gratifying to reflect, that, in most instances, it is man himself who has created these climates of health. Machiavelli, in his early epoch, anticipated this great truth: he remarks, that unhealthy countries become wholesome through the population which cultivates them.

Poverty, cold, and moisture (the two last of which are generally included in the first) are the most powerful enemies to the enjoyment of health and longevity; and affluence, on the contrary, is the strongest safeguard of the body. Epidemics are the offspring of misery, and upon the poorer classes their principal ravages are exhausted. Of an equal number of infants chosen among the poor and easy classes in France, the proportion of deaths among the former is found to be double. The mortality of women is, in some countries, and particularly in England, less than that of men, because they are more secluded from the conflict of life, and are less exposed to weather and to severe labour. In France, the mortality of the sexes is nearly the same, because the female of humbler rank there performs a large part of the manual and out-of-door employment. The conservative tendency of an easy condition is marked by the very inferior degree of mortality and of disease which occurs among insured lives—and generally among any large societies composed of persons in the enjoyment of competence. At the Equitable office, it was found, in 1810, that the actual deaths which had occurred among 83,000 persons insured during 30 years, were in the proportion of only 2 to 3 of what had been anticipated from the Northampton tables. The annual mortality at the Equitable, from 1800 to 1821, was less than 1 in 81 annually. At the University Club it has been, during 3 years, only 1 in about 90, annually. The annual deaths at the Edinburgh High School and Academy, are only 1 in 833. Far different is the fate of the slave: a fifth or sixth part of the negro slaves was formerly computed to perish annually; and of 20,000 exported to Rio Janeiro in 1823, 1400 had died on the voyage—which is, nevertheless, an improved report of a slave ship.

A remarkable table has lately been published by Mr. Morgan, illustrative of the increase and decrease of diseases in this country.

It comprises the diseases (certified by the medical attendant) which were fatal among nearly 152,000 persons insured at the Equitable, from 1800 to 1821; of every age from 10 upwards. The greatest number of deaths under any head, is 262 for "natural decay and old age." This item is nearly a seventh part of all the deaths. Next follows apoplexy, 242; consumption, 153; general fever, 146; dropsy, 137; palsy, 116; dropsy of the chest, 100; diseases of the liver, 79; inflammation of the bowels, 77—of the lungs, 73. Only 8 from calculous disorders. Angina pectoris yields 44 deaths; gout, 26; but small-pox, measles, and scarlet fever, not one. The deaths from consumption are scarcely 1 to 12; whereas, in the bills of mortality, they are at present 22 per cent.; and at the end of last century were 26 per cent.;—two facts which seem to indicate that it is less prevalent than formerly.

After enumerating so many varieties in the distribution of mortality, it remains to consider the causes which diminish it, and which have, in our own country, rendered that diminution so conspicuous.

The particular causes have been long generally admitted: such as improvements in ventilation, and in the general economy of hospitals; the general adoption of a more rational treatment of disease, and particularly of the antiphlogistic plan. They chiefly affect disease already formed, and promote a fortunate termination.

The general causes act on the entire mass of a nation, and operate in the prevention of disease. Among these, the increase of agricultural and commercial industry has multiplied the comforts of the lower classes, and has enabled them to procure a more spacious dwelling, more frequent changes of clothing, and food more abundant and more wholesome; inasmuch that the average mortality and health of every nation are mainly determined by the degree in which its government has encouraged these pursuits, or has checked their free course. So intimate a connexion subsists between political changes and the public health, that, wherever feudal distinctions have been abolished, wherever the artisan or the peasant have been released from arbitrary enactments, there also the life of these classes has acquired a new vigour; and it is certain, that even bodily strength, and the power of enduring hardships, are divided among the nations of the earth in a proportion relative to their prosperity and civilization.

We may easily conceive the different frame of body and of mind which is likely to grow upon the unemployed inhabitant of a decayed city, who gloomily wanders, without an object, through silent streets, whose pavement is choked with grass; and upon the active citizen, who feels himself a constituent member of a flourishing community, and who is attracted on all sides by invitations to the exercise of his faculties.

It is indisputable, that the average propor-

tion of deaths in England and her cities is less than that of any other country in Europe. And it may be added, that the powers of body and of mind are preserved to a late period, in higher perfection here than in other countries;—nowhere are the advances of age so slowly perceived, and nowhere so little manifested on the exterior. An analogous condition of vigour may be also observed in our animals, and in our vegetation; and if it should be replied, that this excellence is owing to the care bestowed on their culture, the answer applies equally to the human being, on whom more attention is here lavished, and who is really here an object of greater value than elsewhere.

If political and moral circumstances actually possess so preponderant an influence on the production of disease, and on the guidance of its fatality, it seems to be incumbent on our profession to study their progress, and to profit by their results. A peculiar set of diseases appears to belong to every age; and it may almost be affirmed, that there is also a mode of treatment adapted to every age.

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From the Medico-Chirurgical Review.

#### REDNESS OF THE INNER SURFACE OF BLOOD-VESSELS.

As this phenomenon leads many people astray, in their speculations as to its causes and effects, we shall take this opportunity of stating the sentiments of Laennec on the subject, as they are entirely in consonance with our own observations.

Corvisart noticed this redness, but avowed his ignorance of its nature and cause. Frank observed it throughout the whole course of the arterial system, and concluded that it was the cause of a peculiar and uniformly fatal fever! The same opinion has been adopted by Kreysig, Bertin, and Bouillaud. But, let it be remembered, that mere redness of a part naturally white, does not authorize us to pronounce it in a state of inflammation. The phenomenon is frequently seen on the inside of the aorta and pulmonary artery. The colouring is of two kinds, scarlet, and a violet hue. Sometimes the redness is confined to the inner membrane—at other times, it penetrates the fibrous, and even the cellular texture. The colour is quite uniform, as if painted, and without any trace of vascularity. Sometimes this stain diminishes progressively from the origin of the aorta; but frequently it terminates abruptly, with irregular edges. In the midst of a very red portion, we sometimes see a circumscribed spot of the natural white colour. When the aorta contains very little blood, the redness only exists in the part in contact with this fluid. The surface of the sigmoid and mitral valves sometimes exhibits this phenomenon—occasionally the whole of the arterial system. Sometimes the auricles and ventricles alone are tinged—and then it is observed that the heart is full of blood, and the arteries

nearly empty. The redness is attended by no appreciable thickness of the membrane, and entirely disappears after a few hours' maceration. M. Laennec is very doubtful whether this redness ever gives rise to any general symptoms, so constant or severe as to indicate its presence. For our parts, we have no doubt on the subject. Like him we have found it in bodies dead of different affections, and without any symptom, during life, that could lead to a suspicion of its existence.

M. Laennec has seen the violet hue of the membrane in subjects dead of putrid fevers, emphysema of the lungs, and diseases of the heart. All these individuals had remained long in a moribund state, with suffocation. In all, the blood was very fluid, evidently altered, with signs of premature decomposition in the body. Accordingly, we more frequently observe this phenomenon in summer than in winter. Both kinds of redness—especially the violet, are accompanied by more or less softening of the heart, and an increased humidity of the arterial tunics—the consequence, most probably, of putrefaction.

Upon the whole, we may safely conclude that this tincture of the internal surface of arteries is owing either to some morbid change in the blood itself, or, at all events, to some process which takes place in articulo mortis, or post mortem. A remarkable instance lately occurred in St. George's Hospital, in the case of a man who died of phlebitis. The whole arterial system was tinged.

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From the Medico-Chirurgical Review.

#### PRIZE ESSAY AND DISCUSSION ON TUBERCLES.

The Royal Academy of Medicine offered last year a prize for the best dissertation on the subject of tubercles, and, at a late sitting, M. Rullier read a report of the commission appointed to examine the candidate essays. The motto of the first essay was "*cujusvis est hominis errare.*" The author passes in review the clashing opinions entertained by pathologists as to the origin of tubercles. Are these bodies (as some represent them) accidental tissues developed in certain parts by a kind of anomaly of nutrition—or are they the product of a morbid secretion, a species of concrete pus disposed in the areolæ of parts? The author cannot tell. Another, and perhaps a more interesting question is, how are these bodies resolved? The author thinks there are three modes employed by Nature—softening, atrophy, and resorption. Messrs. Thenard and Dulong having found in tubercles the same salts, and in the same proportion, as in bones (phosphate and carbonate of lime) the author attributes the formation of these bodies to a deviation of the above salts from the bones. He strengthens this hypothesis by the fact of the greater lightness of the bones of tuberculous subjects, and the exuberant proportion of phosphate of lime in the milk of cows affected

with tubercles. The commission attaches no great importance to this theory. After alluding to the fact, that these bodies are found in the fœtus, he observes that hitherto they have been discovered only in the mammiferous animals and in birds. In the first class, they are seen almost exclusively in the herbivora. It is certain that tubercles have never been detected in dogs. In respect to the EFFECTS of tubercles in the organs where they exist, it appears that these effects are nearly null till the tubercles begin to soften. Then the circumjacent parts inflame, ulcerate, and form accidental tissues destined, he thinks, to isolate and envelope them. As to the primitive seat of these bodies, the author concludes that it is neither in the lymphatic system, nor in the mucous follicles, but in the cellular tissue. Tubercles in the lungs, according to the author, are the sole cause of phthisis, notwithstanding what Bayle and others have said to the contrary. The commission think this assertion is too absolute, and admit the existence of cancerous, calculous, melanic, and even *osseous* phthisis. The commission, also, blames the author for having attributed too much influence to pulmonary inflammation, in the production of tubercles. They think that these bodies exist before the phlegmasia—or if they become developed afterwards, it is owing to the cachectic habit of body produced by the preceding inflammation. The commission ridicules the assertion of Laennec, that tuberculous excavations have healed—or, in other words, that phthisis has been cured. Speaking of bronchial phthisis, where the softened tubercles have discharged themselves by fistulous openings into the œsophagus or bronchia, he notices the curious fact, and illustrates it by a preparation, that the perforation is always through one of the cartilaginous rings, and never through the intermediate membrane. He remarks that tubercles of the mucous membrane are very generally confined to the sub-diaphragmatic portion of the alimentary canal—and especially to the lower portion of ileum, where they lead to ulceration. In laryngeal phthisis, he maintains that tubercles in the larynx are the primary source of the disease—an opinion which the commission contests. They believe that ulceration is the primary condition.

In respect to tubercles in the brain, the author notes the intermission of the nervous disturbances dependent on these tubercles—a fact that is truly inexplicable. It certainly strengthens the doctrine, that a pure intermittent fever itself may depend on some lesion of structure, notwithstanding the periodical states of apyrexia. The author thinks that tubercles in the bones, a disease which has excited but little attention, are the cause of many cases of white swelling, spontaneous luxations, vertebral caries. He showed a preparation exhibiting a veritable tuberculous cavern in the spinal column. The commission passed high encomium on the Essay, but did not adjudge to it the prize. They awarded its author, however, a medal, value 500 francs.

The reading of this report led to an animated discussion. M. Andral freely censured the doubts cast by the commission on the possibility of cicatrization in tuberculous excavations—and consequently the sanability of phthisis. Both these are proved, he observed, by symptoms, by pathological anatomy, and by analogy. Thus, a person will present all the symptoms of phthisis, with unequivocal pectoriloquism under one of the clavicles. The patient recovers, and dies of some other disease; and in the place where the pectoriloquism was heard, will be found a cavity lined, or filled, or filling up with various kinds of tissue, proving the restorative process that had been going on or completed. Analogy shows us that the excavations of diseased lymphatic glands will fill up in various ways. M. Chomel also criticised the commission on this point. They admitted the possibility of cicatrization in cases of insulated tubercles—and this was all that Laennec and others contended for. No man ever stated, that a tuberculated lung would recover after several excavations had formed. This physician adverted to the intermissions of functional disturbance in organic diseases. He conceived that, in such cases, the symptoms of functional disorder were not exclusively dependent on the structural lesion (which is permanent of course) but upon some accidental and temporary causes, which unite their influence with the organic disease. M. Rullier replied and explained, by which explanation, it appeared that he entertained nearly the same opinion as Laennec and others, respecting the cicatrization of tuberculous excavations.

The intermission of symptoms in organic diseases, however, led to some sharp controversies—but the problem was not solved by the “collective wisdom” of the Academy. On breaking the seal of the motto, the author’s name was found to be M. Larcher, internal élève of the Mansion de Santé, rue Faubourgh St. Dennis.

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From the Medico-Chirurgical Review.

#### SPINAL DISEASE.

Mr. Auchincloss, Surgeon to the Glasgow Royal Infirmary, has published a case in the first number of our Glasgow cotemporary, which he considers to be valuable, as tending to confirm the opinion first suggested by Mr. Brodie, regarding the difference in the nature and treatment of ulceration confined to the *intervertebral* substance—and ulceration affecting the *cancellous structure* of the bone. The following case is offered in illustration.

Case. “7th Sept. James Graham, by trade a gardener, aged forty-five, of rather a spare habit, though healthy, met with an accident five weeks ago, the nature of which was as follows:—While standing on a bench four feet high, with his arms extended in the act of pulling fruit from a tree, he happened to lose his balance, when, to support himself, he

caught hold of one of the branches. After swinging a few seconds, the branch broke, and he alighted perpendicularly on the ground, a height of about two and a half feet. He felt little uneasiness at the time of the fall, which occurred at three o'clock, P. M. and, accordingly, continued at his employment during the remainder of working hours. Since the following morning, he has complained of a sense of tightness around the lower part of the chest, with pain in the right hypochondrium, particularly aggravated by motion, though not in the least impeding full inspiration. Pulse 76, weak; bowels moderate; other functions natural. Was bled from the arm three different times by a medical gentleman, whose advice he requested at the time; and has had two blisters applied to the region of the liver, but without producing any good effect.

"The impression on my mind, from a very cursory examination of his ailments on first seeing him, was, that the liver, or muscles in its vicinity and about the back, had sustained some degree of straining by the fall. He was again blistered and otherwise treated as for simple injury of those parts, during the first fortnight. On one occasion, he experienced much relief from cupping near the spine on the right side, but was in no other respect benefited by the treatment.

"The symptoms of his complaint had become now much more apparent, and, on a more careful inquiry being made, were found to be connected with slight curvature of the sixth, seventh, and eighth dorsal vertebræ. The convexity of the curve was turned outwards, and he seemed to suffer but little uneasiness in the part when firmly pressed upon. The following symptoms were then noticed:—Numbness of the lower extremities, with much bodily weakness and inability to sit, for even a few minutes, in the erect posture; scanty secretion of urine, costiveness, and some swelling of the abdomen; constriction and pain around the lower part of the thorax, most acute on the right side; loss of appetite, thirst, and a weak accelerated pulse.

"On the following day, the caustic potash was applied on each side of the protuberance, but without occasioning relief at the time, or on the sloughs separating, four days after. He became daily weaker, and expired on the 21st of October, exactly eleven weeks and two days from the date of his falling from the tree. For some time previous to death, his belly was tympanitic and swollen to a great size; and he was much troubled with feelings of numbness in the left shoulder.

"*Inspection.*—The pleuræ were extensively adherent on both sides. The lungs, with the exception of the posterior part of the right, which was collapsed, exhibited their natural appearance and structure. There was an abscess, containing about five ounces of curdy-looking purulent matter, in the posterior mediastinum. This was situated in front of the sixth, seventh, and eighth dorsal vertebræ, and encroached considerably on the right side of

the chest. The body of the seventh vertebra was almost wholly destroyed by ulceration; but this had proceeded much farther on the right than the left side. The bodies of the other two vertebræ were also partially absorbed. In no part, however, had the disease extended to any of the intervertebral cartilages, all of which remained in an entire state. Notwithstanding the extent of destruction, the osseous structure both of the diseased vertebræ and of those in the neighbourhood, was of its natural hardness and colour. The liver was healthy, as also the other abdominal viscera."

There are very few cases of this kind on record. Mr. Brodie has offered two, in his treatise on diseases of the joints.

"In treating of this disease, with the view of distinguishing it from a similar affection originating in the intervertebral substance, Mr. Brodie regrets that there should exist no better criterion than the following, to direct us in our judgment. In ulceration confined to the cartilage, he remarks, the patient is benefited almost immediately on the issues being made, or, at least, feels himself uniformly easier after each application of the caustic; but this does not take place in the other species, for in it issues of every sort constantly fail in affording even the slightest relief. Moreover, he supposes that the form of disease which begins in the substance of the bone, is generally rapid in its progress, being more immediately followed by suppuration than that which commences in the intervertebral substance; and that, in consequence, destruction of the contiguous vertebræ takes place to a much greater extent in the one species of disease than in the other. 'But farther than this,' says that excellent surgeon, 'nothing which I have hitherto observed enables me to point out any other circumstances, in which the symptoms of these different diseases differ.'

"It affords me much pleasure to be able thus far to bear testimony to the truth of Mr. Brodie's statements, respecting the diagnosis of these two affections. From a knowledge of the facts adduced by him, and to which I have now referred, I was enabled, in the present instance, to form an opinion as to the true nature of the disease, which was afterwards fully justified by the dissection. The case certainly affords a very striking illustration of the great rapidity of the disease, and of the utter inefficiency of caustic issues as a means of cure."

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From the London Medical Gazette.

#### CASES OF RUTURED URETHRA, WITH REMARKS. By H. EARLE, F. R. S., &c. &c.

William Taylor, æt. 42, was admitted into St. Bartholomew's Hospital October 20th, 1826, having fallen across the edge of a door on his perinæum. Considerable bleeding took place from the urethra, and effusion into

the scrotum and perinæum opposite the bulb. He had not passed any urine for many hours. An attempt was made to pass a catheter by the dresser of the day without success, and Mr. Stanley, who was passing through the ward, was requested to see him. After some time he succeeded in passing a small elastic gum catheter into the bladder, and some water mixed with blood was drawn off. Mr. S. distinctly felt the rupture in the urethra in passing the instrument, which was directed to be left in the bladder. I saw the patient the following day, and found a tumour of the size of a large walnut rather to the left of the bulb. The scrotum was black with effused blood, but not much distended. Urine mixed with blood continued to flow through the catheter. The patient was treated on a strict antiphlogistic plan. He was largely bled from the arm, and twenty leeches were applied to the perinæum. He continued to go on favourably for some days without any increase of swelling and no appearance of effusion of urine. On the 25th the catheter slipped out, and the patient attempted to re-introduce it, which caused some return of arterial bleeding. He had suffered during the preceding night with severe rigours and fever. I introduced a large catheter without difficulty, in doing which the laceration in the urethra was distinctly felt about the bulbous part. The shivering and fever returned at night, and the patient removed the catheter, which was followed by considerable hemorrhage. On the following day I found him very ill, with frequent disposition to shiver, and the tumour in the perinæum had increased in size, and was very painful to the touch. I determined now to cut down and give exit to the effused blood and urine. A free incision was made through the tumour, and extended downwards towards the anus. The membranous part of the urethra was distinctly felt, but not opened, as the lacerated opening communicated directly with the upper part of the incision, and afforded a ready exit for the urine. The wound bled freely, and gave the patient much relief. From this time he was able to pass water without the assistance of the catheter, partly through the wound, but principally through the natural passage. The wound suppurated kindly, and speedily healed. It was necessary, for some time, to pass bougies, to counteract the effect of the contraction at the cicatrized portion of the urethra.

*William Barnett*, æt. 36, was admitted September 13th, 1827, into Pitcairn's ward. He stated that on the previous evening he had fallen about fifteen feet, and struck the perinæum across an iron bar. Violent hemorrhage took place from the urethra, and the scrotum and integuments became distended with blood, accompanied with severe pain. He sent for a medical man, who made many attempts to pass a catheter without success. He was bled, and leeches were applied to the part. He passed a night of great misery, and the following day was admitted into the hos-

pital. His bladder was, at this time, sensibly felt above the pubes; no urine had passed since the accident, (sixteen hours,) nor for some hours before. His countenance was anxious, and expressive of much suffering; pulse 100, and full; perinæum and scrotum much distended, and of a dark, livid colour: there was, however, no external wound. He had made many attempts to pass water, but only voided blood. I cautiously introduced a full sized silver catheter, which passed readily down into a cavity filled with coagulum between the rectum and membranous part of the urethra. The finger introduced into the rectum readily detected the point of the catheter in this situation. This being apparent, I immediately made a free incision opposite to the bulb of the urethra, and extended it parallel to the rupture to the extent of two inches. A quantity of coagulum and fresh blood escaped, and the catheter became apparent, passing through the ruptured opening at the upper part of the bulb. I attempted to introduce an elastic gum catheter from this part into the bladder, but not readily succeeding, I desisted from any effort. The finger introduced into the wound passed into a large cavity filled with coagulum. It did not appear that any urine had been effused. The extensive extravasation of blood and the pressure of the coagula had, probably, prevented this. He was placed in a warm hip bath, which encouraged the bleeding from the wound and loosened some of the coagulum. Whilst in the bath some urine flowed through the wound. He now became very faint and was removed to bed, and the bleeding was restrained by the application of lint and cold cloths. In the evening he was much relieved, with a moist skin and soft pulse. He had taken some castor oil, and had an ounce thrown up, which acted well. Urine mixed with blood continued to dribble away. He passed a tranquil night without any return of bleeding. His pulse was small and intermitting. On the 15th he passed about half a pint of urine voluntarily through the wound, which relieved him much. On the 17th he experienced difficulty in passing his water through the wound, and the dresser endeavoured to remove a coagulum which presented itself. This was followed by a return of arterial bleeding, which continued to flow through the greater part of the night, until the patient was alarmingly faint, requiring the administration of brandy and ammonia with opium. On the 18th no urine had passed, but the bladder was not distended. He continued very faint, with a feeble intermitting pulse. On raising him upon a night chair, he was able to pass water through the wound. Suppuration now began to take place, and no farther alarming symptoms occurred. His pulse continued to intermit for a considerable time, the swelling of the scrotum subsided, and urine passed freely through the wound. October 2d, I introduced a good sized metallic bougie, No. 12. On reaching the situation of the wound it met with some resistance, which

was readily overcome, and the instrument passed on without difficulty into the bladder. On withdrawing the bougie, the patient passed some water through the natural passage. I introduced the bougie every second day. The wound in the perinæum now rapidly healed, and was closed by the 8th of October. The cicatrix had however so great a disposition to contract, that it was necessary for a considerable time to pass the bougie every second day.

The preceding cases afford a good illustration of the best method of treating these serious accidents, which are sometimes followed by the most disastrous consequences from the effusion of urine, without the most prompt and decided treatment on the part of the surgeon. I have seen several of these cases, varying in extent and in the degree of danger, and, from an attentive review of all the circumstances attending them, I do not hesitate to state, that, where there is reason to apprehend that the urethra has given way, either by ulceration or from the application of force, the most judicious plan is to make a free opening in the perinæum, by which we at once secure a ready exit for the urine, blood, and matter, and prevent any extension of the mischief. This is far better than to persevere in endeavouring to introduce instruments into the bladder, by which the injury is often much increased, while the attempts prove abortive; and even should they succeed, it frequently happens that the presence of the foreign body in the urethra keeps up the irritation and increases the malady. In many of these cases where there is no external wound, and where the patient is ignorant of the nature of the injury, and the danger to be apprehended, it is often difficult to persuade him or his friends of the necessity of such an operation. Much decision and firmness are required on the part of the surgeon, who should act at once, or he may be too late to prevent extensive or even fatal effusion of urine. No possible danger is to be apprehended from the performance of the operation, which places the patient in a state of security, and enables nature to set about her process of reparation. The wounds always heal readily if properly treated, and the external incision be of sufficient extent.

In looking back to the circumstances of the two cases which have been above related, we find that, in the first case, although a catheter was successfully introduced, and the water drawn off through it, yet it became necessary to operate after the lapse of several days, in consequence of the constitutional disturbance excited by the presence of the catheter, and commencing suppuration in the seat of the effused blood at the rupture in the urethra. Upon giving exit to these, the urine flowed without difficulty through the natural passage, and the hemorrhage from the urethra ceased altogether. I have no doubt that in this case the relief would have been more speedy, and the patient would have recovered sooner, if an incision had been made immediately after the receipt of the injury. In the

second case, I have every reason to believe that the effusion between the urethra and rectum was much increased by the repeated attempts to pass catheters. The presence of so much coagulum kept up an hemorrhagic action in the artery of the bulb which had nearly proved fatal. The successful issue of the case depended, I am convinced, on the free incision which was made early, and the abstaining from all attempts to pass instruments until the breach in the urethra was nearly repaired. The surgeon has two things to learn; when to act, and when to be a passive observer of nature's processes. In the present case, when he has secured a free exit for the urine, he should refrain from any farther interference until the natural efforts at reparation have nearly come to a stand. He is then called upon cautiously to restore the natural passage, and obviate the contraction which would follow the complete cicatrization of the wound.

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From the London Medical Repository and Review.

**UGHT THE PERINÆUM TO BE SUPPORTED WHEN THE HEAD OF THE CHILD IS PASSING THROUGH THE OS EXTERNUM? By CHIRURGUS.**

Having noticed a suggestion in the last number of the Repository, respecting the treatment of lacerated perinæum, I am induced to propose the question to the profession, whether or not it be proper to interfere at all with the perinæum, or with the head of the child in its passage through the os externum in cases of natural labour? Some will perhaps think that I am only displaying my ignorance in putting so simple a question, or, indeed, in endeavouring to raise a question at all about a subject which hardly any one doubts. But to clear myself from the imputation of professional ignorance, I beg to state, that I have attended several hundreds of cases of natural labour without ever meeting with one of laceration of the perinæum, though I make it a point never to support the perinæum, or to keep back the head of the child in its passage through the external orifice. Whether I may attribute this success to chance, or to my mode of practice, I do not pretend to say; but I have reason to believe that more harm than good is likely to result from interfering with the labour at this stage. Teachers of midwifery have not yet agreed amongst themselves about the manner in which the perinæum ought to be supported. Some of them recommend the head to be kept back with the hand, until the external orifice shall have had time to dilate gradually to a sufficient extent to allow the child to pass without incurring a risk of having the perinæum lacerated; and not to supply any pressure against the perinæum itself. This, perhaps, is the only method likely to prove beneficial in guarding against a rupture of the perinæum; but in acting thus, do we not incur a risk of producing a lacera-

tion of the *uterus*? We know that the uterus acts usually with very great force when the head is in this part of the passage, and any impediment, placed in the way of the descent of the child, would require that force to be increased to enable it to overcome the opposition. The uterus would be thus endangered, while the risk of rupturing the perinæum would be but little, if in any degree, diminished.

Others recommend the perinæum itself to be supported, by pressing against it either with the naked hand, or with the hand interposed by a napkin. It is quite clear that this mode of proceeding can render no service whatever in guarding the perinæum against an accident; for, whatever force we apply to this part, the labia, in being pushed out by the head, rather than would be necessary if the perinæum were allowed to project, would be more likely to separate at their lower junction, than if the passage were allowed to yield a little in all directions. Moreover, the perinæum, when the pressure is employed in this way, is not unlikely to be bruised between the head of the child and the hand of the accoucheur.

A third mode of supporting the perinæum is that of grasping it between the thumb and four fingers of the right hand, or of squeezing both sides towards the middle. This method is liable to the same objection as that of keeping the head of the child back, because the passage will, by this means, be rendered more confined, and the progress of the head impeded, requiring, therefore, a greater effort on the part of the uterus to expel its contents. The risk of laceration on the part of the uterus is in proportion to the force with which the organ contracts and to the impediment placed in the way of the head; and as a rupture of the womb would be of much worse consequence than a laceration of the perinæum, it should be considered, whether we ought to expose the organ to such a serious risk when so trifling, if any, benefit can accrue from our interference with the natural process? It should not be forgotten also that the patient is subjected to additional pain by such an interference, for unless the head be kept back until the perinæum be relaxed by the pressure of the head, we do no good, and relaxation cannot take place without pain.

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From the London Medical and Physical Journal.

### HYDROPHOBIA.

#### *Proposal to employ Heated Air in Hydrophobia.*

Nearly all the remedies with which we are acquainted have originated from some accident, which an observer has taken advantage of, and, by a reflective operation of the mind, has perceived to be applicable, if its effects were moderated, to the cure or alleviation of some disease or disorder of the human body. This method of reasoning, and its consequent

application, is and has been productive of much good, and forms a rational ground of medical practice, even if pursued without any connexion with anatomy, physiology, or any other branch of the science of physic. But a fanciful and sort of guess application of remedies must necessarily, in general, be attended with so little benefit, and make an inroad for so much mischief, that I feel the full force of venturing the proposal of one upon such basis, and can easily foresee the ridicule which may arise from its failure. There is, however, in the instance I am contemplating, such a freedom from evil in its consequences, and the case in which I propose it is so totally out of the reach of the present aid of medicine, that it becomes in its result at most an error in fancy, while it will, if successful, be a means of preserving the lives of those who are decidedly out of the reach of medical aid. It would, therefore, be an inhuman error to hold back the present proposed remedy, merely because, in advancing it, there is only a glimpse of hope, instead of a sanguine expectation.

There is a disease which is manifestly communicated by the introduction of a powerful poison into the system. When this poison has given rise to a certain phenomenon, it is invariably productive of death. This disease, like most others, has its name from its prominent symptom, which in this instance is a dread of water, (hence the term hydrophobia.) Every remedy which has been employed here, and, I believe nearly all the articles of our long list of *materia medica* have been essayed, has deceived the flattering hope of the unfortunate, who has been driven to a treacherous repose upon their power. In these cases, anatomical investigation affords but little light, since the exposures of the scalpel display scarcely any two cases alike; and, although traces of inflammation are discoverable, I believe, in all the cases examined, yet its seat is so uncertain, and its indications sometimes so obscure, and, when rightly guessed at, customary remedies prove so inert, as to set at defiance the usual advantages of scientific procedure in common circumstances. Pathology of the disease there is none, and physiology affords not even a data to explain the phenomenon which gives rise to its name. It may be urged that, if this should be explained, the disease would be understood; but I conceive this is a mistaken notion; for, although the mental emotion of fear, as connected with water or any other undulating body, is the most distressing symptom to the sufferer, yet it would not appear that this dread is the cause of death. *On the contrary, this feeling subsides, I believe, as dissolution approaches.*

Without encumbering the subject further with crude observations to no useful end, it is my wish to propose, through you, to the public, that future cases of this disease may be submitted to the full influence of highly heated air, so as to produce from the skin and lungs a copious exhalation.

This treatment affords to my mind a hope of success, because, in all the more virulent and

manifestly contagious diseases, as plague, small-pox, measles, and scarlatina, there is an effort to throw off the poison by cuticular action; and, in the plague, those who get bubo and cuticular disease early do for the most part recover, and, in some instances, *spontaneous sweating has appeared to carry off the disease.*

The abuse of language is often the origin of error, but the origin of words is always in absolute truth, because it is merely the absolute and imperious application of a term to denote a fixed fact; and therefore, in attaining to the precise and exact meaning of the term in its original import, we must always acquire a knowledge of the determinate fact to which it was originally fixed as a denotement.

In the histories of nations, it appears well established that the arts and sciences rise, flourish, and decay, in a mutual conformity; and hence, where the lasting monuments of the former show the perfection to which they have been cultivated, it is at once known that the sister sciences cannot have been neglected, and the degree of excellence in the former is plainly indicative of the eminence reached by the latter. Hence the lasting monuments in art of the ancients are the powerful witnesses of their superiority in science.

Now, our terms of science, more especially of medicine, are derived from the same source as the admired remains of ancient sculpture; and the knowledge of medicine attained to by those people (the ancient Greeks,) was, in all probability, (whatever the moderns may think to the contrary,) equal to their success in the arts.

There is, derived from the Greek language, a word now rarely used by the profession, which lexicographers say means *expeller of poisons*. This word is *Alexipharmic*, and all the drugs comprehended under that term have the property of producing sweat. There would appear, therefore, some reason to believe that the ancients had a knowledge of the means of expelling poisons, and, if we have a correct application of their term to the same class of medicines which they used, it must have been by sweating; and hence an argument is raised upon their superior knowledge to the use of a sudatorium in the cure of hydrophobia.\*

But not to trust to authority for what argument should enforce, it may be worth the time to inquire what would be the dictates of nature (that is the disease) to remedy this fatal and distressing evil.

The natural and evident cause of this disease is the introduction of a powerful poison into the system: the natural and evidently rational method of cure must be the expulsion of that poison, unless some medicine were known

(and there is none) which possessed the property of rendering innocuous that virus. The expulsion of a poison mixed with the blood cannot, like poisons in the stomach or bowels, be removed by emetics or aperients, but must be cast off by the excretory pores of the blood-vessel system; so far the sudatorium is, in the simplicity of genuine philosophy, clearly indicated as the first point of external cure.

To moderate the leading symptom of disease, the dread of water, nothing appears more rational than to induce the powerful counter-desire thirst; and this the sudatorium, by its abstracting the fluids of the body, will tend powerfully to produce: so far the heated air would seem clearly indicated by the dictates of the disease.

I have before stated that there exists no pathology of the disease which is deserving of attention, and the leading phenomenon of the disease has not an explanation even plausible; but it does probably depend upon that concomitant morbid irritability of the body which is so peculiar to this kind of poisoning. There is, perhaps, no means of relieving an undue irritability of body, not dependent upon actual debility, so efficacious as the sudatorium: and hence it would appear also indicated by the nature of the disease itself.

Even if the disease be inflammatory, although the traces of inflammation are, I believe, scarcely sufficiently marked to account for death, the sudatorium would be highly useful, and strongly indicated, inasmuch as it is a powerful antiphlogistic remedy, especially in erratic disorder of the inflammatory kind.

In a state where impressions so trifling as, in the ordinary circumstances of life, would be unheeded, do excite so powerfully the animal frame, a rarer medium, as less powerful in conveying impressions, would be enforced by the salutary dictates of nature, in the same way as she seeks cooler air in fever, and warmth in debility. And hence it is probable that the sudatorium would not only be useful, but perhaps the most pleasant application to the feelings of the distressed in the disease.

These, sir, are the circumstances which influence me in hoping benefit from this practice; and I beg, through you, to submit them to the consideration of the profession at large, that they, with whom they may weigh powerfully as they do with me, may put the opinion to the test. The life of a private individual may pass without affording a case in point for experiment, so that to wait the experience of myself upon a subject so important to my fellows, would neither be justifiable nor humane; and this proposed remedy may as well be fairly and fully tried, as to suffer the miserable subjects of a hitherto universally fatal disease to lie, as they do, curious exhibitions in our hospitals for an unerring prognosis of death. It appears to me that the sweating should be long, frequent, and indeed carried to an extreme point. Z.

\* The name of the herb Contrayerva (or counter-poison) must have originated in its actual or supposed power, and this medicine also is a sudorific; as is also the plant Serpentaria, supposed to be efficacious against the bite of serpents.

From the London Medical Gazette.

**CASE OF DISEASED ANKLE, with some Observations on Exercise.** By A. COPLAND HUTCHISON, F.R.S., &c.

In the Medical and Physical Journal for October last, page 305, the case of a young lady, with diseased ankle, occasioned by using too great exertion at her gymnastic exercises and in dancing, is detailed by Mr. Rose, of Park-place, with so much distinctness and candour, from the very commencement to its fatal termination, that I consider the public to be indebted to him for his having thus early called their attention to a growing evil in some schools as regards an excess of bodily exercise among the rising generation.

Gymnastic exercises in this country are, at present, too indiscriminately used by young people of both sexes, and in some instances they are carried to too great an excess; for, however much we may advocate the great advantages of good air and proper exercise as necessary to a robust state of the body, particularly during the period of youth, conjoined however to a due supply of animal food,—yet every day's experience teaches us that the degree of exercise ought to be regulated according to the constitutional capability of the individual who is to be the subject of it.

On the other hand, I would ask the experienced physician or surgeon, what can be more unnatural, and consequently more injurious to health, than that the only exercise some young ladies are permitted to enjoy after their sedentary studies are over, should be that of walking round and round a green plot in a garden two and two like horses in a mill, and as stiff and upright, with tight laced stays, as if they were soldiers on parade, all attention to the orders of their commanding officer? This antiquated system of school discipline, with which my professional avocations have brought me in contact, is altogether wrong. It ought, and must be abandoned, for the more reasonable and wholesome plan of a well-regulated bodily exercise; at the same time we ought strictly to guard against an excess of it in weakly and delicate children.

In all cases of this nature parents should be made aware of the propriety of consulting their medical attendant, when the child is sent to school, as to the degree of exercise of which the child is capable, both as regards gymnastics and dancing; and at each holiday time the same professional inquiry ought to be made.

Why is it that we so seldom meet with distortions of the spine among young women in the lower, and so much more of it among the higher ranks in life? The exemption in the former case is, because their dress admits of a much freer exercise of the muscles that move the trunk of the body than in the latter class; and, also, because they are obliged from circumstances, and at a very early age, to move about and perform all sorts of domestic work, and by which, therefore, a

greater degree of strength is given to such muscles as support the superstructure of the body. In the latter case such advantages are altogether wanting.

Again, let us but just glance at the comparative frequency of spinal disease among the male and female sex. The same cause operates here; for boys will not be restrained in their exercise after school hours, and hence it is that they are comparatively exempt from curvatures of the spine.

I shall now briefly relate the particulars of a case somewhat analogous to that detailed by Mr. Rose, as far, at least, as regards the seat and rapid march of the disease, and in which I felt great interest, for the patient was an only son.

On the 22d February last, master B. Fosset, aged 14, had been dancing at school with more than his usual spirit. The next day he walked between three and four miles, and, as far as I can learn, without any degree of lameness. On the evening of the 23d, he was teased with some degree of itching on the outer part of the left heel; 24th, a circumscribed red spot appeared precisely where the itching occurred; 26th, redness of the part increased; 27th, great pain, swelling, and inflammation; and on the 6th March, for the first time, I visited the young gentleman at Hendon. He was of a very lax and delicate fibre, and apparently of a strumous habit of body. His pulse was hectic, breathing quick; he had some cough, and his countenance was expressive of anxiety and anguish. The foot and ankle were considerably swelled, the toes drooped, and he had not any power to raise them. There was a small opening by the side of the tendo achilles and over the os calcis, through which considerable discharge issued. This was dilated with the bistoury, when, on examination with the probe, several of the bones of the foot and ankle were found to be carious.

On the 22d of March, a consultation was held at his father's house, in Mornington Place, whither the patient had been removed some days before; Sir Astley Cooper and two physicians, friends of the family, having met me. It was there decided that I should remove the leg below the knee, on the following day; Sir Astley remarking that he had seen one case do well after amputation, where there had been the same state of lungs and caries of the foot as in that before us. Forty drops of tinctura opii were administered to the patient on that evening, and the next day I removed the limb, without the loss of more than two or three ounces of blood, assisted by Mr. Chevalier and Mr. Wade; two other gentlemen being present.

On examining the state of the ankle after the removal of the limb, by forming a crucial incision of that which had been already made, I readily turned out, with my forefinger, the whole of the os calcis, in a carious state. The tendo achilles was quite detached from the bone, but at the place of its insertion it had a scabrous bony surface, as if, during life, it

had been torn from its insertion: but this cannot possibly have been the case, as it appears, by all we can learn, that he walked so very far, without any lameness, on the following day after he had exerted himself so much and so unusually in dancing. Every bone of the tarsus, as well as the ends of the tibia and fibula, were in the same state as the os calcis, and not a vestige of peristeum or cartilage remained upon any of the bones forming the ankle-joint. Such destruction of parts, in so short a period of time, never before occurred in the long course of my practice.

During the first day or two after the operation, our little patient seemed to be doing tolerably well, notwithstanding that his pulse kept up. After this, however, his pulse somewhat increased in frequency, with his cough and breathing; which last continued to oppress him greatly until the 26th March, being the fourth day after the operation, when he died, the thigh and amputated stump being free from swelling or inflammation.

Before we conclude the remarks on this case, it is but proper to mention that the treatment, before I saw the patient, and subsequently, was such as is usual under similar circumstances—namely, perfect rest, leeches to the part, evaporating lotions, and, alternately, fomentations and poultices. The medical treatment was chiefly directed to the establishment of a healthy action in the bowels and skin by the usual remedies, and the rapidity of the circulation was endeavoured to be arrested by the administration of digitalis.

From the Transactions of the Medico-Chirurgical Society of Edinburgh.

#### OBSERVATIONS ON THE CAUSES OF THE SOUNDS PRODUCED BY THE ACTION OF THE HEART. By JOHN WILLIAM TURNER, Professor of Surgery to the Royal College of Surgeons, &c.

If the ear be applied to the chest in the region of the heart, either directly or through the medium of the stethoscope, there are perceived, for each pulsation of the arteries, two sounds or beats following each other in immediate succession, and then a short interval is observed, during which no action or motion of the heart is perceptible; and, after this, the two consecutive sounds are again heard.

In many cases, if the hand be applied to the left side of the thorax, two succussions are felt in immediate succession, followed by an interval of rest; the succussions corresponding with the sounds described as heard by the ear. It is not, in all instances, possible to feel two distinct succussions. In many individuals one only can be felt, and in some, under certain circumstances, the pulsation of the heart cannot be discovered at all by the hand applied to the thorax. In children, the double beat or succussion can, in general, be distinctly perceived by the hand, especially if the action of the heart be unusually increased. I

have often felt it in young persons after the age of puberty, particularly when the functions of the stomach have been deranged, and this derangement has been accompanied with a tendency to palpitation. In adults, the second succussion is more rarely felt; but I have not unfrequently perceived it, more particularly in individuals with small thin chests, and in those affected with dyspeptic and hypochondriacal or nervous ailments. In some instances in which the double succussion could not be perceived, when the persons examined were in the erect posture or lying on the back, it became observable by making them lie on the left side; and in all cases it has appeared to me to be rendered more distinct in this position of the body. Some individuals, in certain states, are themselves sensible of the double succussion by the sensation it produces in the chest, and which it communicates to the ear when the head is laid on the pillow.

This description of the double stroke or succussion which is felt in the region of the heart, applies to cases in which there has not appeared any reason to believe that disease of that organ existed. The double stroke occurs also in organic diseases of the heart; but I cannot recollect to have observed it more forcible or distinct in any cases of these diseases, than in cases in which there has been every reason to believe the heart to be sound.

One of the most remarkable examples of the sensation communicated to the hand by the double beat, which has fallen under my observation, was in a boy about fourteen years of age, who had just passed through the eruption of measles. He continued to labour under an affection of the chest. His pulse, however, was not quick, and was quite regular; but for each pulsation of the artery at the wrist, two distinct pulsations could be felt by the hand placed on the breast. The pulse afterwards suddenly rose to twice its former number, and the double beat of the heart could then be no longer felt. The symptoms of acute phthisis supervened, and rapidly proved fatal. On examination of the body after death, the heart was found to be sound; but there were several lymphatic glands enlarged to a considerable size, situated on the outer surface of the pericardium, near the basis of the heart and roots of the great vessels. The lungs were filled with tubercles, many of which had passed into the state of suppuration.

The two sounds and succussions communicated by the heart, have been considered and described as indicating the successive contractions of the ventricles and auricles. I have long felt doubtful as to the accuracy of this opinion, and have become satisfied, for reasons which I shall state, that it is erroneous.

The late M. Laennec has given a very precise account of the sensations which are communicated by the action of the heart to the hand applied externally, and more particularly to the ear through the medium of the stethoscope. I shall direct my observations to this account, since it may justly be re-

garded of the greater value, that the description contained in it is given by one who was eminently distinguished for his powers and habits of minute and accurate observation; and who, from his attention having been particularly turned to the subject, and his having enjoyed most extensive opportunities of investigating it, had examined the actions of the heart in its healthy and morbid states probably much more frequently than ever has been done by any other individual.

In describing the rhythm of the pulsations of the heart in its natural condition, as ascertained by the stethoscope, M. Laennec states, "At the moment the artery strikes the finger, the ear is slightly raised by a motion of the heart synchronous with that of the artery, and accompanied by a sound somewhat dull but distinct. The synchronism does not permit us to doubt that the phenomenon is owing to the contraction of the ventricles. *Immediately after, and without any interval*, a sound, more sharp and analogous to that of a valve which is raised, the crack of a whip, or the lapping of a dog, announces the contraction of the auricles." He adds, "No motion sensible to the ear accompanies this sound; *no interval* separates it from the duller noise accompanied by the raising of the ear, that indicates the contraction of the ventricles, which it seems to limit and to interrupt suddenly." "Immediately after the systole of the auricles, there is an interval of repose, very short, but well marked; after which, the ventricles are again perceived to raise themselves with the dull sound and gradual progression which are peculiar to them: the sudden and more sonorous contraction of the auricles follows, and the heart again falls for an instant into an absolute immobility."\* M. Laennec also estimates

\* "Au moment où l'artère vient frapper le doigt, l'oreille est légèrement soulevée par un mouvement du cœur isochrone à celui de l'artère, et accompagné d'un bruit un peu sourd quoique distinct. L'isochronisme ne permet pas de méconnaître que le phénomène est dû à la contraction des ventricules.

"Immédiatement après, et sans aucun intervalle, un bruit plus éclatant et analogue à celui d'une soupape qui se relève, d'un fouet, ou d'un chien qui lape, annonce la contraction des oreillettes."

"Aucun mouvement sensible à l'oreille n'accompagne ce bruit, aucun intervalle de repos ne le sépare du bruit plus sourd et accompagne de soulèvement indicateur de la contraction des ventricules qu'il semble borner et interrompre brusquement."

"Immédiatement après la systole des oreillettes, il y a un intervalle de repos très court, mais cependant bien marqué, après lequel on sent les ventricules se soulever de nouveau avec le bruit sourd et la progression graduelle que leur sont propres; suit la contraction brusque et sonore des oreillettes, et le cœur retombe encore pour un instant dans une immobilité absolue." *Traité de l'Auscultation Mediate*, tom. ii. p. 404 & 405.

the respective duration of the different states of the heart; and, according to his observation, one-half of the period of the action of the heart is occupied by the contraction of the ventricles, a fourth by that of the auricles, and a fourth by the state of repose. When the pulse was slow and sudden, he found the interval of repose proportionally longer and more sensible than in ordinary cases. In one instance, in which the pulse beat forty times in a minute, the period of repose seemed to him to equal the time occupied by the successive contractions of the ventricles and auricles.\*

The accuracy of this description of the sensations of sound and impulse communicated through the stethoscope by the action of the heart, will at once be recognised by all who have examined it in this way, and their analogy to those ascertained by the application of the hand to the chest, is very remarkable. On feeling the action of the heart with the hand, however, a distinct motion is perceived on the second of the consecutive succussions. It is generally felt as if at some distance from the surface, and situated towards the base of the heart; an impression which is described by Laennec to be occasionally communicated by the sound given through the stethoscope.† This second succussion does not usually communicate the feeling as if any thing struck the inner surface of the parietes of the thorax, especially if the individual examined be laid on the back, or placed in the erect position; but, in some instances, where the individual has been laid on the left side, or turned towards the face, I have observed a distinct impulsion against the parietes of the chest, towards the upper part of the præcordial region.

Although there can be no doubt that the sensations communicated by the actions of the heart are most accurately described by M. Laennec, yet I suspect that the explanation he has given of these sensations is incorrect. That the first of the two consecutive sounds or sensations of motion is connected with the contraction of the ventricle, is obvious, from its being synchronous with the pulse in the arteries, and also from the apex of the heart being felt, during it, to strike against the ribs. But that the second of the sensations is communicated by the contraction of the auricles, I conceive not to be proved by any thing which has been observed, and to be inconsistent with the nature of the action of the heart, as ascertained by physiologists from the inspection of that organ, in living animals.

In the circulation of the blood, as described by physiologists, this fluid is returned by the veins into the auricles of the heart: the auricles contract, so as to propel the blood into the ventricles, and immediately after this the ventricles contract, producing the stroke of the apex of the heart against the parietes of

\* *Traité de l'Auscultation Mediate*, tom. ii. p. 407.

† *Ibid.* p. 395.

the thorax and the pulse in the arteries. These motions constitute the systole of the heart. Immediately after the contraction of the ventricles, the heart falls into a state of repose for a time, constituting its diastole. After the diastole, the auricles again contract, and the same series of phenomena is observed to be repeated. Now the succession of the actions of the heart, as inferred by Laennec from the sensations communicated to the ear, does not correspond with this account given of them by physiologists. According to the description I have quoted from Laennec, the contraction of the ventricles is made the first of the two consecutive motions; the contraction of the auricles is supposed to follow it instantly without any interval; and the quiescence or diastole to take place on the termination of the contraction of the auricles. On the contrary, according to the observations of preceding physiologists, the contraction of the auricle is the first of the two consecutive contractions; that of the ventricle follows it immediately, and the diastole of the heart takes place between the termination of the contraction of the ventricles and the commencement of that of the auricles.

That this description of the successive motions of the heart, and of the order in which they occur, is not given on vague grounds, may be proved by the writings of various physiologists who have investigated this subject.

Harvey, in treating of the functions of the heart, has, from the extensive series of observations which he had made, described the order of its motions with all the clearness and precision by which his work on the circulation of the blood is so pre-eminently distinguished. He states, "there are as if two motions at the same time,—one of the auricles, the other of the ventricles themselves; nor are these performed altogether simultaneously, but the motion of the auricles occurs first, and that of the heart follows, and the motion is seen to begin with the auricles and to proceed to the ventricles."\* He also says, "It is to be noted, that what I call pulsations in the heart, and in the heart, are contractions; and you will distinctly see the auricles to be contracted first, and afterwards the heart itself."† And he afterwards states, "In the

first place, the auricle contracts itself, and, in that contraction, it throws the blood contained in it into the ventricle, which being filled, the heart is excited immediately, braces all its nerves, contracts the ventricles, and produces the pulse in which it impels into the arteries the blood sent from the auricle."\*

Senac, in treating of the motion of the heart, says, "If we appeal to the testimony of the eyes, we shall certainly see that the contractions of the venæ cavæ and the auricles; of the auricles and the ventricles; of the ventricles and great arteries, succeed each other exactly."† And in another place he states, "These contractions of the auricles are the first motions of the heart; when they contract, the ventricles are dilated, and contract themselves immediately after; that is to say, the dilatations and contractions are successive in these organs."‡

According to the description of Haller— "After the contraction of the auricles,—in warm blooded and vigorous animals, very quickly; in the cold blooded and languid, somewhat more slowly; and in warm blooded animals, sometimes with a short interval sufficiently well marked,—the contraction of the ventricles follows. It follows, I say, because I have seen this in very many experiments in the cat, the dog, the dormouse, and very easily in the chick in ovo; and because most dissectors of the preceding age habituated to the examination of living animals, bear testimony that the motion of the ventricles follows that of the auricles, and have confirmed by their experiments the alternate contraction of the auricles and ventricles."§ In

\* "Primum sese contrahit auricula et in illa contractione sanguinem contentum in ventriculo cordis conjicit, quo repleto cor sese erigit, continuo omnes nervos tendit, contrahit ventriculos, et pulsum facit, quo pulsu immisum ab auricula sanguinem continenter protrudit in arterias," &c. De Motu Cordis, Lugduni Batavorum, p. 37.

† "Si on en appelle au témoignage des yeux, on verra certainement que les contractions des veines caves et des oreillettes; des oreillettes et des ventricules; des ventricules et des grandes artères, se succèdent exactement," &c. Traité de la Structure du Cœur, &c. 2de ed. à Paris, 1783, tom. ii. p. 32.

‡ "Ces mouvemens des oreillettes sont les premiers mouvemens du cœur; quand elles se contractent, les ventricules se dilatent et se resserrent ensuite; c'est à dire que les dilatations et les contractions sont successive dans ces organes;" &c. Ibid. p. 35.

§ "Post auricularum constrictionem celerime in calido et sano animale, aliquanto lentius in frigido, et languente, et nonnunquam satis magno etiam in calidis, tempusculo interposito, sequitur ventriculorum contractio; sequitur inquam cum in plurimis experimentis ita viderim, in fele, cane, glire, facillime vero in pullo incubato; et cum plerique prioris ævi præsectores, in vivorum animalium inci-

\* "Duo sunt quasi eodem tempore motus, unus auricularum, alter ipsorum ventriculorum: nec enim simul omnino fiunt: sed præcedit motus auricularum, et subsequitur cordis,\* et motus ab auriculis incipere, et in ventriculos progredi visus est." De Motu Cordis, Lugduni Batavorum, 1737, p. 31.

† "Notandum est ubique omnes, quas voco, et in auriculis, et in corde pulsationes, contractiones esse: et plane primo contrahi auriculas videbis, et in consequentia, cor ipsum." Ibid. p. 32.

\* Harvey, and also Haller, seem frequently to use the word *cor* to signify the ventricles of the heart. It is here employed in this sense.

another place he says, "It may now be understood why the auricles first, and the ventricles afterwards, are contracted;"\* and, again, "A reason is required why, first, the right auricle, and, at the same time, the left are contracted, while, in the mean time, the ventricles rest relaxed; why, a little after, the auricles are relaxed, but both ventricles contract themselves; and then, in the third place, the ventricles repose relaxed, but the auricles again contract with a quick impulse?"† In describing the diastole of the heart, Haller states, "Then the same heart, after the contraction is completed, passes into that state which we denominate dilatation, remission, or the diastole," &c.‡

I shall only add the description given by M. Magendie. He has not entered at all minutely into the consideration of the order in which the motions of the heart succeed each other; but what he has said of the relation of the contraction of the auricles to that of the ventricles, is sufficiently precise. He states, "The instant when the auricle ceases to contract, is that at which the ventricle begins to act."§

So closely does the contraction of the ventricles follow that of the auricles, that it is difficult, in many cases, exactly to ascertain the termination of the one and the commencement of the other. In cold blooded animals, in the pulsation of the heart when first formed in the embryo of the chick, and in warm blooded animals, when the action of the heart exposed to view becomes languid, they can be observed distinct from one another; but in warm blooded animals, when the action of the heart is vigorous, it is difficult by the eye to distinguish any limit between the termination of the contraction of the auricle and the commencement of that of the ventricle. In these

animals, so rapidly do the contractions follow each other, that Harvey has described them from their commencement in the auricles to their termination in the ventricles, as often, apparently, forming one continuous motion. "These two motions," he says, "one of the auricles the other of the ventricles, are so performed in succession, guided as if by a harmony and rhythm, that both take place together; only one motion is apparent, especially in warm blooded animals, when the parts are agitated with a quick motion."\* "And if any one will attend carefully to the motion of the heart exposed in the living animal, he will see not only, as I have said, the heart to raise itself, and to make one motion continuous with that of the auricles, but also an undulation, and an obscure lateral inclination towards the duct of the right ventricle, and the organ to twist itself slightly, as it were, and thus to perform this action."† He compares the succession of the contractions to the motion of one wheel driven by another; to the ignition of the powder and discharge of a gun by the stroke of the flint impelled by the lock; and to the action of deglutition: in all which, he remarks, there are a number of motions, but following each other in such rapid succession, that their limits cannot be distinguished, and they appear as if they constituted one continued action.

Indeed, so difficult is it to observe the limit between the contractions of the auricles and of the ventricles, that they have been sometimes believed by diligent observers to be, to a certain extent, simultaneous or synchronous with each other. This was the opinion of Lancisi, who appears to have investigated the actions of the heart very carefully by experiment, and to have been well aware of the sources of fallacy in making observations on this subject. He observes, that, in order to see the motions of the heart as nearly as possible in their natural state, it is necessary to select large animals, such as oxen, horses, or, at least, large dogs healthy and vigorous; and to examine the heart immediately on opening them. In such animals, he says, "it may be established by experiment, that the contraction of the auricles is not performed alternately with that of the ventricles, but that the former anticipates the latter a

sionibus innutriti, motum ventriculorum auricularum constrictionem sequi testes se offerant alternasque auricularum et ventriculorum contractiones suo experimento confirmaverint." *Elementa Physiologiæ, Lausannæ, 1757, tom. i. p. 417.*

\* "Denique, incipit intelligi, quod continuo in quæstionem vocabimus: quare nempe priores auriculæ, et posteriores ab eo tempusculo ventriculi constringantur." *Ibid. p. 497.*

† "Poscitur ratio, quare prima dextra auricula, et una, eodemque tempusculo, etiam sinistra contrahatur, dum interim ventriculi laxi quiescunt; quare paulo post auriculæ laxatæ resolvantur, ambo vero ventriculi se constringant, et denuo tertio tempusculo ventriculi quidem remissi otientur, auriculæ vero denuo vivida vi se arcant." *Ibid. p. 498.*

‡ "Deinde, idem cor, post absolutam contractionem, in statum transit, quem dilatationem, remissionem aut diastolem dicimus." *Ibid. p. 386.*

§ "L'instant où l'oreillette cesse de resserrer est celui où le ventricule entre en contraction," &c. *Précis Élémentaire de Physiologie, ed. 2. tom. ii. p. 290.*

\* "Isti duo motus, auricularum unus, alter ventriculorum ita per consecutionem fiunt, servata quasi harmonia et rhythmo, et ambo simul fiant, unicus tantum motus appareat, præsertim in calidioribus animalibus, dum illa celeri agitantur motu." *De Motu Cordis, p. 37.*

† "Et si quis cordis motum diligenter in viva dissectione animadverterit videbit, non solum, quod dixi, cor sese erigere, et motum unum fieri cum auriculis continuum, sed inundationem quandam, et lateralem inclinationem obscuram, secundum ductum ventriculi dextri, et quasi sese leviter contorquere et hoc opus peragere," &c. *Ibid. p. 38.*

little.”\* He divides the time occupied by the contraction, and that occupied by the relaxation of the heart, into three parts—the beginning, the middle, and the end; and thus describes the relation of the motions of the auricles and of the ventricles to each other. “The whole operation of contraction and relaxation is so performed, that the beginning of the systole of the auricles is synchronous with the end of the diastole of the ventricles; the middle of the systole of the auricles is synchronous with the beginning of the systole of the ventricles; and, finally, the end of the systole of the auricles coincides with the middle of the systole of the ventricles: and, *vice versa*, it will be found, that the end of the contraction of the ventricles corresponds with the beginning of the diastole of the auricles; the beginning of the diastole of the ventricles with the middle of the diastole of the auricles; the middle of the diastole of the ventricles with the end of the diastole of the auricles; and that the end of the diastole of the ventricles, as we have said, coincides with the beginning of the systole of the auricles.”† He also states that in the chick in ovo, during the first days of incubation (from the third to the sixth day) the motions of the rudiments of the auricle of the heart can be observed to alternate with those of the rudiments of the ventricle; but that after these two parts join each other, and the heart acquires a conical form, these motions appear continuous rather than alternate; for the contractions of the whole *corculum* then begin in the auricles, and terminate in the ventricles; but we do not see them at all to follow each other.‡ This opinion of Lancisi’s has been discussed by Senac and Haller, who object to it, not exactly by denying his observations, but by adducing those made on cold blooded and smaller animals, in which they say the motions of the auricles and the ventricles can be seen distinct from each other; and combat it

by arguments, to my mind, not very conclusive, some of which, I may observe, were not unknown to Lancisi himself, who has endeavoured to refute them.\* Senac, however, allows that it may be suspected that the last efforts of the auricles are synchronous with the first efforts of the ventricles when they contract.†

Whether the opinion of Lancisi be true or not, his observations confirm, in a remarkable manner, the fact, that the contraction of the auricles immediately precedes the contraction of the ventricles, and that the succession of these motions is so instantaneous that it is difficult to distinguish them from each other. This fact appears to me established by the other authorities I have adduced, which I have selected in preference to many others to the same purport; because the descriptions contained in them are well known to have been founded on, or verified by, the personal observations of their authors. I may add, that, in my own limited opportunities of examining the contraction of the heart in living animals, the phenomena appeared to me to correspond very exactly with the accounts which I have quoted. The contraction of the auricle appeared the first motion of the heart, and was followed so immediately by that of the ventricles, that I have found it very difficult, if not impossible, to distinguish any interval between them.

The accuracy of these statements, with regard to the order in which the contractions of the auricles and ventricles succeed each other, may frequently, I believe, be confirmed in another manner, by observation on the human body, while all the parts are in their natural state. It is very generally allowed, that, on the contraction of the auricles, a portion of the blood they contain is driven back into the *venæ cavæ*, and produces in these vessels a pulsation extending often to a considerable distance from the heart. This is seen in experiments on the lower animals. In the human subject, this venous pulsation is frequently distinctly observable in the jugular and subclavian veins, in certain diseases of the heart or of the lungs, in which the flow of blood through these organs is impeded; but it can also be sometimes observed at the root of the neck in individuals in whom there is no reason to suspect organic diseases of these organs, particularly in thin persons, whose veins are large and lax. In such instances I have frequently examined the motions of the artery and vein very attentively, and have remarked, first the pulsation of the vein indicating the contraction of the auricle, immediately followed by the pulse in the artery indicating the contraction in the ventricle, and then an interval marking the diastole of the heart, again followed by the successive pulsations, first in

\* “Non dubitamus asserere, in natis, atque adultis animalibus experientiâ quoque constare, ipsas auricularum contractiones non quidem vicissim cum ventriculis fieri; sed illas horum systolem parumper antevertere.” De Motu Cordis et Aneurismatibus, Lugduni Batavorum, 1740, Prop. lix. p. 192.

† “Universum siquidem, et contractionis et remissionis negotium eâ lege transigitur, ut principium systoles auricularum synchronum sit, cum fine diastoles ventriculorum; medium verò systoles earundem auricularum sit synchronum cum principio systoles ventriculorum; denique, ut finis systoles auricularum metiatur medium contractionis ventriculorum; et vice versa comperiet, finem contractionis ventriculorum concurrere cum principio diastoles auricularum: principium autem diastoles ventriculorum cum medio diastoles auricularum; tandemque finem diastoles ventriculorum eodem recidere, scilicet cum principio, ut diximus, systoles auricularum. Ibid. p. 193.

‡ Ibid. Prop. xxv. p. 93.

\* De Motu Cordis et Aneurismatibus, Lugduni Batavorum, Prop. lx. p. 194.

† De la Structure du Cœur, &c. tom. ii. p. 33.

the vein, and then in the artery. In one instance, in which the venous pulse was very distinct at the roots of the right jugular and subclavian veins, and in which the two consecutive succussions of the heart could be felt by the hand placed on the thorax, I repeatedly examined the succession of the phenomena, and I observed, first the vein to be dilated; then the artery immediately to beat; and then could feel the second succussion of the heart: after which there was a short interval before the vein again pulsated. Here, I conceive, was distinctly shown, first, the contraction of the auricle marked by the pulse in the vein; secondly, that of the ventricle evinced by the beat of the artery, and accompanied by the first succussion in the thorax; and, thirdly, and last, the second succussion in the thorax, observed immediately to follow the first, distinct from the contraction of the auricle, and separated from it by the contraction of the ventricle on the one hand, and the interval of repose of the heart on the other.

From all these considerations, I am led to conclude that the second consecutive sensation communicated to the ear or hand when applied to the thorax, is not occasioned by the contraction of the auricles. Indeed Laennec seems to have been, in some degree, aware that his account of its relation to the other motions of the heart is not exactly reconcilable with the descriptions of the actions of this organ given by those who had examined them by inspection; for he remarks, "this repose, after the contraction of the auricles, does not appear to have been known to Haller, or, at least, it was not regarded by him as a natural state."\* Laennec conceived, indeed, that the motions of the heart could be examined more exactly by means of the stethoscope than by the opening and inspection of living animals; but he does not inform us by what process of observation or of reasoning he satisfied himself that the second of the consecutive sounds emitted during the action of the heart, is dependent on the contraction of the auricles. I suspect it must have been a somewhat hasty assumption, which was not unlikely to be made. In the action of the heart, two consecutive contractions had been seen; and, on applying the hand or ear to the thorax, two consecutive succussions or sounds were observed. As one of these sounds or succussions was found to correspond with one of the contractions, that, viz. of the ventricles, the inference would immediately, and not unnaturally, suggest itself, that the second depended on the other contraction, viz. that of the auricles; an inference which I have endeavoured to prove to be erroneous.

I am disposed, therefore, to conclude, that, in applying the ear or the hand to the thorax, the contraction of the auricles of the heart either is not evident to the senses, or is perceived continuous with the contraction of the ventricles, and that the two contractions com-

municate only one sensation—the first of the two consecutive sounds and succussions. The latter of these suppositions seems to me the more probable, and to be supported by the length of the period which the first succussion occupies.

It remains to be determined, on what depends the second of the sounds and succussions which takes place immediately after the contraction of the ventricles. Can it be accounted for by the impulse occasioned by the falling back on the pericardium of the relaxed heart in its diastole, after it has been elevated or moved from its place in the systole? It has long been a common opinion that the heart is not inactive during its diastole, but has a power of dilating its cavities. Some authors have attributed this power to a *vital* action of the muscular fibres of the heart, an hypothesis that does not appear tenable, or to be reconcilable with what is known respecting the structure of this organ, and the living properties of muscular fibre. Others have attributed it to a *mechanical* power of the heart—its elasticity,—by which the cavities of the ventricles, when contracted or compressed, have a tendency, they say, to expand themselves again in the same manner as a bag of caoutchouc or strong leather expands after having been compressed; or as an artery removed from the dead body recovers and retains its cylindrical form and cavity, after its parietes have been pressed together. On this supposition has been founded the hypothesis that the motion of the blood depends in part on the ventricles of the heart attracting this fluid by suction into their cavities, from the corresponding auricles. This hypothesis does not seem improbable; and such an action, if it really occurs, may perhaps contribute, in addition to the falling back of the heart, to produce the sensation communicated to the ear or the hand, which I am endeavouring to explain.

This explanation of the second succussion of the heart seems to me to correspond very exactly with several of the phenomena observed during it; for instance, with the apparent retrocession from the ear of the sensation it communicates, as described by Laennec; with the motion appearing deep in the chest, and proceeding from near the base of the heart; and with the impulse felt during it when the body is laid on the left side or on the face; and if the hypothesis of the dilatation of the heart by its elasticity, and its consequent power of suction, be true, this may be conceived to produce a sound very analogous to those to which M. Laennec has compared that emitted by the heart,—the raising of a valve or the lapping of a dog.

In offering any conjecture with regard to the actions or motions of the heart, I am well aware of the numerous sources of fallacy to which we are exposed in attempting to judge of phenomena concealed from our view in their natural state, and in the production and modification of which a great variety of causes may concur. The very object of this essay could not fail to impress my mind with the ex-

\* De l'Auscultation Mediate, tom. ii. p. 406.

istence of such difficulties, and to render me very diffident with regard to the value or correctness of any opinion I might form on the subject to which it relates. If the views I have taken be correct, it is obvious that an error pervades the otherwise valuable descriptions given by Laennec of the actions and diseases of the heart; for in all of them the second sound heard in the motion of the heart is considered and denominated the contraction of the auricles. If, as I suppose, this eminent pathologist has been mistaken in this opinion, it would afford the strongest proof of the difficulty of avoiding the causes of deception, and of the impropriety, in describing the phenomena, or in judging of the nature of diseases, of proceeding on a doubtful physiological hypothesis.

Since the foregoing Essay was read to the Society, I have perused a Thesis published by Dr. Barry, "*Sur le passage du sang à travers le Cœur*," in which, among the pathological propositions deduced from the experiments and reasoning contained in it, there are the two following:—"2e, Les deux sons que le cœur présente à l'auscultation sont produits par la dilatation des ses cavités, et non par leur contraction. 3e, Le premier son, qui ne correspond pas toujours à la pulsation artérielle, est le resultat de l'expansion des appendices, et le second de celle des ventricules." The experiments related in this Thesis, as well as those contained in Dr. Barry's previous publications, appear to me very ingenious and original; and to illustrate interesting facts respecting the state of the heart and of the blood-vessels connected with it, during the circulation, and the effects of respiration on the motion of the blood in the large venous trunks. I cannot, however, fully comprehend the author's statements and reasonings with regard to the effects produced on the motions of the heart, and on the passage of the blood through this organ by the removal of the atmospheric pressure. If I understand his meaning rightly, some of the conclusions and inferences do not appear to me to be warranted by the experiments related, or by what is known of the mechanism or functions of the organs of respiration and circulation. It is not my intention to enter into a detailed discussion of this subject; but I may remark, that even if it were allowed that the act of inspiration may have some effect in attracting the blood to the right auricle of the heart, which is directly connected with the large veins *external* to the thorax; yet I conceive a similar effect cannot be produced on the left side of the heart, because the whole of the veins connected with the left auricle are *within* the thorax, and consequently exposed equally with the heart itself to the influence of any power which has a tendency to remove the pressure of the atmosphere from the viscera contained in the cavity; so that such a power cannot have any influence in promoting the flow of blood from the pulmonary veins into the left auricle of the heart. There are facts, also, which show that there are other powers quite independent of the re-

moval of the pressure of the atmosphere from the surface of the heart, either by respiration or by its own contraction, which are sufficient to produce the motion of the blood through the heart and blood-vessels. This seems to be proved by the circulation of the blood in the fœtus in utero; and by the cases in which the surface of the heart has been exposed to the atmosphere, and its action, and the circulation of the blood have continued. We have examples of this kind in infants, in which the heart has been protruded to the outside of the thorax, through an aperture in that cavity, and in cases in which the cavities of the chest and pericardium have been opened by injury or disease. It appears also to be proved by experiments on animals, in which, after the muscles of respiration have been paralysed by poisons, or the cavities of the thorax have been laid open, the action of the heart and the circulation have been continued by maintaining artificial respiration; as well as by cases of asphyxia in the human subject, in which, the motion of the heart and of the blood has been restored by the inflation of the lungs through the trachea: in these experiments and cases it is obvious that, instead of the pressure of the atmosphere being removed from the heart or lungs, an additional pressure is made on all the contents of the thorax, during its expansion. With regard to the explanation offered by Dr. Barry of the sounds emitted during the action of the heart, I cannot discover that it is justified by any of the facts stated in the experiments, or by the reasoning founded on them. The cause assigned for the second of the consecutive sounds appears to me liable to the same objection that I have stated to that supposed by Laennec; for the dilatation of the ventricles is synchronous with the contraction of the auricles; and I have endeavoured to show that this does not coincide with the second sound or succussion produced by the motion of the heart.

From the London Medical Gazette.

TRANSACTIONS OF THE ASSOCIATION OF FELLOWS AND LICENTIATES OF THE KING AND QUEEN'S COLLEGE OF PHYSICIANS IN IRELAND. Vol. V. Dublin. 1828.

Although the Irish association dates its origin only from the year 1816, its members have succeeded in establishing for themselves a highly respectable character with the profession at large; every successive volume of their Transactions has been received with decided approbation, and the present one, in our judgment, will not be found to depreciate the merits of its predecessors.

*Mr. Ryall on Nitrate of Silver in Affections of the Eye.*

The first paper is a communication from Mr. Ryall, of the National Eye Institution, on the use of nitrate of silver in certain affections of

the eye. Nitrate of silver has long been a popular remedy in several of the morbid conditions of this delicate organ; but its success having led to its almost indiscriminate application, it has been, but too frequently, productive of injurious effects in the hands of the inexperienced. Mr. Ryall, whose experience as an oculist entitles him to considerable credit, points out those conditions of the organ in which this remedy is peculiarly applicable, noticing by the way, such auxiliary means as tend to promote its efficacy. In *ulcer of the cornea*, one of the most frequent and serious terminations of ophthalmia, the nitrate of silver is a remedy of great value. When the ulceration is superficial, and the cornea is seen occupied by innumerable minute depressions, or breaches of continuity, a solution of two grains of the caustic in one ounce of distilled water will generally answer, conveying it to the part, if necessary, (as in the cases of children) in the form of injection. But for ulcers deeply penetrating the laminae of the cornea, in addition to the collyrium, it will be requisite to apply immediately to the affected part a fine camel-hair pencil charged with as strong a solution of the nitrate as can be made, or the caustic itself, in substance, pointed like a crayon—dropping into the eye, after each application, a little oil of sweet almonds. The internal use of mercury is, in general, inadmissible in cases of this description: patients labouring under ulceration of the cornea are for the most part of delicate and debilitated constitutions, and more likely to be benefited by nutritious diet, baths, mild tonics, healthy situation, and fresh air, than by mercurial alteratives. Cases, no doubt, do occur, in which the stimulant application is contra-indicated; for instance, under circumstances of redundancy of lymph or of interlaminary effusion; here the vascular action must be diminished by local blood-letting, purgatives, and emollient fomentations; after which a pill of calomel, antimony, and opium, should be administered twice a day until the mouth be slightly affected or the symptoms recede. The practice of dropping venous tincture of opium into the eye, in cases of ulceration of the cornea, tends greatly to aggravate the irritation, without in any degree contributing to produce healthy action of the parts. In *pustule*, or aphthous affection of the conjunctiva, generally the concomitant of a scrofulous taint, the tendency of the pustules to inflammation will be restrained, and in most cases speedily removed, by a few light touches of the stronger solution. In *sloughing of the cornea*, Mr. Ryall strongly approves of the exhibition of extract of cinchona, as recommended by Saunders: even in several instances where death of the exterior laminae of the cornea, or of a segment of its entire thickness, had already taken place, and when the eye had presented the appearance of a disorganized mass, Mr. R. has succeeded, by a judicious use of the extract, in preserving both the shape and function of the organ, to an extent which he could hardly have anti-

cipated. The sulphate of quinine too is a valuable auxiliary while using the nitrate of silver in cases of slough.

When ulceration has proceeded so far as to cause a collapse of the cornea and *protrusion of the iris*, the nitrate of silver is our chief, perhaps our only resource. By a few applications of the pencil-pointed caustic the happiest effects are produced; healthy action and new growth of parts are quickly obtained, the cornea resumes its convexity, the protruded iris is retracted, and unless the breach has taken place in the centre of the cornea, vision is completely restored. Material benefit is derived from the collateral aid of extract of belladonna, reduced to a cream-like consistence, and applied to the brow and exterior eyelids twice or thrice a-day. *Nebulous cornea* is accompanied by a turgid and tortuous state of the veins of the sclerotic, as well as by a profuse morbid secretion from the tarsal glands. The application of leeches to the inner surface of the lower palpebra, the exhibition of purgatives, and precaution against strong liquors and cold, are the first objects to be attended to. After this, a circle, or as large a portion of one as will embrace the opaque part of the cornea, is to be described, with the pencil-pointed caustic, on the sclerotic, at about two lines distance from its junction with the cornea. Ulceration must be kept up for some time by these means, and the solution of nitrate of silver (from two to six grains to the ounce of water) is to be frequently injected into the eye; in addition to which we should apply the ointment of the red oxide of mercury every night to the tarsi. In some of the worst cases, solution of the acetate of lead is employed with great advantage, though not with such invariable success as the solution of lunar caustic. Tonics, such as dilute sulphuric acid, and sulphate of quinine, are productive of singular benefit in this complaint; and Mr. Ryall relates a case in which the accidental injection of the dilute sulphuric acid by mistake for the caustic solution, was attended with the most unexpectedly beneficial result. In *albugo*, absorption, in the inflammatory stage, is more judiciously attempted by topical blood-letting, and alterative doses of calomel and opium, than by local stimulants; however, when the inflammation has subsided, a strong solution of the caustic will generally be found successful. The *staphyloma* of young children, which succeeds to small-pox and purulent ophthalmia, has been checked, and ultimately removed, by keeping up for some time artificial ulcers by means of the nitrate of silver. *Granular palpebrae*, another consequence of long continued inflammation, produce by friction against the cornea not only great irritation, but eventual opacity; and it is the first business of the surgeon when called to a case of protracted ophthalmia, to evert the palpebrae, particularly the superior, and he will generally at once discover the cause of irritation. It is not always necessary to have recourse to excision in these cases; the caustic simply will effect a

cure, observing after each application to wipe the parts with a sponge, and to drop into the eye some oil of almonds. Sulphate of copper will contribute very essentially to prevent the recurrence of the granulations, and to restore the healthy action of the parts. *Ectropium* may in most instances be removed by producing upon it repeated eschars with the nitrate of silver; where the surface is callous, it will be necessary previously to remove, with a knife, or curved scissors, the exuberant conjunctiva; and in general, whenever excision is employed, as in staphyloma, encanthis, pterygium, and other fleshy excrescences of the sclerotic and cornea, the aid of an escharotic may be found necessary, as well for their ultimate destruction, as to restrain the healthy granulations of the wound. The nitrate of silver is the safest escharotic that can be employed: in many instances this caustic alone, without any previous operation, will effect their removal.

*Dr. Jacob on Internal Inflammation of the Eye following Fever.*

The occurrence of a local inflammation of this kind is not only of importance to ophthalmic surgery, but to pathology generally.

The peculiar inflammation which forms the subject of this paper, should not be termed iritis, as it has been by Hewson, in his slight notice of the complaint in his work on Venereal Ophthalmic—the iris is not the part which is primarily or exclusively attacked; on the contrary, all the internal parts of the eye participate in the inflammation, particularly the retina, as is proved by the most obvious symptoms. All the cases (about 70 or 80) met with by Dr. Jacob within the last year, had previously suffered from fever—some of them, certainly, as far back as seven or eight months—but there were only one or two cases in which the previous existence of fever was doubtful. It occurs more frequently in young persons than in old—in few after the age of five-and-twenty; more frequently among the poor than the rich—among females than males. In the majority of instances, it made its appearance in six weeks, or two months, after recovery from fever; and Dr. J. has not met with a single case in which both eyes were affected. A remarkable feature in the progress of this complaint, is the formation of a pink zone, encircling the margin of the cornea. This arises from the enlargement of the capillary vessels of the sclerotic at this part, admitting red blood; which vessels, in a state of health, we conclude, carry transparent blood only. The larger distinct vessels, which appear at a more advanced period, converging to the circumference of the cornea, and obscuring the white of the eye, are the branches leading to those capillaries; and as they do not ramify so extensively, or form the same reticulated arrangement as those of the conjunctiva, they afford a good means of distinguishing inflammation of the globe, or its parts, from inflammation of the conjunctiva. Another remarkable symptom is the alteration

which takes place in the colour of the iris—the brilliancy of its tints are totally lost; it never, however, acquires the decidedly yellowish green observed in syphilitic iritis; neither have those globules of lymph been observed which characterize the latter form of inflammation; purulent matter, however, is occasionally secreted in the anterior chamber of the aqueous humour, constituting hypopion, or unguis. The following history of one well-marked case affords a good summary of the symptoms:—"The patient states that he recovered from fever two months ago, and returned to work at his trade as usual. About three weeks ago his sight became dim, his work confused by motes or flies floating before him, and a few days ago the eye became red and sore. He complains of pain in the eyeball, extending to the temple, and suffers from exposure to sunshine, fire, or candle. He cannot read small print, nor tell the hour by the watch. His eye feels hot, and there is a considerable discharge of tears. The sclerotic vascular, the pink zone, the cornea slightly clouded; the margin forming a whitish circle resembling the *arcus senilis*. The iris altered in colour; the pupil irregular, acting sluggishly, or nearly immoveable. The crystalline lens appears clouded, of an amber tint, and opaline appearance; and vision is permanently impaired, or totally lost, with dilated pupil, and other symptoms of perfect amaurosis." The treatment of this inflammation of the eye is not attended with much difficulty. Antiphlogistics, with opiate stupes, belladonna, and blue pill, comprise the *materiel* of the practice. Belladonna freely daubed over the lids and brow, and kept moist by a light fold of old linen wetted every ten minutes, will not only prevent closure of the pupil, and adhesion of its margin to the capsule of the lens, but will give decided relief when there is deep-seated pain extending from the eye-balls to the temple. Beer's ointment, composed of equal parts of belladonna and blue ointment—a drachm of it rubbed in upon the temple every night—is an excellent application. But the main stress of the curative treatment must be laid upon the use of mercury. "In my own practice," says Dr. Jacob, "I have found the relief from the use of mercury so certain and decisive, that I have trusted to it almost exclusively, with the assistance of the belladonna. I have generally found that two grains of calomel, with a quarter of a grain of opium, three times a day, answered every purpose; and in the majority of cases, I produced the necessary mercurial action, as marked by tenderness of the gums, in eight or ten days, by the use of three, four, or five grains of blue pill alone, three times a day; and if the pain should be severe, combining hyoscyamus, or belladonna, with the dose taken at bed-time." To the exhibition of bark, or sulphate of quinine, in this complaint, Dr. Jacob is decidedly opposed.

*On Peripneumonia, by Dr. Cumming.*

The next communication which we have to notice is a paper on the peripneumonia of

children, by Dr. Cuming. This complaint does not appear to have hitherto attracted all the attention which its importance demands; the only authors who have at all touched upon its investigation are, Hastings, in his treatise on inflammation of the mucous membrane of the lungs; and Cheyne, in his pathology of the mucous membrane of the larynx and trachea: the latter styles the disease in question the epidemic peripneumony of children; but both these able physicians would seem to have taken a superficial or partial view of the pathology of the complaint. They report the morbid appearances to be chiefly confined to the mucous membrane of the bronchia, whereas, in every case witnessed by Dr. Cuming, the parenchymatous as well as the mucous tissues, particularly the former, have been the seat of inflammatory action. Under these circumstances, however, we think it may reasonably be questioned whether Dr. C. be not confounding two very distinct diseases, namely, bronchitis and pneumonia, under one title. But we shall not delay our analysis in discussing the propriety of mere names. This peripneumonia occurs in children of all ages, from a few days old up to eight or nine years. The most frequent subjects of its attack, however, are children between nine months and two years. In general a trifling cough, with other slight symptoms of catarrh, precedes by a day or two the complete formation of the disease. When fully formed, the symptoms are a hurried, laborious, wheezing respiration; a frequent, short, and dry cough; and a greater or less degree of fever, with extreme restlessness, moaning, and aversion to be moved. In bad cases, the wheezing is converted into rattling; and in the course of eight or ten days from the commencement of the attack, death generally takes place by suffocation. The frequency of the respiration is stated to have amounted, in some instances, to upwards of a hundred in a minute. No disease of childhood seems to be attended with a higher degree of fever than peripneumonia. Intense heat of skin, dryness of the lips and nostrils, loaded white tongue, excessive thirst, and a pulse of from 168 to 200, have been frequently observed, in addition to restlessness and jactitation, with moaning, and starting out of sleep. Though the stomach is occasionally irritable at the beginning of the complaint, it is remarkable with what difficulty vomiting is excited towards the close. So great is the insensibility of the stomach a short time before the fatal termination, that the strongest emetics, and in the largest quantities, have been repeatedly administered without effect. The same insensibility seems to extend itself to other parts, particularly to the skin, where we often fail in exciting inflammation by the application of a blister. With regard to the duration of the disease, when an unfavourable event takes place, it is generally upon the eighth or tenth day; when the case terminates in recovery, it is seldom protracted beyond a week. Peripneumonia is more prevalent in winter and spring, than at any other season. The morbid appearance

most frequently met with is an increase in the solidity of the lung, varying in degree, from that of the slightest sanguineous congestion up to complete hepatization. This increase of solidity or induration is not equally great in every part of the lung. The inferior and posterior part is in general the region affected; and it often happens that while the upper portion is in a healthy state, or merely a little more congested than natural, the inferior portion is completely hepatized. It would appear as if the morbid process, commencing in the lower part of the lung, had completed its course there, before the superior portion had advanced beyond the stage of sanguineous congestion. But although this indurated condition of the lung be the principal morbid appearance, constantly observed, yet along with this, in almost every case, is combined more or less of inflammation of the mucous membrane of the bronchia; and there is this remarkable peculiarity in the pathology of the complaint, that the more intense the latter inflammation is, and the more considerable the consequent effusion into the bronchia, the less in general is the induration of the lung, and *vice versa*. With respect to treatment,—in order to arrest the inflammatory process before it has gone the length of effusion, the most powerful remedy is blood-letting. “It is as great a mistake,” says Dr. Cuming, “to suppose that children do not bear bleeding well, as that they are not liable to disease which require it. Where no peculiar delicacy of constitution was manifest, I have found children to bear blood-letting as well as adults; and I can speak from pretty extensive experience when I say, that there is no disease which more imperatively demands the employment of the lancet than the pulmonary affection in question. And the earlier it is employed, the less occasion in general will there be for its repetition. The quantity of blood to be withdrawn must depend upon the age and constitution of the patient, the violence of the disease, and the impression that may have been made by the previous treatment of the symptoms. From two to three ounces in an infant between six and twenty months, will be sufficient. In an infant under six months, though general blood-letting be required, the application of three or four leeches to the back of the hand or foot will for the most part answer the purpose, where a vein, which is frequently the case, cannot be found. Dr. C. is of opinion that leeches applied to the extremities are nearly as efficacious in removing local inflammation in infants, as when applied in the vicinity of the part affected. They seem to produce the effect of a general blood-letting, and syncope not unfrequently takes place. Sometimes a state of nervous agitation and general commotion is induced, which if not speedily removed, may terminate in death. The best remedies in cases of this kind are the horizontal position, cool air, and a drop or two of the tincture of opium. It is in the first stage of the disease, or that which elapses previous to the occurrence of much effusion or collapse, that benefit

is to be principally expected from purgatives, and a combination of calomel and julap is to be preferred. To a child between six and twenty months we may give a grain of calomel, with four or five of julap, and one of ginger, for a dose; and the action of the bowels may be advantageously kept up by administering a grain of ipecacuan, with one of calomel every second, third, or fourth hour, according to circumstances. Antimonial emetics too, are employed with great benefit, and the application of blisters may be practised with the best effects. Much caution, however, is necessary in the management of them: we should not allow a blister to remain on for more than three or four hours. When it is applied for a longer period, excessive general irritation is apt to be induced; and such is the delicacy of the infant skin, that the blistered parts not unfrequently become gangrenous. In cases of this kind, the death of the child has been sometimes the consequence. Though vesications may not have formed at the time the blister is removed, they generally take place after the application of the dressing. In infants of an irritable habit it will sometimes be advisable to dilute the blistering plaster with an equal quantity of the emplastrum ceræ. When in an advanced stage of the complaint the debility is considerable, and suffocation appears to be impending, our principal reliance is to be placed on the exhibition of stimulants. Carbonate of ammonia, in three or four grain doses, every third or fourth hour, is recommended; and the best vehicle in which it can be administered is the decoction of senéka. Dr. Cuming has given a judicious and satisfactory appendix of ten illustrative cases.

*On open Foramen Ovale, by Dr. Crampton.*

Although we cannot adopt the opinion of Portal, who thought he had reason to conclude, that cases in which the foramen ovale is open, are quite as numerous, and occur as frequently, as those in which it is shut, yet it cannot be doubted that instances of this deviation of nature are far from being rare.

Dr. Crampton gives an account of two or three which occurred in his own practice. The first of these was James Spellman, aged 18, admitted into the Hardwicke Hospital as a fever patient, on the 6th of February, 1827. The thoracic viscera appeared to suffer from inflammation; he had a severe cough, complained of pain in different parts of his chest, chiefly at the left side, expectorated blood, and was distressed in his breathing. These symptoms, however, gave way to the usual remedies, and about the 14th he was pronounced convalescent. But his interval of remission was short. He presently after, from premature exposure to cold, exhibited every symptom of acute rheumatism, with which all his joints were occupied, being excessively tumid, painful, and accompanied with a high degree of fever. In a few days the rheumatism became metastatic, the pectoral organs being again attacked, more especially the heart, in the region of which he appeared to suffer

extremely. On the 27th Feb. the symptoms were, pain in the region of the heart, severe cough, with a croupy sound, much mucopurulent expectoration, and great distress in his breathing. At this time he stated that he had for years been occasionally subject to pain in the left side, but particularly after running, or any active exertion; his pulse extremely rapid, sometimes irregular, countenance pale, feet œdematous. The stethoscope indicated acute bronchitis, with hypertrophy, and disordered action in the heart, the motions of that organ being tumultuous and irregular. On the 6th of March he expired. The indications of the stethoscope were borne out by the dissection; but the circumstance which attracted most attention in examining the heart was the open condition of the foramen ovale. The septum between the auricles exhibited an oval depression, or attenuated space, of about one-third of an inch in diameter, guarded only by a thin membrane; but at one side it was evidently pervious and open, with a rounded and thickened edge. This membrane acted like a curtain or valve; when viewed or pressed from the left auricle, it was closed, the curtain or membrane pressing close, and overlapping the opening; viewed from the right auricular cavity, or touched with a probe, it opened, and allowed a free passage, fully as large as a goose quill, compressed so as to exhibit an elongated or oval section. The blow-pipe exhibited the same difference of a closed or open space, as it was used from the left or right side of the auricular septum. The commencement of the aorta appeared unusually narrow—that of the pulmonary artery greatly enlarged. The auricles were in a state of considerable dilatation; and the ventricles quite filled with a dense white coagulum, firmly attached to their parietes.

It is easy to understand what tumult and distress must have been felt in the heart and respiratory organs. In the history of his state of health antecedent to his feverish and inflammatory attack, he dwelt on the pain in his left side, and on the distress he experienced in his breathing on making any active exertion. An additional source of trouble must have arisen from the contracted state of the commencement of the aorta. But still it appears that such a condition of the heart is not incompatible with the continuance of life for eighteen years; and that were it not for over-exertion, mental emotion, or some sudden attack of disease, it might have continued for a longer period. From this and another case, Dr. Crampton ventures to suggest that this uncommon disposition of the heart may tend to a useful purpose, in preventing a still greater degree of aberration from the healthy state. The additional passage *to the left heart*, by diminishing congestion in the lungs of a person predisposed to consumption, might have averted the usual catastrophe; or by affording an easier return to the venous blood from the head, might have prevented an apoplectic seizure, or a paralytic attack, in a patient otherwise liable to such an occurrence. In many instances,

nature thus, by unusual and anomalous arrangements, provides against still greater evils; and what at first sight might appear to be an imperfection, is in fact a most ingenious contrivance to remedy an error of more importance. The foramen ovale was found unclosed in another patient, aged 34, who died in the Witworth Hospital, in April, 1827. The symptoms on admission were, excessive dyspnoea, or rather orthopnoea, violent palpitations, tremulous motion of the jugulars, face pale and dingy, lips and nails of a dark leaden colour, ankles œdematous. In the heart, as in Spellman's case, the valve was found to overlap and protect the aperture, so that no blood was likely to pass, unless the contracting power of one side of the heart prevailed over that of the other. There was no appearance of any such preponderance under ordinary circumstances, for the heart was sound, and in due proportion in all its parts; but when any impediment occurred on either side, the current might take the passage when the overlapping curtain was so easily removed. The distressing palpitations and the colour of his lips and nails, might lead one to adopt this opinion. The curtain could only be pushed aside from right to left; and the same conjecture as in Spellman's case, already advanced, might here be hazarded, that life was perhaps prolonged by this anomalous mechanism. The progress of tubercles towards suppuration and destruction of the lungs, might have been slower than if the whole force of the circulation, from the right side of the heart, had been constantly exerted on the lungs, the blood occasionally taking the devious path already described. In this way the unsound lungs (for we should have mentioned they were found tuberculated, and hepatized, with a large cavernous excavation in the left lung) were enabled to perform their functions for 34 years; nature thus, as observed before, compensating in some degree for the imperfection, by an unusual and extraordinary arrangement.

*Removal of a large Tumour from the Neck, by Dr. Daly.*

Bold operations of this kind have not been unfrequent of late; but we think that the circumstances of the present case may be deemed worthy of particular notice. The tumour was of the steatomatous character, and suffered to run on for 13 years. When the patient, who was a stout healthy man, 27 years of age, was seen by Dr. Daly, the appearance and situation of the tumour were as follow:—It occupied nearly the whole of the right side of the face and neck, extending from the zygomatic arch, under which it seemed imbedded, to within two inches of the clavicle, in which direction it measured in its anterior circuit eleven inches and a half. The ear was pushed up towards the temple, so that its lobe was expanded over the tumour; and the sides of the meatus externus so closely pressed together, that hearing was entirely obstructed on that side. Posteriorly it extended to the distance of five inches and a half from the lobe of the

ear, and anteriorly over the upper and lower maxillæ, and to within an inch of the angle of the mouth; in this direction it measured from behind, round the most projecting part of its surface, to the sulcus in the skin, parallel to the trachea, twelve inches and a half. The surface was studded with irregular knobs; several ran in various directions over the tumour, but the external jugular was found below and behind it of its natural size. The carotid was felt beating deeply under it near the clavicle, but near the angle of the jaw it could not be ascertained whether the vessel ran through it or under it. We pass over the details of the operation, which was of a truly arduous and appalling nature. Suffice it to say, that it was completely removed, not a particle being left behind. The whole tumour weighed two pounds fourteen ounces. The blood lost during the operation was chiefly venous, and did not exceed twelve ounces. On looking into the cavity which remained, its depth appeared formidable indeed, as the clenched hand might easily be buried behind the jaw; and nothing but the pharyngeal muscles and mucous membrane prevented the fingers from passing down the œsophagus. The epiglottis was easily and distinctly felt, but the tonsil of that side was not to be found, having been either pushed deeply backwards and upwards towards the base of the skull, or what is more likely, absorbed. Under the most strict antiphlogistic regimen, the wound gradually improved, and was completely healed up by the termination of the fifth week. The man has been since constantly engaged in his usual occupations. His hearing was restored immediately after the operation, as soon as the ear had recovered its natural position: however, as several branches of the seventh pair of nerves were unavoidably divided during the operation, he may probably always labour under a partial paralysis of the right cheek. It may not be improper to remark, that the patient traced the origin of the tumour to a severe pulling of the ear, and pressure of the thumb behind the angle of the jaw; immediately after which it began to form; but it was principally during the year which preceded the operation that the tumour acquired its vast bulk.

From the Lancet.

ON THE DISEASES WHICH ARE OBSERVED IN EGYPT, AND ON THE ACTUAL STATE OF MEDICINE IN ARABIA. By DR. EHRENBURG.\*

The physicians whom Drs. Ehrenberg and Hemprich met with in their travels to Cairo, Suez, Damietta, Syria, and Arabia, only presented a glimmering of the knowledge which existed formerly in those parts; they appeared conscious of their ignorance in the presence

\* Hecker's Litter. Annal. der gesammten Heilkunde; Janv. 1827, p. 1.

of the European travellers, and had no idea of the nature or treatment of diseases. The Canon of Avicenna is still their oracle. The most modern work on their art is that of David. In order to become a physician, it is necessary to make several copies of these works, and to learn them by heart. Bleeding, scarifications, application of cupping glasses, and circumcision, are the common surgical operations performed. Monks sometimes undertake amputations and castration. Dislocations are often left to themselves after unsuccessful attempts to effect their reduction; fractures are seldom united. Penetrating wounds from fire-arms are at first washed and cleaned with wine; then twenty or thirty threads, smeared with some kind of balsam, are introduced in the same manner as a seton; each day the threads are changed, and the number is gradually diminished by the subtraction of one thread at each dressing; the removal of the thread promotes the exit of pus, foreign bodies, and splinters from the wound. The patient is generally cured at the end of twelve or fifteen days, and fistulous openings seldom remain.

In inflammatory diseases the medicines employed internally, are cream of tartar, nitre, grains of succory, leaves of rosemary, tamarinds; and the means used externally are cupping glasses, and scarifications with a razor; in ophthalmia, antimony is given during the inflammatory stage, and this treatment appears to be one of the causes of the great number of blind in this country. In bilious diseases, which the Arab physicians only recognise by the yellow tint of the body, tongue, or eyes, scammony, fruits of several kinds of *terminalia* and *phyllanthus emblica* are given. Anchusa is, with them, a specific. In dropsy, scarifications and cautery are employed; drastic purges are also given, with diuretics. In diseases affecting the mucous membranes, such as catarrh, &c., small doses of ipecacuanha, sage, tea, grains of paradise, and cardamom, licovite, and sebastin (fruits of *cordia myxa*) are used. In dysentery, grains of rhus coriaria are employed. Assafetida, and the root of the *cyperus fuscus*, with a small fruit of undetermined origin, called *mahleb*, *ischaemum schænanthus*, mixed with flour, are considered the best medicines for abdominal diseases.

Arabian physicians recognise, moreover,

1st—Diseases of the skin, which they treat with purgatives, emollient applications, and baths. Frictions of butter and sulphur, or verdigris, are used in the itch; pressed grapes of Corinth are employed in elephantiasis; no cure is known for leprosy; the properties of myrrh, in malignant ulcers, are not known, although it is indigenous in Arabia. The warm baths of Birket Faraun, on the Red Sea, are said to have cured several obstinate diseases.

2d—Worm diseases, in which senna and other purgatives are made use of, but without any great success.

3d—Syphilitic diseases, which are treated

with sarsaparilla, and a kind of wood which comes from India, and called Chinese wood.

In diseases of the eyes, washes of dilute vinegar, zinc, vitriol, and saturnine water, and very frequently crude antimony, with the pulverised grain of the *cassia absus*, are used. When the inflammation is very violent, scarifications are resorted to. The physicians are generally druggists; they are not respected, and their art is by no means lucrative. The diseases little known to Drs. Ehrenberg and Hemprich, which they observed in Egypt, were—1st, an eruption called *hydroa æstivum Ægyptiacum*. Small miliary pustules, filled with serum, and appearing on the skin; these dry, and are followed by a fresh eruption, but not attended with any constitutional derangement, accompanied with a troublesome itching. This eruption manifests itself every year, at the overflowing of the Nile, among the Franks, Turks, and Arabs, whilst the Nubians, Dongolians, and Negroes are free from it. In children and persons who have a tender epidermis, the pustules run into each other, and are filled with pus, but they leave no marks after them. After the inundations this disease disappears.

2d—Erysipelas of the scrotum in mariners. (*Intertrigo scrotalis navium*.) The skin of the scrotum is greatly inflamed, painful, and sometimes relaxed; its whole surface secretes a puriform matter; the disease disappears on landing, but instantly returns when the patient is on ship-board. The Arabs suffer from it less than the Franks.

3d—Acute diseases of the knee joint. According to some accounts, this rheumatic affection sometimes prevails epidemically in Egypt and Arabia; in Alexandria it is supposed to be contagious; it soon manifests itself by pain in the knee, which increases on pressure; gastric fever generally comes on. The disease usually lasts but nine days, and a relapse seldom occurs.—Dr. Hemprich was affected with it in a slight degree.

4th—Rheumatic ophthalmia of Egypt is very frequent among the inhabitants. The affection of the eyes first shows itself in a slight redness of the conjunctiva, with a sense of weight on the eye; on the next day the pressure is less, but the eye cannot support the light, and the conjunctiva is completely tinged with red, and inflammatory pulse; there is very often sneezing and want of appetite; one eye is generally worse than another; sometimes one alone is affected.

On the third day, if the disease be not checked, the conjunctiva is of a varicose appearance; the edges of the lids are rather cedematous, and glued together by a viscid mucus. A flow of tears escapes when this mucus is removed by water or warm milk; the lids remain open only when they are kept separate.

In the last stage of the disease, the conjunctiva becomes spongy and swollen to such a degree that the lids cannot close. The cornea is deeply situated in the middle of this

mass, and seldom becomes affected; vision does not suffer.

In some, this disease sometimes terminates very unfavourably. The transparent cornea is sometimes affected, and vision is lost. In some the inflammation gradually disappears, the globe of the eye either wastes away, or preserves its usual size. In others, the inflammation passes to the chronic state, and becomes very obstinate; there sometimes only remains an insupportable pain at the bottom of the eye, without much alteration externally. The state of convalescence is long, when the disease has been violent; vertigo, and pain in the head, when it is bent forwards, and imperfect vision, often remain after the disappearance of the redness of the eye.

The two European travellers could not discover that the disease was contagious; and the miasmatical character, which it assumed at the time of the French expedition, appeared to be accidental. M. Ehrenberg is not aware of any symptom which distinguishes the ophthalmia of Egypt, from the common rheumatic ophthalmia; but the following difference exists in the ophthalmia of Europe, and that of Egypt: viz. that the former principally attacks the lids, whilst the globe of the eye chiefly suffers in the latter; dust, heat, putrid emanations, irritation of the eye by light reflected on planes of white sand, &c., are not, as is commonly said, the principal causes of the ophthalmia of Egypt; this complaint is produced by the changes of temperature and the state of atmosphere.

## Medical and Philosophical Intelligence.

*Aneurism of the Abdominal Aorta, with a cribriform condition of the Artery.*—A man, æt. 58, was admitted into the Hospital Saint Antoine in October 1821; he was emaciated, wrinkled, and presented all the appearances of premature senility. Since 1814 he had been subject to attacks of gout in the feet and hands; these returned twice every year, and lasted each time about fifteen days. At the time of his entrance into the hospital, he complained of a deep-seated pain in the abdomen, increased on pressure; this region however was soft, and upon examination, no disease could be detected; the pulse was small, and the extremities cold. The pain was attributed to a metastasis of gout. At the expiration of twelve days, his condition remained unaltered; about this time, while in the act of rising, he fell into a state of syncope, and died in about an hour.

On dissection, forty hours after death, the lungs were found healthy, and the heart of its natural volume, but the parietes of the left ventricle were more than an inch in thickness, those of the right from three to four lines only. The mesentery was developed and voluminous, and coagulated blood was infiltrated into the cellular tissue composing it. This extravasation occupied the whole extent of this part of the abdomen, extending from the columns of the diaphragm to the base of the sacrum; it was removed carefully, together with the great vessels, without deranging their anatomical relations. The aorta, opened posteriorly throughout its whole length, presented, upon a level with the columns of the diaphragm, a dilatation about the size of a small hen's egg, involving the whole circumference of the vessel; the internal surface of this tumour was smooth, but was perforated by a multitude of minute foramina, so small that water poured into the

artery, filtered guttatim through its parietes; it was evidently by this route that the blood had been extravasated into the cellular tissue of the mesentery. The internal coat of the aorta lined the dilatation throughout, and was smooth, polished, and altogether sound, with the exception of the perforations above mentioned; the middle coat terminated circularly and abruptly, above and below the aneurism; around the body of the latter it had entirely disappeared; the cellular investment, attenuated and adherent to the internal coat, presented like the latter, a multitude of little foramina, but was in other respects unchanged. The dilatation extended as far as the origin of the celiac artery, which was not involved; the lumbar vertebræ and surrounding soft parts were sound.

The narrator of the preceding case, M. Vosseur, requests attention to the following circumstances. 1st, the patient presented no striking morbid symptoms which could have led to a suspicion of the disease of the aorta; 2d, the blood was progressively extravasated and infiltrated into the cellular tissue of the mesentery through the foramina in the artery, for the coagula in contact with the artery, were nearly as colourless as those situated the most remotely—but admitting this slow and progressive extravasation, how are we to account for the death of the patient?—this was owing, according to M. Vosseur, to his having attained the last stage of debility; 3d, on examining the body, no rupture of the aorta was discoverable, and it was evident that none existed.

An aneurismal tumour of a similar character was twice observed by Mr. Pott, situated in both instances upon the leg, in the course of the posterior tibial artery; it is noticed also by Boyer in his *Traité des Maladies Chirurgicales*, and the following passage occurs in an

article by M. Roux, in the 20th volume of the *Dictionnaire de Médecine*, p. 250. "There are sanguineous fungous tumours which attack exclusively, or almost exclusively, the vessels which circulate red blood; they are, it may be said, arterial or aneurismatic. The alteration commences in a large arterial trunk, or in several arteries of a moderate caliber,—these vessels swell, dilate—while their parietes undergoing some unknown disorganization, ulcerate—and are pierced by an infinite number of minute openings, presenting a cribriform appearance, and permitting the blood to escape; this fluid, which can only percolate slowly, instead of forming in the neighbouring parts a simple cavity, as in cases of aneurisms, infiltrates by degrees, penetrates into the surrounding cellular tissue and the adjacent parts, the organization of which soon disappears, and is transformed into a soft spongy tissue steeped in blood, which can only be slowly expressed." In the preceding case, the importance of the diseased vessel, and the extensive extravasation of blood, occasioned the death of the patient before this transformation of the cellular tissue had had time to take place. M. Vosseur observes, that the present is the first example of a similar disease affecting the aorta.—*Journal Général de Médecine, &c.*

*Softening of the Womb.*—In the last number of the *Repertoire d'Anatomie*, (tom. v. p. 32,) there is a valuable communication on softening (ramollissement) of the womb, by Dr. Luroth.—The following are the principal points alluded to in the paper:—

1. By softening, when applied to pathological anatomy, is generally understood the diminution of cohesion in organic tissues.
2. This alteration has been observed, with few exceptions only, in all the organs and tissues of the body.
3. Softening of the uterus seldom takes place but in the pregnant and puerperal states.
4. This complaint may be divided into three stages; the first, where the organ is only very flaccid, relaxed, soft, and often infiltrated with serum, but without any very remarkable alteration of structure; the second, where the uterus is very friable, and its structure altered to such a degree that its tissue presents but very weak traces of organization; and the third, in which there is a liquefaction of the part, or reduction of it, into an inorganic pulp.
5. If the softened parietes of the uterus are also thinner, the complaint may be termed atrophy of the organ.
6. The symptoms to which softening of the womb gives rise are vague and uncertain; additional cases are wanting before they can be determined with precision.
7. Softening of the uterus, and even the disposition to this alteration, may cause difficult or unnatural labour.
8. Rupture of the womb may occur with

this alteration, particularly if the parietes are thin as well as softened.

9. In most cases, this affection is produced or favoured by an inflammation of the organ; it is more rarely a primitive alteration dependent on diminution of the vitality, and an anomaly in the nutritive action of the affected part.

10. The inflammation which precedes the softening has a tendency to effect a prompt disorganization, and belongs to that class which has been termed by some authors asthenic, putrid, &c.

11. Putridity of the uterus may be regarded as a kind of softening, and is generally seated in the neck of this organ.

12. Termination by resolution in putridity, as indeed in softening generally, is rare.

13. The diagnosis of these affections is difficult; an examination per vaginam, however, affords material assistance. The prognosis is always unfavourable, and the result generally fatal. No curative measures which have been devised appear to have any effect.—*Lancet.*

*Iodine in Gouty Diseases.*—In the July number of the *Journal of Foreign Medicine*, it is stated that this article had been successfully employed by M. Gendrin in the treatment of arthritic diseases; the following is a translation of the paper, mentioned as having been addressed by that gentleman to the Institut Royal.

Among obstinate maladies, gout is, beyond contradiction, one of the most severe and painful. The great variety of medicines proposed for its cure sufficiently shows this. So many attempts, as yet futile, ought to inspire the greatest caution in any one who does not wish to expose himself to the chance of uselessly increasing the catalogue, already so numerous, of antiarthritic remedies. This caution, although it should lead to doubt, should not check efforts to extend the resources of art against so formidable an affection. In this persuasion, I am going to mention a remedy which I have successfully used in treating gout, both to resolve chronic engorgements and the articular concretions which result from repeated attacks of this disease, and also to cure its paroxysms in the acute stage. This remedy is iodine, the rational and proper use of which is without the least inconvenience.\*

\* M. Gendrin observes, that by many physicians, iodine is regarded as a very powerful irritant, while according to him it is much less active than the mercurial preparations. Its local action being stimulant, it should not be given when the digestive apparatus is in an irritable condition; the most immediate and constant effect which he has observed, is constipation, to such an extent as to render it important to exhibit occasionally gentle laxatives. When used in frictions it is more liable than when given internally, to induce

I first employed it externally in chronic gouty tumours, because it had before been extolled in old swellings of the joints. Its resolvent action was so rapid, that I asked myself if it did not, in these cases, act on the nature of the disease? Following this indication, I used it against acute fits of gout, both externally and inwardly. Success in a violent fit of gout in a very strong man, induced me to continue my experiments. Since that, seven patients with acute and violent gout have been completely cured by this remedy. In two only was the cure difficult. There it was necessary repeatedly to have recourse to the administration of the iodine, to prevent the paroxysms, and check their development when threatened. In all these cases, the continued use of iodine for two or three months after the complete cure of a paroxysm, has prevented its return. One patient has passed eight times when he should have had it, without its appearing. Another, five,—one, four,—and two, only three, without relapse.

Of four gouty subjects with chalky concretions and chronic engorgement of the joints, two have been quite cured for more than four years, and only employed the iodine externally, but for a considerable time. One has been cured a year; and the fourth is now under treatment.

With all these patients, its action has been seconded by a regimen nourishing and slightly tonic for chronic gout, and demulcent in the acute gout.

I do not know whether experience will continue to give as favourable results; but I make the present cases known that other practitioners may give the remedy a fair trial; and, if their experience of its utility corresponds to mine, that it might be generally adopted in the treatment of this disease.—*Journal Général de Médecine.*

*Malformation of the Heart.*—In a paper contained in the *Journal Général de Médecine*, M. Gendril has noticed in detail, the different malformation of the heart; the principal of which are the unusual communications of the cardiac cavities, by the absence of the inter-ventricular septum; the irregularity of the vascular orifices of the heart, for instance, the transposi-

anorexia, and a saburral condition, for which evacuations of the primæ viæ are necessary, as when the same phenomena follow the exhibitions of the mercurial preparations.

Whether internally or externally employed, iodine should be exhibited in small and progressively increasing doses. When the symptoms above mentioned do not make their appearance in the first ten or twelve days, or when they have been removed by an appropriate treatment, the dose of the iodine may be carried to a considerable extent, provided the patient be confined to a suitable diet, and drink copiously of any demulcent beverage.

tion of the aortic orifice to the right ventricle, the contraction or even obliteration of the pulmonary artery, and the substitution of an impervious trunk. From the similarity of these different malformations, M Gendril concludes, 1st. That the heart, originally, is formed of a single cavity.

2d. That afterwards this organ is divided into two cavities, an auricle and a ventricle.

3d. That, at a more advanced period, these two cavities are themselves divided each into two others, which, however, communicate for a long time after, by the imperfection of their septa. To these indisputable facts, the author submits the following opinions.

The pulmonary arteries arise at first from the aorta, by means of the arterial canal; it is only at a later period that the trunk of the pulmonary artery is formed, and opens into the heart from without inwards, by a mechanical impulse à tergo.

When the pulmonary artery opens into the ventricle, the communication between the ventricular cavities is then, for the first time, closed. The pulmonary veins open originally into the superior vena cava.

By the development of the superior part of the auricle, which is affected at the expense of the lower part of this vein, the pulmonary veins reach the auricle, and it is at this period that the formation of the auriculo-ventricular septum commences. Whatever may be the varieties of the malformation of the heart, they are nevertheless subject to certain invariable rules, dependent on the regular and constant succession of the changes which are effected in this organ.

The aorta, or pulmonary arteries, never open into the auricles; the venæ cavæ, or pulmonary veins, never open into the ventricles; neither the right auricle, nor the left ventricle, are wanting, if there be more than one cavity. The pulmonary veins never open into the ascending vena cava, nor into the aorta; the pulmonary arteries never open into the venæ cavæ.

If there be any imperfection in the inter-ventricular septum, it is always at its superior part towards the base of the heart, and never at its inferior part towards the apex. When the orifice of the pulmonary artery does not exist, or only exists imperfectly, the ductus arteriosus is never obliterated; this is also the case when the aortic orifice is not free; but, in this case, the orifice of the pulmonary artery is constantly present. When the orifice of the aorta, or that of the pulmonary artery, is completely obliterated, there always exists an opening in the inter-ventricular septum. When the pulmonary artery is imperfect, it is always on the side of its orifice, and never towards its continuity with the ductus arteriosus.

If the ductus arteriosus is obliterated at its origin, the aorta and pulmonary arteries are always perfect, and open into the same ventricle; or if they open into different ventricles, there is a communication between these cavities across the septum which separates them.—*Lancet.*

*Experiments on the Re-union or Cicatrization of Wounds of the Spinal Marrow and the Nerves.* By M. P. FLOURENS.—In my former experiments it has been seen, that different parts of the nervous system may be completely separated from the rest of the body, and still present a certain degree of life or action, so that they are susceptible of being united to parts from which they have been separated, and recover in certain cases the full exercise of their functions. One of the hemispheres of the brain, for instance, on being divided, instantly loses its functions; after the lapse of a certain time the incision cicatrizes, and the functions of the part return. This, indeed, is the case with all the parts of the brain and spinal marrow.

The experiments of Fortana on the re-union of the nerves are well known; they have been repeated by a great number of physiologists and by myself also. I divided the eighth pair on a cock. Two months afterwards, the wound being cicatrized, I again exposed the nerve on which I had operated, and found it a little enlarged, but the ends completely united. I thought it important to ascertain whether the inferior extremity had acquired, by its union with the superior, the power of again uniting if divided, which I found, by a repetition of the experiment, that it had.

The following are the results of my experiment, which contain some points that do not appear hitherto to have been noticed:—

The ends of a nerve that has been completely divided, can again unite.

A nerve that has been divided unites, and re-union also takes place if the nerve be again divided below the points at which the experiment was originally tried.

Two different nerves may be crossed so, that the inferior extremity of the one may correspond to the inferior of the other, and *vice versa*, and in this case also union takes place.

Lastly, if the divided extremity of the eighth pair be brought in contact with a cervical nerve, union takes place.

As to the return of the functions, I have not been able accurately to determine this point in some of my experiments; the functions have, in others, not been restored.—*Ann. Sci. Nat.*

*Apparent Death.*—At a late meeting of the Académie Royal de Médecine, M. Bourgeois related a case of apparent death, which happened in a woman immediately after labour, and also in the infant. The former was in a state of syncope after uterine hæmorrhage. The child presented all the appearances of congenital asphyxia. The woman was 26 years of age, and had been in labour, with her first child, about twenty-four hours. Every thing was going on very favourably, when, from some sudden vexation she experienced, the pains diminished; and symptoms of internal hæmorrhage occurred. She was soon delivered of a child, which was apparently dead. She had now frequent attacks of syncope, during one of which she was convulsed, and appeared to breath her last. Such was the

state of the case when M. B. arrived. Although, however, he was assured that the mother was dead, and from all appearances he believed it was the fact, he proceeded to introduce his finger into the cavity of the uterus, through a firm clot which filled the vagina. He stimulated the internal surface of the uterus, whilst he desired friction to be actively employed over the whole body. Stimulating odours were also applied to the nostrils, and cataplasms of vinegar, and water nearly boiling, to different parts of the body. In the course of fifteen minutes, a slight muscular motion was perceived in the abdomen, which was the first indication of uterine contraction. Large clots were quickly expelled, and the powers of life were evidently returning. The patient was now attacked with a paroxysm of convulsions resembling epilepsy. The functions of the various organs gradually returned, but still she remained for some time perfectly insensible. During the whole of this time, the infant had remained near a fire, wrapped in linen, and considered to be dead. By inflating the lungs and stimulating the surface of the body, it was at length restored. M. Bourgeois cautions practitioners from too hastily presuming that the vital spark is entirely extinct, after severe hæmorrhage. Patients may lie for a considerable time in a state of syncope, and yet be recoverable if proper means are employed. The case would certainly have afforded a fair opportunity of trying the transfusion of blood into the veins, as it has latterly been practised in this country.—*Lond. Med. Gaz.*

*Intermittent Fever affecting only one half of the Body.*—Madame B—, aged 22 years, of a sanguineous temperament and robust constitution, had always enjoyed uninterrupted health. Without any evident cause, she was attacked by rigour, followed by heat, and a cephalalgia, occupying only the left half of the cranium and face; after continuing some hours, these symptoms subsided, and a perspiration made its appearance, limited to the same side. The paroxysm returned, with increased severity, the same hour on the following day, and ran through its several stages as before. The attending physician, M. Lepalvy, was present at the third accession—after a rigour which, sitting on the middle of the dorsal vertebra, radiated into the left superior and inferior extremities, reaction commenced, accompanied with headach, suffusion of the left cheek, brilliant, animated and weeping eyes, the teguments of the cranium tender upon pressure, slight convulsive movements of the upper eyelid, tongue red and dry on one side, the other moist and natural. The face presented a very curious aspect, red, tense and shining on the left side, while on the other it was not changed from its natural condition. The chest sounded well throughout, but a crepitating râle could readily be detected on the left side, unaccompanied by cough or expectoration; the epigastric region and the abdomen were also painful on the same side. The paroxysm began at 8 o'clock, increased

till ten, when it began to diminish, and terminated by a gentle perspiration, (limited as above mentioned,) eructations, and the discharge of a rose-coloured urine, depositing a lateritious sediment. The disease yielded readily to the sulphate of quinine. Analogous cases are met with in authors; M. Duval saw a soldier at Brussels, attacked with a quartan fever, which presented a singular anomaly. The cold stage continued about two hours, and was accompanied by a violent spasmodic contraction of the muscles of one side of the body. The contraction was more sensible about the face, and gave to the patient a hideous aspect. The opposite side was attacked on the following paroxysm, and so on alternately. The *Revue Medicale* for 1814, contains the case of a man who having slept in a room recently repaired, contracted an intermittent fever, which involved only the half of the body corresponding to the wall, against which the bed had been placed. Mangold relates an instance of quartan fever, with hemiplegia, sweats, and convulsions, sometimes implicating the right, and at others, the left side. M. D'Auxiron has seen a case in which only the inferior half of the body beneath the diaphragm was attacked. *Journ. de Med.* 1766. Both Van Sweiten and Laméthrie have seen a young woman affected with catalepsy of one side alone; and Morgagni cites a case of icterus, occurring in an old man hemiplegic on the right side, the consequence of apoplexy. The icterus was limited exactly to the paralysed side. Etmuller, Paulinus, Pechlin, Plenck, Berger, and Robert, relate similar, or analogous instances.—*Journal des Progres, &c.*

*Method of treating an Abdominal Tumour supposed to contain Hydatids.*—A man, about 34 years of age, was recently admitted at the Hôtel Dieu, who, for the last 18 months, has complained of tumour in the situation of the liver, which increased very gradually, but at present occupies the whole of the right hypochondrium. Attentive examination showed that it is connected with the liver. It is perfectly free from pain even when pressed upon; has not produced any derangement of the healthy functions of the part, and gives rise to no other inconvenience than that of preventing the man from pursuing his usual avocations. He has had advice from many different medical men, and has tried a great variety of internal and external remedies, to no purpose. It so happens that M. Recarnier, about a year ago, treated a tumour somewhat similar to this, which proved to contain hydatids; and as the means he then employed were perfectly successful, he determined to adopt them on the present occasion. The tumour in the present instance is, however, much larger than the former one; and, although some thought they could perceive that sort of crepitation which hydatids cause when pressed against each other, yet the diagnosis is by no means very clear. However, M. Recarnier proceeded, as in the former case, to puncture the tumour with a very fine

trocac, in order to ascertain the nature of its contents. The fluid which flowed out, as had been anticipated, was limpid and colourless, and did not coagulate by heat. After having extracted a small quantity of this fluid, the puncture was carefully closed and healed; and a week after, when the tumour was again distended to its former size, the caustic potash was applied, so as to form an opening into the cavity of the cyst. In the former case, this method answered admirably, as great numbers of hydatids were extracted. A sufficient quantity of water was then injected, to fill the space they had occupied, in order to avoid the introduction of the air; the external wound was dressed in the usual manner, and the patient recovered rapidly. The object in applying caustic, rather than making an opening by incision, is to produce adhesions between the cyst and parietes of the abdomen, so as to form an uninterrupted canal from the tumour to the external parts.—*Lon. Med. Gazette.*

*Singular Disease of the Spleen;* communicated by Dr. Westman to the Medical Society of Stockholm.—A young woman, æt. 28, had a suppression of the menses in consequence of checked perspiration, followed by colic and swelling of the epigastric region. To this state succeeded a hemorrhage from all the emunctories of the body, and when this ceased an induration was observed in the left side. Subsequently an extravasation of serum distended the cavity of the abdomen, especially about the umbilicus. The catamenia having resumed their flow, again disappeared, and were followed by a hemorrhage like that above mentioned, so profuse as to terminate the life of the patient. On dissection, the liver was found atrophied; the spleen greatly enlarged, its parenchyma, transformed into a glutinous fluid, enveloped three osseous concretions, one of which was two and a half inches in length.—*Bull. des Sciences Médicales.*

*Circular Contraction of the Rectum.*—Madam M., ætat. 40, of a full habit and strong constitution, had experienced, for the last three years (since the time of her accouchment,) a great difficulty on going to stool. Frequently fifteen days would elapse without an evacuation taking place; then violent pains, which the patient compared to those of labour, came on, and were followed by the expulsion of a small quantity of liquid, or solid fecal matter. The patient was in this state when she consulted, on the 30th of August, 1827, Dr. Longueville. This gentleman, suspecting some affection of the organs contained in the pelvis, submitted them to an examination. The womb, although large, did not appear diseased; but on the finger being introduced up the rectum, it met, at the distance of two or three inches, with an obstruction which appeared like a circular contraction of the gut. Dr. L. attempted to pass the finger beyond this point, which he did with considerable difficulty, and found

that the rectum, above the stricture, was dilated, or rather distended, with fecal matter. The daily use of injections was prescribed, and the introduction, up the rectum, of a tent covered with opium cerate. On the 11th of December following, Dr. L. again saw the patient, who confessed that she had neglected, or but imperfectly followed up the directions; two days before she had a very copious stool, since which she experienced greater pain than usual. The abdomen was swollen, and painful on pressure; frequent liquid motions, and nausea, small pulse, great anxiety, and cold extremities; these were the symptoms in addition, which presented themselves, and in two days afterwards the patient expired.

The body was examined fourteen hours after death. There was considerable effusion of a sero-purulent fluid in the abdomen; the peritoneum was thickened, and inflamed; the intestines were distended with gas, and covered with a yellowish soft, adventitious membrane. The muscular coat, and the layer of cellular membrane which unites it to the mucous tunic, was converted into a fibro-cartilaginous structure. Towards the anus there were a great number of varicose veins, and, about four inches up the gut, there was a circular contraction, scarcely admitting of the passage of a quill, and which must have considerably increased since the 30th of August. The mucous membrane in this point was healthy, but thin; the muscular coat, and the two layers of cellular membrane uniting it to the peritoneal and mucous tunics, were changed into a thick, gray, hard tissue.—*Jour. des Progrès, &c.*

*Melancholy, complicated with Aphonia, cured by means of the Actual Cautery.*—A man, æt. 30, of a strong constitution and sanguineous temperament, was affected with deep melancholy complicated with aphonia, for the relief of which a variety of measures had been unsuccessfully adopted. Dr. Rossi ultimately determined to try the effects of the actual cautery applied upon the vertex; its action extended to the bones beneath. On the following day the condition of the patient was sensibly improved, and he was able to speak in a low tone. In proportion as suppuration was established, the intellectual faculties became more free; the improvement of the voice however did not appear to be progressive. Purgatives were employed, the posterior part of the neck was covered with blisters, frictions with ammoniacal liniment were made anteriorly, and an infusion of arnica with the addition of camphorated ether was given internally, while at the same time the patient inhaled the vapour of ammonia. After this treatment had been continued during two months, the patient recovered his health.—*Repertorio di Torino.*

*Application of Plates of Lead to Wounds.*—M. le Baron Yvan, head surgeon of the Hôtel Royal des Invalides, in a letter to M. Reveille Parise, gives us the result of his extensive

experience on the dressing of wounds by the application of thin plates of lead. 1st. The first trials were made on large chronic ulcers, of disgusting aspect, abundant suppuration, foul bottom, callous edges, and with most a tendency to erysipelas to some distance. The first day of the application, the pain was diminished, the suppuration changed its nature, inflammation yielded, and the edges in thickening also contracted.

2d. The extremities, which were covered with thick crusts, were equally wrapped up, as in elephantiasis. Baths, poultices, styrax plasters, sulphur cerate, had all failed in removing these crusts; but by means of the lead they fell off without effort, and permitted again the cutaneous transpiration.

3d. Emboldened by this success, he did not hesitate to employ the same means with sores in a state of hospital gangrene, and with decided good effect.

In conclusion, he adds that this mode of treating ulcers offers the immense advantage of diminishing pain, resisting the tendency to erysipelas, thickening the edges, modifying suppuration, in fine, of causing solid cicatrices.—*Lon. Med. & Phys. Journal.*

*Congenital Dislocation of the Femurs.*—A man, seventy years of age, was affected with retention of urine, for which he was admitted into the Hôtel Dieu; the water was withdrawn, but the patient soon expired. He also appeared to have congenital dislocation of the femurs, on which account M. Dupuytren wished to examine the body with great care.

The thighs could not be separated from the body, without describing with the feet a segment of a circle; the trochanters were more closely approximated to the ilia, and much more elevated than in the natural state; the heads of the ossa femorum were situated higher, the knees carried more inwards, and the thigh shorter than usual; in fine, there was a total change of relations, and a marked difference in direction and length. From this change of the relations of the bone, it results, says M. Dupuytren, that the cavity destined by nature for the bone, either does not exist, or is more or less obliterated. From the change in length and direction, the points of insertion of some of the muscles are also changed. Some of the muscles being relaxed, others put on the stretch. The superior part of the thighs was enlarged, the trunk bent backwards, and the abdomen carried forwards; the pelvis, instead of being oblique, was nearly transverse; the buttocks soft and flabby, which must have been owing to the approximation of the insertion of the glutæi magni. Passing then to the examination of this region, it was found that the glutæus magnus was relaxed, whilst the medius was put on the stretch. The glutæus minimus was destroyed; and the pyriformis, instead of being placed obliquely, as in the natural state, was on a horizontal plane; the gemelli and quadratus were extended, the adductors shortened. All the arteries (even those of smallest caliber) was ossified. The

ligamentum teres was wanting on the right side, in the situation of the acetabulum; there was a small triangular cavity filled with adipose cellular tissue, on the left side; there was no trace whatever of this cavity on the right side; the head of the femur, which was rough, and considerably diminished in size, was situated in a superficial cavity, surrounded by a fibro-cartilaginous capsule, which was the only impediment to the still further ascent of the bone. On the left side, on the contrary, the head of the femur was more developed than in the natural state, and situated in a deep bony cavity. The left os ilii was as thin as a piece of writing-paper, and perforated in some points.

There was great power of motion between the last lumbar vertebra and the sacrum; but, on examining this articulation, the only cause which could be discovered, was the relaxation of the cartilages. — *Lancet*.

*Amputation of the hip joint; by M. DELPECH.*

—In June 1823, a young man came to the hospital of Montpellier, with numerous fistulous openings in the thigh, some of them leading down to the bone, of which the probe detected several loose portions, and some fragments had been evacuated with the discharge. It was manifestly a case of necrosis of the femur. The repeated attacks of inflammation in different parts of the limb had given to it a degree of lardaceous density, (*densité lardacée*.) It was impossible to amputate in the course of the thigh, from the necrosis having extended so high up; but the hip joint remaining sound, it was resolved to remove the limb by its disarticulation, notwithstanding the great enlargement of the surrounding soft parts, as M. Delpech had found by experience, that such tumefactions speedily disappear when their exciting cause has been removed.

The crural artery was tied in the first place by means of a thread passed through the groove of a catheter, the point of which had been insinuated between the artery and vein. Instead, however, of making two flaps, as has been usual (one external and the other internal,) M. Delpech resolved to make only one large flap, and that on the inside: this flap he proposed bringing from within outwards, so as to make it cover the articular cavity with a thick cushion of muscle.

This plan was but incompletely executed: the inner flap was formed of the proposed length and thickness, by turning the knife at an early stage of the operation from within backwards, so as to take in the most fleshy part of the thigh. The limb was placed in a state of artificial abduction, and the capsular ligament was pierced, and then divided on the head of the bone through the half of its circumference. There was rather brisk hæmorrhage at this time, which, however, was suppressed by tying one artery. The thigh being now turned inwards, in place of cutting in a curved line nearly parallel to the upper or iliac edge of the buttock, the muscles of which would thus have been divided about their middle, and the formation of an external flap

avoided, the knife was directed obliquely downwards, by which a small external flap also was made. The capsule was then divided through the rest of its circumference on the head of the bone, and the limb completely removed. Two more ligatures were necessary. The only thing remaining to be done was to promote the immediate union of the two flaps, as the patient could not probably have borne the suppuration of such an immense extent of surface. The extreme density of the inner flap rendered it very difficult to make it fold outwards, to effect which required both perseverance and some degree of force. Numerous points of interrupted suture, including the skin only, brought the edges exactly together; but the surgeon had the mortification to perceive that the line of junction crossed the external edge of the acetabulum, where the covering over the joint was not sufficiently thick. Compresses and bandages were applied, so as to keep the flap in its proper position. Next day the dressings required to be changed, in consequence of the abundance of serous discharge, by which, however, the swelling of the parts was diminished, and union by the first intention took place, except at the spot over the edge of the articular cavity above mentioned. Here a sero-sanguinolent discharge came on, followed by suppuration of a healthy character, which gradually diminished, and at the end of thirty days the cure was complete. At present this patient appears to enjoy perfect health, and even walks tolerably, by means of a wooden leg.

The second operation was performed in 1824, and precisely on the plan above mentioned, no difficulty having been experienced in forming the flap in the manner intended. The patient had suffered from an old ununited fracture at the upper part of the femur. Unfortunately he would not submit to the operation till his life had become endangered by abdominal inflammation. The operation perfectly succeeded; the adaptation of the flap to the other parts was complete, and the wound healed perfectly in 20 days, without any suppuration. The patient died eight months after, and the dissection of the stump showed the articular cavity filled with the soft parts constituting the inner surface of the flap; cellular membrane, of considerable density, formed the medium of union between the joint and the parts covering it.

The previous ligature of the artery, M. Delpech supposes necessary, on account of the little dependence which can be placed on compression for arresting the hæmorrhage. The suture he used, because he has not found it hurtful, provided the skin only be included; but what he chiefly insists upon, is the advantage of not making an external flap, because the tendons which are then involved unite less readily to the parts to which they are applied, and are much more disposed to suppurate than the muscular fibres. He regards it as of great moment to cover the cavity of the joint well with a thick envelope of soft parts;—this precaution, indeed, he regards as mainly contribu-

ting to prevent suppuration.—*London Medical Gazette.*

*Large Fibrous Tumour removed from the Uterus.*—A woman, of from 50 to 60 years of age, of delicate health, and rather emaciated, had complained for a long time of pains in the loins, shooting down to the fundament and thighs; she had also occasional hæmorrhage from the vagina, proceeding from a tumour of the womb, which she had had for some years. She never had been pregnant. M. Dupuytren discovered, upon examination, a large, smooth, hard, round tumour, very moveable, which, together with its size, induced him to believe that it originated from the cellular tissue immediately beneath the mucous membrane of the uterus, but he was not able to discover either through the rectum, or by the vagina, whether or not it was affixed to that organ by peduncle.

The woman being desirous of having an operation performed, was placed in the position for the operation of lithotomy: when the labia were opened the tumour was perceptible: a pair of forceps were then introduced upon the finger, and the tumour was gently drawn downwards: the patient was recommended to assist this by making efforts to evacuate the bowels, and an assistant made pressure on the hypogastrium. When fairly drawn down, M. Dupuytren gave the first pair of forceps into the hands of an assistant, (retaining their hold of the tumour,) whilst with a second pair he attempted to draw it entirely out, making pressure with his finger introduced into the anus, but the perinæum became so much distended as to make him fear that it would be ruptured. Seeing that the size of the tumour rendered this accident almost inevitable, M. Dupuytren determined to make an incision in its upper part by means of a button-pointed bistoury immediately: the efforts to draw it forth being renewed, it was entirely extracted: it was about the size of a new-born child's head, and was attached to the internal and posterior part of the neck of the uterus by a narrow stalk, which was cut with a pair of curved scissors.

A few spoonfuls of blood only were lost during the operation, which the patient bore admirably. At the instant when the tumour was removed there was a discharge of a few ounces of violet-coloured blood. On inspecting the tumour, which weighed about eight ounces, it was found to be covered with a very vascular mucous membrane; on removing this, a white and dense substance presented itself; its surface was covered here and there with slight ulcerations. The hæmorrhage had doubtless proceeded from the outer and very highly vascular membrane: the white substance, when cut into, appeared perfectly analogous to the structure of the inter-vertebral substance.

*Remarkable Malformation of the pupil of the right eye after a wound.*—The following

case is interesting, both in a surgical and physiological point of view, as a proof of the injury that may be inflicted upon the eye without loss of sight. A healthy young man wounded himself in the eye with a pointed instrument, and made an opening through the cornea, similar to the incision in the operation for cataract. A considerable quantity of blood-water escaped through the wound, and the power of vision was for the time destroyed. Upon examination, the eye was found collapsed; the aqueous humour had escaped. It was impossible to determine whether the lens had passed through the wound, or whether it was still in the interior of the globe, and separated from the adjacent parts; neither could the state of the vitreous humour be ascertained. When the supervening inflammation was reduced, and the eye could be opened, it was discovered that the margin of the iris was torn in three places, and that a portion of it was lost entirely. There were now three openings through the iris: one on each side, and one in the middle. It was now presumed, from the appearance of the eye, that the lens had escaped at the time of the accident. It was remarkable that the patient afterwards saw objects in their true position, and that he had not double vision: he saw distinctly with the injured eye when the other was closed. From the malformation of the iris, it would have been imagined that he would have seen every object tripled, and that his power of vision would have been otherwise injured, from the entrance of the rays of light through three pupils, and from their converging to a focus upon three points of the retina. Four years have now elapsed since the infliction of the injury, and although the eye is rather weaker than the other, the patient sees every object clearly and distinctly. An attempt was made to improve his weakened sight, by means of spectacles, which it was hoped would perform the duty of the lens; but their use was given up, as the patient saw objects double and triple as long as he continued to wear them.—*Grafe and Walther's Journal.*

*On the Property which Sulphuric Acid possesses, of dissolving without oxidizing the simple oxidizable bodies.*—Vogel, of Bayreuth, while engaged in some researches upon anhydrous sulphuric acid, observed that, when placed in contact with sulphur, it assumed a beautiful blue colour, which passed to a green or brown by the addition of a greater quantity of that substance. Water precipitated the sulphur from these combinations, and heat decomposed them. It was very probable that the sulphur was held in a state of simple solution by the acid, and the analogous examples cited by M. Magnus, no longer leave any doubt on this subject. Müller long ago observed, that tellurium, sprinkled with concentrated sulphuric acid, dissolved into a perfectly transparent fluid, of a beautiful crimson red, without any remarkable development of gas or sensible odour of sulphurous acid. When water was added, the metal was pre-

cipitated in form of a metallic powder of a deep brown, or almost black colour; it was also gradually decomposed, when permitted to absorb the humidity of the atmosphere, forming a sulphate of the oxide, with a disengagement of sulphureous gas. Selenium was dissolved in like manner; and the solution was of a very beautiful green; the addition of a small quantity of water threw down a reddish precipitate. According to Bussy, iodine is also dissolved in anhydrous sulphuric acid, communicating to it a bluish green colour. It follows, therefore, that sulphuric acid has the property not only of dissolving the compound bodies, as Berzelius has proved in relation to the metallic cyanurets, and Vogel of Munich, in relation to corrosive sublimate, but also simple bodies, such as sulphur and selenium, for the oxides of which it has no affinity, and tellurium, with the oxide of which, on the contrary, it forms a crystallizable compound.—*Annalen der Physik*.

*Influence of Camphor in increasing the solubility of Corrosive Sublimate in Alcohol and Ether;* by M. Karls.—At the ordinary temperature, four parts of the ether dissolve one of the sublimate, but when an equal quantity of camphor is added to the latter, three parts only are required. Augmenting the proportion of the camphor, he obtained the following result:—4 parts of ether, with 4 of camphor, dissolved 2 of the sublimate; 4 parts of ether, with 8 of camphor, dissolved 4 of the sublimate; 4 parts of ether, with 16 of camphor, dissolved 8 of the sublimate.

Three parts of alcohol will take up one of the sublimate, but by adding to the latter half its weight of camphor, one and a half part only of alcohol is required. By employing heat, 4 parts of alcohol may be made to dissolve 16 each, of the sublimate and camphor, but upon cooling, there takes place a small deposition.—*Bull. des Sciences Médicales, from the Annalen der Physik und Chemie*.

*Nitrate of Soda in Dysentery.*—This article had been already recommended by Dr. Velsen. (Horn's Archiv. 1819.) In the treatment of a dysentery which prevailed epidemically in 1822, it was employed with such success by M. Meyer, that it became a popular remedy. He lost only two patients out of one hundred. He has given from half an ounce to an ounce of the salt, dissolved in eight ounces of gum water, or decoction of marsh mallows. This medicine, notwithstanding its great analogy with nitre, cannot be replaced by that article; it acts as a refrigerant, purges gently, or determines to the surface. According to M. Meyer, it is applicable to the treatment of all inflammations of the alimentary canal.—*Bull. des Scien. Méd. from Hufeland's Journal*.

*Cardiac Sounds.*—According to M. Barry, the two sounds which are distinguished by the aid of the stethoscope, during the action of the heart, are produced by the dilatation,

and not by the contraction of the cavities of that viscus. The first, which does not always correspond with the pulsation of the arteries, arises from the dilatation of the auricles; the second, from that of the ventricles.

In cases of hypertrophy, when the contractions of the heart are most energetic, the sound instead of possessing greater force, has a much duller character, while the impulsion is stronger. On the contrary, if there be an unnatural thinness and dilatation of the parietes of the heart, the sound is more distinct, and the impulsion weaker, and thus, according to the nature of the sound, the different states of hypertrophy and *amincissement* may be detected.—*Journal Univ. des Sciences Médicales*.

*On the Properties of British Opium.*—The London Medical and Physical Journal for June, contains some observations, by a member of the College of Surgeons, on the superiority of British to Turkey opium. It is purer, and while its anodyne properties are quite equal, its narcotic or deleterious qualities are much weaker than those of the latter. The writer has given it to the extent of from three to six grains in the day, in a great number of cases, and recollects only two instances where it had any unpleasant effect, while in most cases it has produced decided relief from pain, without exciting the least headach, vertigo, or confusion of intellect,—effects, which he supposes to arise from its containing a larger proportion of morphine and a less proportion of narcotine than the Turkey opium. From a recent report of the French Institute, it appears that the same conclusion has been drawn respecting the opium grown in France.

*On the Bitter Principle of Aloes, the Aloetic Acid of M. Braconnot;* by M. Liebig.—The peculiar substance resulting from the action of nitric acid upon aloes, unites with the different bases, and forms salts which detonate upon the application of heat. M. Liebig made some experiments on this body, not long since, but they led to no satisfactory result; he has recently repeated them, and has ascertained the detonating principle to be carbazotic acid. The bitter of aloes is obtained in great quantity, by employing nitric acid of 1.25; with potass it forms a purple salt, slightly soluble, which precipitates the salts of barytes, lead, and peroxide of iron, in flocculi of a deep purple, and the nitrate of the peroxide of mercury, in flocculi of a clear red colour.

In order to analyze the salt formed by its combination with potash, M. Liebig decomposed by means of the acetate of lead, and contrary to all expectation, the weight of the precipitate was found to be less than that of the salt of potass employed. The water employed in washing had a yellow colour, and deposited minute crystals of a similar hue; these crystals treated at an elevated temperature with the sulphate of potass, furnished

carbazotate of potass, from which the carbazotic acid was separated.

When aloes is heated with nitric acid, of 1.432, as long as there is any disengagement of nitrous vapour, the liquid afterwards mixed with a little water, to separate a small quantity of the bitter principle, neutralized with potass and evaporated, a great quantity of the carbazotate of potass is obtained in beautiful crystals. The bitter principle of aloes is therefore a combination of a peculiar substance, participating the properties of resinous bodies, and carbazotic acid.—*Annales de Chimie, &c.*

*Belladonna.*—MM. Ranque and Limonier recommend an ethereal tincture of this plant, prepared by macerating one part of the pulverised leaves in four parts of sulphuric ether; this ethereal tincture is then introduced into a glass retort, with an equal quantity of distilled water, at from 45° to 68°, the ether is evaporated, and the water retains in solution the active principle of belladonna. In order to procure an extract, which shall contain the whole of this active part, it is only necessary to filter the water in order to separate the inert and insoluble resinoid substances with which it is united, and afterwards to evaporate carefully.—*Journal Général de Médecine, &c.*

M. Donné read to the *Institut Royal*, a paper relative to some peculiar effects produced by iodine and bromine upon the vegetable alkalies. When the former, contained in a small capsule, is introduced beneath a bell glass, with a grain or two of each alkali placed on small pieces of paper with elevated edges, and left at the ordinary temperature of the atmosphere for some hours, each alkali will be found to have assumed a particular colour; thus brucine and morphine will have acquired the hue of Spanish tobacco, cinchonine and strychnine a yellow colour, narcotine a deep green, and the others a clear or brownish yellow. The same result may be obtained more promptly, and colours more strongly marked, by heating the apparatus to 18° or 20° cent.

With bromine, still brighter and more beautiful colours are produced, with this difference, that morphine assumes a fine canary yellow, while brucine undergoes nearly the same change as with iodine; narcotine, on the contrary, changes to a beautiful yellow with a slight tinge of red.—*Revue Médicale.*

*Effects of the Inhalation of Iodine.*—M. Chevallier having been a considerable time in a laboratory filled with the vapour of iodine, was attacked immediately after his departure by violent colic pains, which yielded, after a time, to gum water and laudanum. He did not at first attribute his illness to the respiration of air charged with the vapour of iodine, but some time afterwards, having broken a flask containing eight ounces of this substance, and being engaged in collecting the iodine, and separating it from the glass with which it was mixed, he was again attacked in a similar manner.—*Journal de Chimie Médicale, &c.*

*On the Cause of the Difference in the Strength of the Right and Left Arms.*—M. Lecomte lately read before the Institute of France, a paper on the superiority of strength of the right over the left arm. This physician thinks the difference owing to the position of the fœtus in the womb during the latter months of pregnancy. In by far the larger number of cases, the position of the child is such, that its left shoulder, arm, and the whole of the left side, are pressed against the bones of the pelvis; from this pressure, a contraction of the blood-vessels (a kind of atrophy of the left sanguineous system) ensues. The weakness of the left side, then, arises from this congenital disposition. According to M. Lecomte, it is the right side which is the weakest, when the child is placed in the uterus in the contrary direction, which accounts for those who are left handed.—*Lancet.*

*Application of Belladonna to the Cure of Tinnitus Aurium.*—It is stated by Sir Gilbert Blane, in a paper recently read before the College of Physicians, that in two cases of tinnitus aurium which fell under his observation during the past winter, a cure was effected by belladonna. In the first case, which occurred in a lady rather advanced in life, the sensation was so distressing, that she requested, if it could not be removed, something might be administered "to make her sleep for ever." Belladonna was applied externally to the ear, and, after a short time, the relief was complete, and has since remained permanent. The other case was in the person of one of his Majesty's household, who compared the noise to the "rushing of winds, the roaring of cataracts, the discharge of firearms," and every thing that was most distracting. A third of a grain of belladonna was given internally, three times a day, and in three days the patient had entirely recovered from this distressing symptom.

*Preservation of Anatomical Preparations by Oxalic Acid.*—The peculiar property which sugar possesses of preserving both animal and vegetable substances, without materially impairing their colours, induced Dr. M'Donnell to make trial how far the same object might be effected by oxalic acid. A solution in pure water, in the proportion of one scruple to the ounce, he recommends as superior to any other menstruum he has hitherto tried. The muscular parts are as distinctly marked after a month's immersion as on the first day, while the solution continues perfectly limpid, unstained by any colouring matter, and no sign of putrescence or decomposition could be detected.—*Lon. Med. Gazette.*

*Culture of the Cinnamon Tree.*—The elevated temperature of the countries in which the cinnamon tree grows spontaneously, has induced the belief that a high degree of heat was essential to its vegetation. From a circumstance, however, recently observed by M. Boursault, it would appear to be otherwise.

This gentleman possesses in his hot-house, the most beautiful tree of the kind in France; for several years past it has borne seed, which has been sowed, and given origin to numerous plants. One day he observed in a border of his garden, a young plant, which must have been produced from a seed that had fallen from the tree into some vessel placed in its vicinity, and been subsequently thrown upon the spot where it was then growing.

This germination in open air, led to the supposition that it did not require so elevated a temperature, as its origin and mode of cultivation might seem to indicate; to test the truth of this conjecture, several plants were transported to the temperate-house, and two sprouts growing from the seed, were subsequently planted in the great *conservatoire*, where they flourished very well, in open air. During winter, a temperature is maintained in this conservatoire, barely sufficient to exclude the frost.

M. Boursault has sent plants into various countries, especially in the south of Europe, where he believes that it may ultimately be completely naturalized.—*Journal de Pharmacie, &c.*

**Preservation of Hydrocyanic Acid.**—A quantity of hydrocyanic acid, prepared according to the process of Ittner, having begun to assume a yellow tinge at the expiration of a month, M. Schütz rectified a part upon the calcined sulphate of zinc, and obtained a colourless acid, which he preserved during three years and a half; ten drops were sufficient to kill a large dog.—*Bull. des Scienc. Méd.*

**Hydrocyanic Acid.**—Dr. Elliotson reports, as the result of his further experience with this medicine, that it is highly useful when administered with medicines which have a tendency in themselves to induce nausea and sickness. For instance, when given in conjunction with colchicum, the latter will not irritate the stomach, whilst, at the same time, we have its full remedial powers. The importance of such an adjuvant we scarcely need dwell upon.—*Lancet.*

**New Cinchona.**—M. Gondat, professor of botany at Bogota, has recently discovered a new species of cinchona in the large forests which border upon the town of Muzo; he has designated it under the name of *Cinchona Muzonensis*. Botanical characters;—foliis ovato-oblongis, acutis, basi attenuatis, stipulis revolutis, paniculâ brachiâtâ, corollis albis, limbo imberbi. Although it does not produce flowers with hairy borders, as do the best species of this genus, it may notwithstanding be employed with much advantage in medicine.—*Journal de Pharmacie, &c.*

## New Publications.

A Treatise on Gout, Apoplexy, Paralysis, and Disorders of the Nervous System. By A. Rennie, Surgeon, &c.

Hints to Young Medical Officers of the Army on the Examination of Recruits, and respecting the feigned Disabilities of Soldiers; with Official Documents, and the Regulations for the Inspection of Conscripts for the French and Prussian Armies. By Henry Marshall, Surgeon to the forces. 8vo. pp. 224. Burgess and Hill, London, 1828.

This is a most useful work, and no Army medical officer ought to be without it.

A Compendium of the Diseases of the Human Eye, containing Descriptions and Explanations of the various Diseases, illustrated by Engravings, and accompanied by Practical Observations on their Treatment. By Alexander Watson, Fellow of the Royal College of Surgeons, Edinburgh, &c. &c. &c. Second edition, much enlarged. 8vo. pp. 192. Edinburgh, 1828.

A Dissertation on the Nature and Properties of the Malvern Waters, and an Inquiry into the Causes and Treatment of Scrofulous Diseases and Consumption, together with some Remarks upon the Terrestrial Radiation of Caloric upon Local Salubrity. By William Addison, Surgeon. 8vo. pp. 192. London, 1828.

The Evidences against the System of Phrenology, being the Subject of a Paper read at an Extraordinary Meeting of the Royal Medical Society of Edinburgh. By Thomas Stone, Esq. 8vo. pp. 109. Edinburgh, 1828.

The *Anti-Phrenologists* will read this Pamphlet with much pleasure; and the Phrenologists likewise, with some degree of interest.

Observations on Strictures of the Rectum, and other Affections of the Intestinal Canal: including Hæmorrhoidal Tumours (called Piles,) accompanied with the Mode of Treatment by the late W. White, Esq. By Samuel White, Member of the Royal College of Surgeons, London. Fifth Edition.—8vo. pp. 110, and Plates.

First Steps to Botany, intended as popular Illustrations of the Science, leading to its study as a Branch of general Education. By James L. Drummond, M. D. Professor of Anatomy and Physiology in the Belfast Academical Institution.

In 4 vols. 8vo. The English Flora, by Sir James Smith, M. D. F. R. S. President of the Linnean Society, &c. &c.

## LITERARY INTELLIGENCE.

Dr. M'Cormack, of Belfast, announces for immediate publication, an Essay on the Causes and Cure of Stammering and Impediment of Speech. Dr. M'Cormack, has, it appears, discovered a method of removing these troublesome affections in a few days, and often in a few hours.

Shortly will be published, "An Introduction to the Comparative Anatomy of the British Vertebrated Animals," with Plates. By J. F. South, Lecturer on Human and Comparative Anatomy, St. Thomas's Hospital.

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From the Transactions of the Medico-Chirurgical Society of Edinburgh.

**ADDITIONAL CASES AND OBSERVATIONS ILLUSTRATING THE ORIGIN OF TUBERCLES.** By W. P. ALISON, M. D., F. R. S. E., &c. and Joint Professor of the Institutes of Medicine in the University of Edinburgh.

The inquiries on the subject of scrofulous diseases, of which I formerly laid an account before the Society, were directed chiefly to the elucidation of the two following questions: *1st*, Whether the scrofulous diathesis, or disposition to scrofulous disease, unfortunately so prevalent in our climate, is to be regarded as the result of the climate alone; or whether other and more remediable circumstances in the mode of life of those who become so affected, contribute generally and powerfully to its production; and *2dly*, Whether the deposition of tubercles, so often the original cause of danger and death in scrofulous diseases, is to be regarded, in any case, or in any considerable number of cases, as an effect of inflammatory action.

On the first of these questions, I have since obtained information from various quarters, tending to confirm me in the opinion, that in regard to the tendency to scrofulous diseases, and particularly to those in which tubercles are formed, there is a greater difference between the inhabitants of large towns and of the country, than between the inhabitants of the warm and cold climates, at least of Europe; and therefore, that the general prevalence of the scrofulous diathesis is to be ascribed, rather to the modes of life which an advanced and artificial state of society implies, than to the circumstances of climate. But the information which I have collected on this head, is not yet so ample and satisfactory as I hope to be able to make it, before submitting it to the Society.

In regard to the second point, I should wish it to be remembered, that my object was, not to explain the whole history, and all the possible causes of tubercles, but only to investigate the question, which seemed of the greatest practical importance, whether or not inflammatory action has, in any circumstances, the power to produce them? And on this subject I cannot help remarking with satisfaction,

that although, at the time when I wrote my former paper, the most celebrated authors and teachers in the school of Paris, who had studied this subject minutely, Bayle, Laennec, Rostan, Louis, Velpeau, &c. were disposed to allow little or no influence to inflammation in the production of tubercles; yet those of the French pathologists who have attended most minutely to the subject since that time, particularly Professors Andral and Cruveilhier, have adopted an opinion almost identical with that which I endeavoured to explain.\*

The testimony of Andral is the more valuable, as his previous opinion in regard to the formation of tubercles appears to have been nearly the same as that of Laennec. The following sentence in the third volume of his *Clinique Medicale* expresses very nearly the opinion which my own observations have led me to form, regarding the connection of tubercular deposition which increased vascular action. "If the disposition to the formation of this new product is very strong, then the slightest local congestion of blood is sufficient to give occasion to it; wherever such congestion takes place, the same product appears; or what is called the tubercular diathesis is produced. If this disposition is less strong, it is requisite for the formation of a tubercle that the congestion of blood should be so considerable, and so permanent, as to amount to inflammation. But, when there exists no such predisposition, the most intense, and the longest continued inflammation, will not produce a tubercle."† For the last clause of this sentence, I should be disposed to substitute, "the kind of inflammation which is most generally excited has no tendency to produce tubercles." He remarks, in another place, that the condition which seems most conducive to the deposition of tubercular matter from inflammation, is not its intensity, but its long duration.

\* See Andral *Clinique Medicale*, t. iii. p. 13, *et seq.*; also an excellent "Essai sur les Tubercles," by a gentleman who has distinguished himself by his attention to the subject both at this school and at Paris, M. Lombard; and Cruveilhier's paper in the *Bibliothèque Medicale*, September 1826.

† T. iii. p. 24.

I shall now take the liberty of directing the attention of the Society to the facts which have occurred to me, both in the way of reading and of observation, since my former paper was read, and which seem to me to concur in establishing the proposition, that in certain constitutions, inflammation, acute or chronic, but most generally chronic, does frequently and directly lead to the deposition of tubercles.

I think it an important point, in the outset of this inquiry, and one which we may consider established by the researches of some of the pathologists of Paris, that, in so far as the most minute anatomical observations can inform us, tubercles are very seldom found in the bodies of children who are still born, or die very shortly after birth. On this point I trust chiefly to the statements of Dr. Denis, lately *Eleve Interne* at the *Enfans Trouvés* at Paris. In the "*Recherches d'Anatomie et de Physiologie Pathologiques sur plusieurs maladies des Enfants nouveau nés*" of this author, we have a detailed account of 139 dissections of still-born, or very young children, which took place in the Hospice, under the eye of the author, in 1823; and after these extensive opportunities of observation, and after having attended minutely to the statements of former observers, he was persuaded, that where they had described tubercles in the lungs in the foetal state, or very soon after birth, they had been mistaken; and thought himself justified in asserting that, "*On n'a pas encore trouvé jusqu'ici de tubercules pulmonaires avant les premiers mois que suivent la naissance; on commence seulement à les rencontrer dans des enfans de 5 ou 6 mois.*"\* He then goes on to state (what agrees with my own observations,) that even at this last age "*les tubercules ne sont que rarement sous forme miliare chez les enfans; leur substance infiltre ordinairement une masse considérable des poumons, ou seulement les glandes bronchiques.*" And in speaking of the abdominal diseases of new born children, he states equally decidedly, "*Jamais, chez les nouveau nés, et meme rarement avant la première dentition, l'engorgement (des corps lymphatiques) ne se termine par une induration remarquable, par suppuration, ou par une dégénérescence tuberculeuse. Le Carreau (Tabes mesenterica) est une maladie qu'on doit rayer du cadre des affections des enfans en bas age.*"†

To the same purpose I find it stated by Velpeau, that both he and Breschet, (surgeon to the *Enfans Trouvés* at Paris,) had frequently sought for tubercles in the fœtus, and never found them; and that though other observers, particularly Orfila and West, had seen them, it was only in a small number of cases.‡

We must, therefore, as I apprehend, necessarily suppose, that in by far the greater number of the very numerous cases in which tubercles are found in bodies of young children, the diseased actions by which they were form-

ed had originated after birth, and seldom sooner than some months after birth,—parents transmitting to their offspring only the tendency to this kind of diseased action, and very seldom the actual disease. I would next recall to the recollection of the Society the observation of Magendie, that in those cases in which he had detected tubercles of the smallest size, and apparently in the earliest stage of the bodies of young children, they were surrounded by circumscribed vascularity. This I have myself observed, not uniformly, but certainly in a great many cases. I formerly stated likewise, that in cases of young children, which have come under my own observation, and where I was accurately informed of the whole progress of the symptoms, when tubercles have been found in unusual numbers, and been apparently the cause of unusually rapid emaciation, and early death, the first and chief symptoms of the disease have been decidedly those of inflammation, from which the patients never recovered, and of the consequences of which, judging from the symptoms, it was obvious that they died.

The two following cases, which have occurred to me since my former paper was printed, seem to me striking illustrations of this general fact.

1. Allan Mackintosh, a child aged two years and two months, living in a confined and ill-aired part of the town, had nevertheless been healthy from his birth,—had not had any of the contagious diseases of children, or any sickness of a day's duration, and had never had a cough, until the night of the 12th May 1824, when, after having been taken to some distance from town, and exposed to cold, he was seized with heat, restlessness, cough, and short breathing. I saw him on the 13th, after he had been prescribed for by one of the pupils attending the Dispensary. His pulse was then very quick and full; his face flushed, skin hot, and breathing much oppressed. Four leeches were applied that evening, and again the next day; these bled freely, and the strength of his pulse and the heat of his skin were much diminished. The pneumonic symptoms continuing, however, in a less degree, leeches were again applied twice within a few days after this date; he also took laxatives, and on several days repeated doses of a solution of tartar emetic. About the end of the first week of his disease, the pectoral symptoms had abated very much, but his breathing continued short, though not difficult, and he had still cough, not severe, and generally dry. His pulse was still very quick, and rather feeble. His bowels at this time were rather obstinately costive for a few days, and appeared to be pained at times. He had also slight spasms, and a tendency to coma, so as to excite suspicion of hydrocephalus, but these symptoms gradually went off, and the alvine evacuations became nearly natural. After this he had frequent irregular fits of chilliness, succeeded by heat and thirst, but without sweating. His tongue was generally rather dry, his appetite never returned, and he be-

\* Op. cit. † Op. cit. p. 118.

‡ Thesis ad Aggreg. p. 10.

came weak and emaciated. In the beginning of the fourth week of his illness, his breathing became more short and difficult, and leeches were again applied, without any good effect, the dyspnœa continuing till his death, which took place on the 8th of June, the twenty-seventh day of his disease.

On dissection, the left lung appeared much condensed, and had on its surface many flakes of lymph, of a light yellowish colour; and disposed in irregular forms. When cut into, a part of its condensed substance had the common appearance of hepatization, but the greater part consisted of yellowish matter, partly diffused or "infiltré" through the substance of the lung, partly disposed in roundish tubercular masses, and partly also in long irregular flakes, which had exactly the same form, and were composed apparently of exactly the same substance, as those already noticed on the surface of the pleura.

Several of these tubercles, of the different forms now stated, were softened, and contained pus in their centre, and some of them were nearly emptied.

The right lung appeared unusually voluminous; there was some partial hepatization, but with hardly any deposition of tubercles in its lower lobe.

The other viscera of the thorax and abdomen were healthy.

2. Isabella Leslie, æt. nine months, though of a family in which there had been one death from consumption, was a healthy and lively child, and never had a cough until the 20th March 1826, when she first became feverish, after having been exposed to the contagion of pertussis. She died on the 5th April, after seventeen days of illness. I did not see the child until some days after the illness began, and the symptoms which appeared to me at first the most urgent, were a tendency to stupor, with retching; the skin was hot, and the pulse frequent, but rather feeble. The child was leeches and purged, and the comatose tendency abated, but the cough then became frequent, with short hurried breathing, and soon assumed the peculiar character of whooping cough. The heat of skin continued, though with remissions, and with more evidence of debility than is usual in the inflammatory stage of pertussis; on which account evacuations were not carried to such an extent as perhaps they ought to have been. A few days before death the cough again abated, the dyspnœa continuing, with feeble pulse, and occasional spasms.

On dissection, the upper lobes of both lungs were found somewhat emphysematous, and a considerable part of the middle and lower lobes of both was found condensed, so as not to crepitate under the knife. The effused matter producing this condensation was in some places of a reddish colour, but the greater part of it was of a grayish white. In general no distinct line of separation could be traced between the portions that had these different colours. The whitish matter was disposed partly in irregular diffused masses, but partly

also, both under the pleura and in the substance of the lungs, in little nodules, more or less distinctly circumscribed, and which had just the usual appearance of incipient or miliary tubercles. Some of these nodules had a yellowish colour, and at one spot there was a small cavity containing yellow pus. In a few places there was a little deposition of lymph, of irregular form, on the outside of the pleura; and this matter, on minute examination, appeared identical with that which had the form and appearance of the incipient tubercles beneath the pleura.

The only other morbid appearance was a quantity of frothy mucus in the bronchia.

I shall only observe of these cases, that they appear to me to comprise *both* the circumstances which I formerly stated as indicative of the dependence of tubercles in many cases for their existence, or at least for their chief properties, or inflammatory action. *1st*, The patients, previously quite healthy, suffered an attack of acute inflammation, from a known cause, very recently before death, and never recovered from that attack, but died manifestly of its consequences. A large deposition of tubercular matter was the chief appearance on dissection, and the disorganization produced in this way was so extensive, that it was quite impossible to suppose it to have existed previously to the inflammatory attack, when the patients appeared to be in perfect health. *2dly*, The tubercles appeared closely connected with, and graduated insensibly into, the usual and acknowledged effects of inflammation. The tubercular matter which was diffused through the substance of the lungs, passed by insensible degrees, as I have seen in many other cases, into the adjoining hepatized induration; and the lines of tubercular matter in the substance of the lungs appeared so precisely similar to the contiguous flakes of lymph on the pleura, that I think no one could reasonably ascribe to them a separate and wholly different origin; while these last, again, had so much the common appearance of inflammatory exudation on that surface, that I think before the lung was laid open, no one could in either case have hesitated about calling them the effects of inflammation.

In the following case, which is almost precisely similar to one formerly communicated to the Society, the symptoms were those of acute hydrocephalus, and the appearances found were such as indicated clearly inflammation of the brain and pia-mater. The effusion on this last, however, though composed every where of the same *substance*, had in some points the usual *diffused form*, and in others the *form of tubercles*.

Pitcairn Smith, æt. thirteen months, December 1825. Had had running from the ears for a long time, and for three months an ulcer behind one ear, of scrofulous appearance, but without disease of the bone, and was always subject to slight spasms. Three weeks before death he became feverish, with screaming and vomiting, and soon after nearly comatose, with spasms; his bowels were rather loose,

the pupils not dilated, the pulse quick and small. He was in this state when I first saw him; the coma abated, and recurred repeatedly before death.

On dissection, the ventricles of the brain were found much distended with serum. There were two large tubercles, the largest of the size of a hazel-nut, in the medullary substance of the hemispheres. The pia-mater lining the base of the brain was very vascular, and on this vascular membrane, in front of the tuber annulare, were two patches of lymph, the one having the usual diffused form, and as large as a half-crown, and the other somewhat larger, and of precisely similar appearance, except that it was for the most part composed of little tubercular masses, set close together. The substance of the brain, just above this last, was red and softened.

The gradual and imperceptible transition of the matter constituting tubercles, into that which is clearly the effect of inflammation, is, of all the phenomena observable on dissection, that which appears to furnish the most direct evidence of the formation of tubercles by inflammatory action. I have no doubt of being able to form a series of preparations, in which a gradual transition shall be obvious, from the true hepatized condensation of the lung, through the intermediate changes of structure to which Laennec has given the name of gray hepatization, and of tubercular infiltration, to that kind of disorganization which consists for the most part of tubercles; and I am much confirmed in my opinion as to the close analogy of these structures, by finding that the researches of Andral have led him to the belief, that the appearance to which Laennec gave the name of "*Tubercules infiltrés*," is always to be regarded as the effect of a chronic inflammation.\*

One important link in this series of preparations must consist of cases of the kind of tubercles called *granular* by Andral, and minutely described by him in the 3d volume of his *Clinique Medicale*, (p. 4. *et seq.*) These have just the external appearance and size of pretty large tubercles, but, when cut into, appear merely nodules of hepatized induration, confined to minute lobules of pulmonary substance. In a case which I attended lately, one lung was found generally studded with tubercles of this description, while at its upper part only there was a pretty large irregular excavation, containing some clots of blood. The patient had died from repeated fits of hæmoptysis. This case illustrates the analogy of this to the more common form of tubercles.

I have seen several fatal cases of adults, in which almost the only morbid appearance found on dissection in the lungs was a deposition of common tubercles, so extensive as to have produced death before they had passed the first stage of their progress, and when their appearance was that called miliary; and in these cases both the external causes and the symptoms of the disease appeared to me to be

decidedly those of inflammation, less acute, indeed, than where the hepatization of the lungs has been the chief appearance on dissection, but nevertheless abundantly well marked.

The most striking instance of this kind, of which I have a full account, is the following.

George Gordon, æt. sixteen, of delicate habit, and somewhat emaciated, was admitted into the Clinical Ward on the 6th of January 1823, complaining of frequent, severe, hollow cough, with copious, rather difficult, puriform expectoration; of pain under the sternum, and difficulty of lying on the right side; and of very short and difficult breathing. His pulse was 140, soft and compressible; his tongue of a florid red, with much thirst; his skin hot and moist; his face flushed, but lips livid, and countenance very anxious.

He described having had, from exposure to cold, one morning before breakfast, a distinct febrile attack, with rigours, five weeks before admission; since which time the cough and shortness of breath, with thirst and heat of skin, especially at night, had continued and increased, particularly during the last few days; he had struggled, nevertheless, to continue his employment as a printer, and had been in the printing-office on the 3d of January, the fourth day before admission.

After being laid in bed, his pulse felt somewhat firmer, and the heat of his skin was 102°; he was bled at the arm, and became faint after 3x had been taken, without any improvement on his breathing being perceptible. A blister was applied, and rose well without any better effect. An expectorant mixture, ether draughts, and the digitalis, with a little wine, were used without benefit. He became delirious on the day after admission; his breathing continuing very quick and short, while his pulse became feebler, and skin cool; and he died on the evening of the 9th.

We found that he had been a patient in the hospital under Dr. Spens, from the 20th October till the 11th November; he had complained, on admission, of slight febrile symptoms, with cough and pain under the sternum, of five days' standing, and ascribed to cold. His pulse was 86. He used a blister and purgatives. On the sixth day after admission, he was reported to have no pain of breast, and very little cough; on the eighth, to be free from complaint; and on the eleventh he was dismissed cured. His friends said, that previous to this time he had not been subject to any pectoral symptom whatever.

In mentioning his case at lecture, the day before his death, I stated, that from his emaciated and enfeebled appearance, from the duration of the symptoms, the peculiar hollow cough, the puriform expectoration, and the clean florid tongue, I had no doubt of his being phthisical, but that the dyspnoea was much more urgent than usual, at that or indeed at any period, during the ordinary course of phthisis; that the history of the case, and the symptoms we saw seemed to indicate, that this depended on an unusual degree of pneumonic

\* See Lombard's Thesis, p. 32.

inflammation being *combined* with the changes in the lungs that are peculiar to the first stage of phthisis; and that there was thus an injury done to the texture of the lungs, which must be inevitably and rapidly fatal; much sooner than the ordinary course of phthisis could be expected to be.

On dissection, there was a very slight deposition of lymph on the pleura pulmonalis of both lungs. The lungs themselves appeared externally to be healthy, and crepitated almost universally when cut; but they were studied throughout with an immense number of very small tubercles, exactly corresponding to Laennec's description of "*Tubercules Miliaries.*" I had never before seen so very general a deposition of these bodies, in a state so uniform, throughout the lungs. In the upper parts of the lungs only, they had the yellowish colour, opacity, and hardness, of the "*Tubercules Crus;*" and here some of them were nearly as large as peas. Only one was observed in a state of suppuration.

The proper pulmonary substance was of a somewhat redder colour than usual, and more serum than usual ran from it when cut; and in a few places, there was a little condensation or hepatization of it; but this was to a very small extent only.

I consider this case a very important addition to the set of cases adduced in my former paper, in which well marked pneumonic symptoms existed before death, and were never recovered from, and in which tubercles were the chief or only morbid appearances found on dissection.

We know, that on the 11th of November, this lad appeared quite free from complaint, having been kept in the hospital for some days before that time, after the catarrhal symptoms, for which he had come in, had disappeared. His pulse was then natural; and if his breathing had been difficult, or even peculiarly hurried, it must have been remarked. A fortnight after his dismissal he caught fresh cold; the complaint began with distinct rigours, followed by cough; this advanced insidiously for a time, but became attended with pain of breast, thirst, and other febrile symptoms, and then with dyspnoea; at the end of five weeks these symptoms were very urgent; his pulse 140, heat 102; his fever had no hectic exacerbations or remissions; he died before the end of the sixth week; and had his body not been opened, I think the disease could have received no other name than a neglected pneumonia, occurring, no doubt, in a phthisical habit.

On dissection, however, the common unequivocal appearances of inflammation were to a small extent only; but the lungs appeared disorganized to a great extent, by tubercular deposition. These tubercles were so *very numerous*, that I think we can hardly suppose that they could be deposited, or could exist, in the lungs, without materially impeding their functions; they were (with the small exception already noticed) *so exactly in the same state of progress*, that their deposition must have been simultaneous; they were so small that it must

have been recent; and besides, they were of the kind which the most experienced observers regard as the first stage of their formation. To me the influence seems almost irresistible, that they had been formed during the five or six weeks preceding his admission; if so, then we know that their formation had been preceded and attended by the usual symptoms of pulmonary inflammation, occurring in a scrofulous habit, and advancing; probably on that account, more slowly than is usual.

If we reject this opinion, in regard to the origin of the tubercles, and suppose them to have existed in the lungs before the fatal illness commenced, then we must admit that *nothing* appeared in these lungs which could explain the urgent dyspnoea, which was the chief symptom of that illness, and cause of the death of the patient; the degree of hepatization of the lungs, in this case, having been quite trifling, and the serous effusion in the lungs not more than is often seen when the respiration has hardly been affected before death.

It would appear, that tubercles seldom form in the lungs in so great numbers at once, as to be quickly fatal; and to this we must ascribe the rarity of cases of this kind, where there are so many tubercles, without any vomicae or ulcers, in writings on phthisis. The case which I have seen described, that comes nearest to the above, in the symptoms and appearances, is one recorded by Bayle,—the second case that he has given at length. A postillion, æt. 24, previously quite healthy, was seized, on the 17th November, with a dry cough. Some days after he had a fall from his horse, and his breast was much hurt. From this time his breathing became short, and the cough was aggravated; these symptoms increased; he became hoarse; his legs became slightly swelled; his appetite continued, but he was unable for his employment. On the 7th December he was taken into the hospital; his breathing was then very short and difficult; he had much cough, with frothy expectoration; his pulse was very small, frequent and irregular; his tongue red, rather dry in the centre, but covered with whitish mucus on the edges; his voice almost gone, and countenance expressive of exhaustion; he died on the ninth, "*dans un accès de suffocation;*" *i. e.* the twenty-second day after the commencement of the cough. He did not appear at all emaciated, and had very slight anasarca.

On dissection, the lungs were not adhering, and appeared healthy externally. When cut into, it is said merely that the substance of both appeared red, and loaded with innumerable whitish semitransparent grains, all about the size of a pin's head, more closely set together in the upper than in the lower lobes. This was the only morbid appearance.

It is obvious that the remarks I have made on the case of Gordon apply equally to this case; but my chief reason for quoting it is, to point out the difficulty which attends the view of the case which is taken by Bayle himself, who, believing that tubercles are altogether

independent of inflammation, suppose these to have existed before even the cough had commenced. Twenty-three days before his death, says he, no medical man, examining this young man, who then enjoyed the most brilliant health, could have suspected that he was consumptive. Yet phthisis already existed in him, and *was already incurable*. Had he not met with the fall, the true phthisical symptoms, depending on the progressive changes of the tubercles already existing, would soon have appeared, and might have been long of running their course. But the fall had given a shock to the lungs, which *deranged their functions, and produced death*. It is needless to animadvert on the vagueness of this phraseology; but it is important to observe, that Bayle had no better account to give of the cause of death in this case. And indeed, it is obvious that if we suppose, with him, that tubercles are never an effect of inflammation, we must suppose, in this case, *first*, that the tubercles caused no symptoms; and, *secondly*, that the cause of death left no marks; that the lungs were loaded with innumerable tubercles at the time when the young man appeared in the enjoyment of the most brilliant health; and that the affection of the lungs which supervened on the injury, and went on increasing, till it was fatal, three weeks thereafter, was merely a disturbance of the functions, not an alteration of the structure of the lungs; the only alteration of structure that was found, having been of the kind which this theory does not allow to be a product of inflammation, and cannot, therefore, refer to the agency of a cause of inflammation.

My belief, on the other hand, not grounded on these cases, but certainly strongly supported by its application to them, is, that an inflammatory action in the lungs, denoted first by the sudden attack of cough on the 17th of November, and afterwards aggravated by the injury of the chest, was the cause of the original deposition of the tubercles, or at least of their rapid progress and increase, within three weeks, from a state in which they were compatible with *the most brilliant health*, to a state in which they so far interfered with the functions of the lungs, as to cause *death by dyspnœa*. Even if we adopt the latter conclusion only, the question, whether the rudiments of the tubercles existed prior to the fatal illness, or whether there only existed the constitutional disposition to their formation, seems to be divested of all practical importance.

Dr. Denis records a striking case of very general tubercular deposition, chiefly found in the first stage at the time of death, in the lungs and bronchial glands, and on the pleura, peritoneum, and omentum, in a young child which had been exposed on the Boulevards of Paris in very cold weather, and was taken into the *Enfans Trouvés*, in a state of extreme depression from cold, seven weeks before death;\* and I think no one can read his reflections on

the case, without remarking how much he is embarrassed in drawing his conclusions as to its pathology, by the preconceived notion that tubercles could not proceed from inflammation; and therefore, that the great mass of them there found could not be ascribed to that barbarous exposure.

The affections of the intestines and mesenteric glands, which are so apt to occur in the course of phthisis, appear to me to furnish frequently unequivocal examples of tubercular deposition resulting, I do not say from active inflammation, but at least from increased vascular action and congestion of blood, which may be properly called chronic inflammation.

It has been indeed supposed by Laennec, that the colliquative diarrhœa of phthisis depends in general on the existence of tubercles in the intestines, and of ulcerations formed in these tubercles; and it is certain, that in those patients who are affected with this diarrhœa long before death, these tubercles and ulcers in the intestines, as well as more or less of the tubercular degeneration of the mesenteric glands, are very generally found. But I have had several opportunities of observing, in cases where the colliquative diarrhœa, although well marked, has been of shorter duration before death, that the mucous membrane of the intestines has presented no morbid appearance, except general vascularity; and in other cases there have been only a few tubercles at the most vascular points; from which I conclude that the numerous tubercles and ulcers found in those cases where the diarrhœa has lasted long, are the consequence, not the cause, of that increased vascular action of the part, which produces the diarrhœa. And on examining the mesenteric glands in persons who have died of phthisis after various degrees of the colliquative diarrhœa, or the different mesenteric glands in the same subject, it has appeared to me obvious that the first change which takes place in them, is an increase of size and of vascularity; that the first tubercular deposition occurs at minute points in glands already thus altered, and that it is only by the gradual extension of these deposits, in glands larger and more vascular than natural, that the conversion of the glandular substance into the scrofulous cheesy matter is effected. The first part of these changes in the abdomen, in phthisical persons, is I think generally attended with pain, though not constant, and not always severe.

Another set of cases to which I would beg to refer, as furnishing strong evidence on this subject, consists of those where a disease, which ultimately takes the form, and exhibits the appearance, of tubercles, appears manifestly to have been excited by external injury. I have seen several examples of persons certainly previously healthy and vigorous, who have been subject to shotness of breath, and other pectoral symptoms, only from the time of their receiving such injuries on the chest, as confined them to bed for some days together; and in whom the disease has ultimately turned out to be tubercular deposition, chiefly or solely situated in the immediate neighbourhood of

\* Op. Cit. p. 473.

the parts injured. The following case appeared to me the most striking of this kind that I have seen.

A boy, æt. twelve, a year before his death was severely injured on the *lower part of the left side of the chest* by a fall. According to the account of his parents, he had been quite free from any pectoral complaint before that time, but had been confined to bed for some weeks immediately after the injury; had never recovered his strength, and his breathing had always been short, insomuch that he had seldom been out of doors after that time. He died of confluent small-pox, without suffering nearly so much from dyspnœa in the course of that disease as many others do. On dissection the *lower lobe of the left lung* was found adhering to the pleura costalis, and completely condensed, chiefly by red hepatization, but partly by circumscribed tubercles, which were of the yellow colour and cheesy consistence, several of pretty large size, and one of an irregular form nearly as large as a hazel-nut. There were no other tubercles, and no other morbid appearances in either lung.

From the history of this case I think we cannot doubt that the partial condensation of the lungs had resulted, for the most part, from the inflammation that had been excited by the injury. And if we do not regard the tubercles as likewise the effect of the inflammation which had condensed the pulmonary tissue around them, we must make two suppositions, both highly improbable; *first*, that the tubercles had pre-existed in that part of the lung only which is known to be least subject to their deposition; and, *secondly*, that this unlucky portion of the lung, which was already exclusively the seat of tubercles, had been likewise that which alone sustained injury from the fall.

The very great frequency of phthisis in masons, in this country, has been already remarked, and it was stated that, in many of these cases, occurring at a somewhat advanced period of life, less tubercular disease is found on dissection than in younger persons who die of phthisis. Since that time I have met with few cases in which more or less of tubercles were not found; and I have seen several fatal cases, under the age of forty, where there was a very great number of them, in men originally of robust habit of body, and who were free from pectoral complaints up to the age of thirty. If we suppose, as I think we must do, that the phthisis of masons is chiefly to be ascribed to the irritation of the particles of sand inhaled, we may regard the undoubtedly peculiar prevalence of the disease among them as a proof that long continued irritation by foreign bodies may occasion the development of tubercles in many, in whom they would not otherwise have appeared.

The question, whether tubercles can be excited by external injury, and consequent inflammation, is obviously susceptible of illustration from experiment; and the experiments of Cruveilhier on this point, published in the *Nouvelle Bibliotheque Medicale* for September 1826, are so satisfactory as to leave no

doubt on my mind of the correctness of the conclusions above drawn from the different pathological appearances I have described. He injected mercury into the femoral artery of a dog, which was killed some days after this had been done. He then found in the limb thousands of little miliary tubercles, quite regularly formed, and each surrounding a very minute globule of the mercury. He injected, also, by an opening in the windpipe, ℥ij of mercury into the lungs of a dog. The greater part was rejected by coughing: the animal became apparently phthisical, and died emaciated at the end of a month. The lungs were found "*farcis de tubercules isolés et aglomerés, ayant tous les caracteres des tubercules miliaries.*"

Similar experiments were performed in the Royal Infirmary here, in the beginning of this winter, by my friend Dr. J. P. Kay (author of the paper on the Physiology of Respiration, which was read to this Society, and afterwards published in the *Medical and Surgical Journal* for January last;)+ and the results of some of these I can now exhibit to the Society.

A small globule of mercury was introduced into the tracheæ of each of the rabbits which were the subjects of the experiments, by small incisions, which soon healed. This produced at first much coughing, which occasionally returned afterwards; but the animals did not appear much incommoded, and took food well; their breathing, however, appearing rather hurried.

The first rabbit was killed eight days after the introduction of the mercury; and it is singular that the appearance of clusters of tubercles was more distinct in this than in any of the others. These lungs are preserved, but the colour of the tubercles does not now contrast with that of the pulmonary substance as before the lungs were put in spirits, and several of the clusters were destroyed in examining them minutely. The appearance of several of the clusters was so exactly that of tubercles in their early stage, that my learned colleague Dr. Monro, and several other friends, gave them that name without hesitation, before they were aware how they were produced; but on cutting into them, each contained in its centre a minute globule of mercury.

It is to be observed farther, that, in several parts of these lungs, there was partial hepatization.

The appearances found in the other rabbits are accurately described by Dr. Kay in the following paper.

"The *second* rabbit, after appearing drowsy and oppressed for a day, was found dead eight days after the operation.

"The upper lobe of the left lung was discovered hepatized in the whole of its anterior border, through which were scattered small granular bodies, of a yellowish colour, each con-

\* *Nouv. Bibl. Med.* September 1826.

† *Vide Journal of Foreign Medicine*, Vol. I. p. 481.

taining in its centre an extremely small globule of mercury. They had all the external characters of ordinary tubercles. In the centre of this lobe existed a cavity, containing a soft caseous substance, surrounded by a firm membranous cyst. It was about two and a half lines broad, and three in length. Large globules of mercury escaped from this cavity. In the posterior part of this lobe was an extremely small cyst, containing neither tubercular matter nor mercurial globule.

"The lower lobe of the left lung was also hepatized in the whole of the anterior border, through which numerous small, granular, and apparently tubercular bodies were deposited, containing generally a small globule of quicksilver in their interior. The superior lobe of the right lung was, in some portions, hepatized. A grayish, and apparently tubercular substance was infiltrated into portions of the pulmonary texture. In the anterior part of this lung, existed a large encysted cavity, filled with a soft, yellowish, granular mass, of a caseous consistence, resembling the matter of softened tubercle. The cyst was distinct, apparently well organized, being of firm texture; and the cavity was capable of containing a large pea. In the lower lobe of this lung, tubercular deposition also existed, surrounded by hepatized structure. Several bronchiæ were much dilated, and a similar substance was deposited round their parietes.

"The other rabbits were killed four weeks after the operation.

"*Third Rabbit.*—In the upper lobe of the left lung, a large globule of mercury was found imbedded in a quantity of soft and grayish lymph, surrounded by a distinct cyst. Many globules of mercury were found scattered in the lungs, without either hepatization or deposition of lymph. The lung, however, appeared emphysematous, and some dilated bronchiæ were observed. In the right lung the appearances were similar. The globules generally were found in the thin border of the lung, and appeared to have excited little inflammation there. Some partial condensation, and a few granular bodies resembling tubercles, were observed.

"*Fourth Rabbit.*—Much hepatization had been produced in this lung. Dilated bronchiæ were observed in many portions, and in the extreme thin border of the tissue many globules of mercury existed, unsurrounded by hepatized structure or lymph, but apparently contained in enlarged cells. In the centre of the lung, as before, they were, in some cases, surrounded by lymph, which had the form of tubercles.

"*Fifth Rabbit.*—The general appearances in this lung resembled those described in the others; but, at the extreme thin border of the lung, a row of globules of mercury existed, not simply contained in air-cells, but surrounded each by a very evident thin deposition of lymph."

The tubercular appearances in these lungs (which were always slighter than in the first,) were also much destroyed in examining them, and two small portions only have appeared

worth preserving; the one showing an ulcerated cavity, with a distinct cyst resembling that of a tubercular excavation; the other showing a row of small tubercles (each no doubt containing a little globule,) which have formed along the edge of one lobe, apparently in the cellular substance just beneath the pleura. In this situation the thickness of the lymph surrounding the globules is less than where similar appearances were found in the central parts of the lungs.

I consider the results of these experiments (coinciding as they do with those related above,) as sufficient proofs, that, by the inflammation which is generated by the irritation of foreign substances, depositions of lymph may be excited in the lungs of these animals, which present all the external marks, and which appear, in some instances, to run the usual course, of tubercles.

If we consider it established that masons and others, who are in the habit of inspiring irritating particles, are much more subject than other persons to the deposition of precisely similar matters in their lungs, I think we cannot doubt that the deposition is produced in the same manner; and we know that the matter there deposited produces the ordinary symptoms, and undergoes in general the ordinary changes of phthisis.

It may be said, that if this kind of irritation, acting on the lungs of healthy rabbits, is supposed to produce a deposition of tubercles, resembling those which we distinguish in the human body as scrofulous, we depart from the doctrine generally received among physicians, and illustrated in the former part of this paper, that a peculiar general scrofulous diathesis is much concerned in the production of tubercles in the human body.

I would answer, 1st, That if it be true, as matter of fact, that mechanical irritation of the lungs will produce deposits in the lungs, not differing in appearance from scrofulous tubercles in their early stages, we must not set aside that fact, because it does not accord with our preconceived notions of the pathology of the diseases, in which similar deposits takes place in the living body.

But, 2dly, It was stated already, as the result of the observations of Andral, that the conditions which appear most requisite, in order that inflammation may generate tubercles in the living body, are the *long duration* and *slight intensity* of that inflammation. It is highly probable that the scrofulous diathesis disposes inflammation to terminate by tubercular deposition, simply by giving to it these characters,—keeping it up long, and not permitting it to rise high.

Now it is very easy to conceive, that the continued irritation of a minute globule of mercury imbedding itself in a portion of pulmonary substance, or the repeated irritations of particles of sand inhaled, may furnish just the same condition to the inflammation which they excite,—as is given in other cases by the peculiar strumous diathesis,—rendering the inflammation long continued, but of slight inten-

sity. When this cause is habitually applied for a long time, and during youth, to the human species, as in masons, I believe that very few cases exist, even of constitutions previously strong and healthy, in which more or less of tubercular deposition is not excited.

It is to be observed, that the appearance of tubercles is by no means the only change which was determined in the lungs of these rabbits, by the irritation of the mercury. In each of the cases some portions of the pulmonary substance were found hepatized,—probably parts which had been irritated by larger masses of the mercury than those which caused the deposition in the tubercular form. Such larger masses were in fact found, enclosed in enlarged bronchiæ, in some of these hepatized portions, but in general they appeared to have escaped into the cellular substance close on the pleura.

I would farther observe here, that in asserting that inflammation does in certain circumstances cause deposition of matter, which takes the tubercular form, I do not assert that such products of inflammation must always necessarily run the course that is commonly followed by tubercles. It has been rendered almost certain by Dr. Baron and others, and I have seen various cases illustrating the observation, that the subsequent course of substances, which in their commencement have all the characters of common tubercles in the lungs, may vary remarkably. I have no doubt that the course of tubercles, once deposited, may vary according to the age and constitution of the patient, according to the diseases which may subsequently attack him, and according to the texture in which they have been formed; and on another occasion I hope to be able to show, that the pathology of the earlier stages of diseases, generally regarded as very different from scrofulous affections, does not differ materially from that of scrofulous tubercles deposited in certain circumstances of inflammatory action.

In the mean time, I think the facts stated in this paper, and in the latter part of my former one, may be regarded as sufficient evidence of the proposition, that scrofulous tubercles may be, and often are, deposited in consequence of inflammatory action; and therefore, that as, on the one hand, scrofulous diseases may be in many cases prevented by applying the *tonic regimen* to persons of feeble constitution, but not yet affected with actual disease; so, on the other, they may also be frequently prevented by the early and prudent use of the *antiphlogistic remedies* in those in whom the slight inflammatory complaints so often preceding them have already appeared.

This general conclusion does not differ from that which I believe the greater number of practitioners have been induced to form, from their own practical observation; but it seems to me of great importance to have attained to it by pathological inquiries, founded on fatal cases,—because, until the pathological question as to the frequent dependence of tubercles on inflammation is determined, no observa-

tions on the effect of the antiphlogistic remedies, where tubercular disease is apprehended,—if made on cases that terminate favourably,—can be held to be conclusive on the subject.

From the London Medical Gazette.

#### MEMOIR ON A NEW METHOD OF TREATING ARTIFICIAL ANUS. By BARON DUPUYTREN.\*

It is with just sentiments of diffidence that I proceed to speak of artificial anus, a malady at once loathsome and dangerous, which condemns those who have the misfortune to be afflicted with it, to give up the world, burdensome to themselves and others; and which makes them languish in misery, or carries them off from slow and painful marasmus.

Artificial anus has been generally looked upon as incurable; but I trust that, after the details I am about to enter upon, it may hereafter be ranked among those maladies which admit of relief from art, without much difficulty or danger.

It was requisite that I should first endeavour to ascertain the exact anatomical condition of the parts in this affection. In the natural state, the aliments traverse, in a given time, the whole length of the intestinal canal, and undergo, in each of its parts, a series of different changes, as the result of which they furnish to the absorbents the elements of nutrition; after this, the residue passes on towards the anus, and is expelled by actions, which are under the control of volition.

The length of time which they remain, the space which they traverse, the successive elaborations, the absorption of the chyle, and the evacuation of the residuum, constitute a series of necessary conditions indispensable to the regular action of the alimentary canal. Hence it happens that if, in consequence of any disease, these numerous conditions are altered, or even impeded, the digestion becomes disturbed, and more or less diminution of nutrition follows. This is what takes place in preternatural anus—a malady which consists either in an original or accidental opening in the alimentary canal, at a point different from the proper anus, by which opening the aliments, or the feculent matters, are evacuated involuntarily, and before they have been subjected to the necessary changes. The preternatural opening is rarely congenital, but almost always results from wounds, with or without loss of substance; inflammations, abscesses, and particularly from hernia, terminating in the destruction of a portion of the intestine. I mean only to speak of the latter variety—artificial anus.

This condition is by no means so easily produced as might be supposed; and even where life can be preserved only by means of this in-

\* Memoires de l'Academie Royale de Médecine, 1828.

firmity, nature and art united often fail to overcome the difficulties opposed to its formation. Art fails much oftener than nature, because, in order that it may be produced without danger, certain preparatory steps are required, which are within the power of nature, but beyond the reach of art.

The establishment of an artificial anus, in fact, requires the co-operation of many circumstances. It is necessary that the intestine, at the expense of which the new anus is to be formed, should be placed opposite that part of the abdominal parietes through which the matters are to make their exit; that the intestine should admit of being kept in this situation,—or, still better, that it be fixed in the opening; that a ready communication can be kept up between this aperture and that in the bowels; and, above all, it is necessary that these be capable of forming adhesions to the neighbouring parts,—circumstances, the simultaneous occurrence of which experience has shown to be rare. Once established, the artificial anus presents an opening formed at the expense of the intestine and abdominal parietes, intimately united together. This opening, almost always round, but occasionally irregular, varies in size, from a few lines to an inch or more in diameter, and is surrounded by radiating folds of the skin plaited upon itself. The border presents throughout a cicatrix, uniting the skin of the belly to the mucous membrane of the bowel. Round the aperture exists that union between the intestine and abdominal parietes without which the preternatural anus could not be formed.

These adhesions are the product of inflammation, and always commence in the serous surfaces of the intestine and abdominal cavity; and thence extend to the other textures, soon reaching the skin and the mucous membrane. In hernia, these adhesions precede the destruction of the parts, and thus prevent the escape of the intestinal contents into the abdomen. In wounds, again, they do not take place till after the division of the intestine; and this is the reason why these are so frequently fatal. Their extent varies; it is from half a line to a line, in most cases—but in others it is several lines, and sometimes, though rarely, extends through half an inch. The medium of union is a substance which successively passes from a glutinous state to a cellular, and, at length, to a fibrous texture. Arrived at this last stage, it is sufficiently strong to resist effectually most of the causes tending to separate the bowel from the walls of the abdomen. But as these adhesions never extend very far along the intestines, it results that a sort of *cul de sac* is formed, the opening of which looks towards the belly, and the bottom of which corresponds to the skin. Into this cavity the abdominal viscera are protruded, in some individuals, so as to produce hernia, which obstruct, or even alter the position of the artificial anus.

The opening of the anus is almost always occupied by some part of the internal membrane of the bowel, irregularly puckered, and

of a more or less deep red colour. Not unfrequently, protrusions of the bowel take place, the mucous membrane becoming irritated and inflamed. This eversion generally occurs at the upper end of the intestine, sometimes at the lower, and occasionally at both at once; but always forms a curved line, owing to the shape and resistance of the mesentery. Its length varies, from one to fifteen or more inches, and it may be easily conceived how much it must add to the pain and inconvenience.

Between the opening of the skin and the bottom of the artificial anus, there is a kind of funnel-shaped cavity, which Scarpa has well described. This is formed at the expense of the various parts which inflammation and the contact of the alimentary matters have brought to a state identical with that of mucous membrane. The skin forms its border, the intestine its base. Its length, direction, form, and dimensions, vary infinitely, and have very great influence on the cure of the artificial anus. The greater the length and capacity of this funnel, the greater, in general, the tendency on the part of nature to cure the infirmity, or to second the efforts of art in effecting this object.

It is in the bottom of this cavity that the most remarkable and important dispositions of the artificial anus exist. There the orifices of the two extremities of the intestine, and the partition which separates them, are to be found. Of these openings, one belongs to the part of the intestine leading from the stomach, and, in consequence of the feculent and alimentary matters always passing through it, it is the larger and freer of the two. The other orifice belongs to the inferior extremity of the intestine; and as it does not receive any, or, at all events, but very little of the above matters, it is generally narrow, puckered up, and difficult to find.

Beyond these two orifices are the two extremities of the intestine, of which they are the terminations. These extremities, which are villous, and covered with mucus internally, and moistened with serous secretions externally, retire into the abdomen, sometimes crossing and sometimes parallel, but most frequently separating from each other at a greater or less angle; and at length they become more and more curved, till they are lost among the general convolutions of the bowels.

Between the two orifices, placed across, is a projecting angle, more or less marked. This projection, noticed and described by Saviard and Morand, is produced by the juxtaposition and union of the sides of the intestine. Formed by the part of the bowel which the mortification or the wound has spared, on the side next the mesentery, this projection juts forwards, nearer to or farther from the skin, according as the intestine has suffered a greater or less loss of substance, and undergone more or less considerable change in its situation. It is small, and scarcely to be seen in the depth of the funnel, when the intestine has only just been pierced by a wound or eschar, and when

it runs along the posterior surface of the parietes of the abdomen in the natural direction of its curve. But it is very great, and comes out to the level of the skin, when the whole circumference of the intestine has been destroyed, and when, in consequence of this, the two extremities meet at a sharp angle, and, *à fortiori*, when they are parallel. In the former case, there exists, between the two orifices of the bowel, a kind of gutter, which may still direct the matters from the upper one towards the lower; and this, therefore, is the kind of preternatural anus most easily cured. In the latter case, there is no vestige of this gutter; and the projecting *buttress* of which we speak, placed between the two ends of the intestine, forms a barrier which the intestinal contents can neither break down nor get round;—this is the kind of anus most difficult to cure.

This projection does not divide the bottom of the funnel into two equal parts; or even if this be the case at first, it does not long continue so. In fact, thrown aside by the passing current of matters from the upper portion of the bowel, it becomes applied to the lower orifice, acting the part of a valve, and concealing it: hence the difficulty often experienced in finding the lower opening.

This buttress, examined from the cavity of the intestine, has the form of a crescent, the angles of which presenting from the concavity towards the convexity of the bowel, lose themselves insensibly on the inside of the gut, or on the borders of the artificial anus. Examined from within the belly, it is seen to unfold itself, and the two equal parts of which it is composed separate and receive the mesentery between them. This division of the buttress at its base is the result of its mechanism: it is not formed of one single wall except at its sharp edge; at every other point it consists of two sides, having a triangular interval between them, which becomes larger in proportion as they separate from each other on entering the abdomen.

It results from this, that the openings of the two ends are separated by a double partition, the surfaces of which towards the belly are smooth, and free from any adhesion; so that, in order to pass from one of these openings to the other through the intervening partition, it is necessary to traverse the peritoneal cavity. From this arises the difficulty and the danger of attempting to establish a communication between the two portions of the canal, by attacking the projection which separates them.

The buttress and double partition are not fixed so firmly but that they can advance and recede: attached to the mesentery, they follow to a certain extent the movements communicated to them by that ligament. The distribution of the mesentery in artificial anus, though less important than that of the intestine itself, yet merits consideration. Stretching from the anterior part of the vertebral column to the concave part of the intestinal convolutions, it has, in the natural state, no greater

extent than between those two points; and although extensible, it is always more or less dragged when the intestine leaves its natural situation, and is protruded from the belly, as in most cases of hernia and penetrating wounds of the abdomen, with protrusion of the bowels. Compelled to follow the gut which is displaced, the mesentery forms a kind of cord from the vertebral column to the part of the bowel most distant from it. This cord is necessarily tense, and inclines the body forward; thus preventing the power of keeping it upright, and still more of throwing it back. This is particularly observed in cases of hernia, which are adherent. In consequence of this distribution of the parts, the projection or buttress which has been described, as well as the intestine itself, is constantly pulled inwards by the mesentery, with a force proportioned to the degree of tension in this membrane. Hence we easily perceive the influence which the position and movements of the body must have on the cure of this malady. This tension, however, is not free from danger, as I have known it sufficient, in two cases, to destroy the adhesions which united the extremities of the bowel to the parietes of the abdomen, and thus to produce fatal laceration and effusion into the peritoneum.

This action of the mesentery on the intestine does not cease even when the artificial anus has been cured—it is continued long after, and gives rise to the following remarkable phenomenon. Several individuals cured of artificial anus without operation, having re-entered the Hôtel Dieu after several years, and having died of diseases unconnected with this, I examined the parts, and what was my astonishment when, in place of finding the intestine fixed to the inner surface of the abdominal parietes, I found it free and unattached! I might have suspected some mistake, had not the identity of the individuals been perfectly established, and had I not found a fibrous cord extending from the intestine to the part of the abdominal parietes corresponding to the artificial anus. Thus the efforts of nature were not limited to closing up the preternatural opening; they had even separated the intestine from the parietes of the belly—they had restored its natural curve and mobility, by elongating the cellular substance in the form of a cord; and the smallness of this about its middle justifies the conjecture that its laceration would at once have removed the last trace of the derangement which had preceded, accompanied, and followed the formation of the artificial anus.

Nor are these the only changes which take place. The upper extremity of the bowel, excited by the presence and passage of the intestinal contents, acquires increased activity and size; a change in which the mesentery and lymphatic glands participate. The lower portion, on the other hand, ceasing to perform its functions, becomes gradually attenuated, till at length one part of the canal resembles that of an adult, and the other that of a new born infant. Nevertheless, the lower

end does not become obliterated, nor is it even altogether empty—it is filled to a certain extent with the usual intestinal secretions, which, from some remains of the natural functions of the part, are converted into a white mass, of a soft consistence and albuminous appearance, and which may remain, without undergoing decomposition or causing uneasiness, for months, or years, till it is either voided by a natural effort, or washed out with injections.

The consequences produced by artificial anus are these: in the natural state, the intestine free, and floating in the abdomen, though attached to the mesentery, describes a series of uniform curves, along which the contents pass without difficulty; but no sooner is an artificial anus established, than this becomes altered. A portion of intestine directed towards a particular point of the abdominal parietes forms a triangle, the base of which is towards the mesentery, and the sides of which are formed by the upper and lower extremities of the bowel.

The mobility, another condition necessary to the changes of situation, volume, form, and, above all, to the peristaltic motions, by means of which the alimentary matters are propelled:—this mobility is changed, through a greater or less extent, into an absolute fixture, caused by the new adhesions. This fixed portion becomes a *point d'appui* for the efforts of the canal, so that the intestinal contents are constantly directed towards it; and hence results a real acceleration in their progress from the stomach downwards. The space traversed by the aliments is diminished—the period of their detention is abridged—their digestion is rendered incomplete—nutrition is impaired; and the evacuation of the bowels is removed from the control of volition. Every animal has an alimentary canal, the length of which is in proportion to the nature of its food, and each portion of the canal exerts upon the aliment which passes along it an influence different alike from that which precedes and that which follows it; and hence the artificial anus, by diminishing the length of the canal, lessens the degree of elaboration which the food undergoes, and thus impairs digestion—the more as the preternatural opening is nearer the stomach. Thus we see voided from an artificial anus, sometimes matters which are digested; sometimes such as are but half so; and at others, we see the food pass unchanged. In some individuals, the nutrition is not remarkably diminished; in others, it falls off rapidly; and in those who have the artificial anus very near the stomach—as for example, at the commencement of the small intestine—the strength diminishes, the body wastes, and the patient dies of inanition, after a time which varies according to circumstances.

With regard to the involuntary evacuation of the intestinal contents, the opening is not surrounded by any muscular apparatus capable of acting upon it at will; and the aperture is, therefore, always open to the matters which are constantly arriving. Besides, even if there were the necessary muscular arrangement, the

contents of the bowels, deprived of a reservoir where they can become united, retained, and formed, as in the great intestine, would constantly require to be voided. There is thus a constant flow of mucous, biliary, alimentary, or feculent matters, according to the state of digestion and the situation of the opening; and hence the person of the patient is affected with an offensive smell, and the parts are liable to excoriations, erysipelas, and intolerable itching, which renders existence a continual torture. All the contrivances to obviate these evils, do so but very imperfectly; and compression, so as to retain the matters within the bowels, often gives rise to such mischief as to render its abandonment absolutely necessary. It is evident that the buttress and partition which separate the two extremities of the intestine, are, by their greater or less projection into the artificial anus, the causes which facilitate or oppose the cure. How are these obstacles to be overcome? Can we push back towards the belly the parts which form them? Might we divide them by incision—by ligature—or by a slow and graduated section?

In the first part of this memoir I have detailed the nature of the obstacles that prevent the restoration of the intestinal canal; nevertheless, these obstacles, although considerable, are not insurmountable; nature and art have more than once succeeded in overcoming them. The loss of substance of the intestine is certainly irremediable, but the dilatation of its cavity, and the extension of the parietes, may supply, in certain cases, that loss, in a manner more or less complete. The adhesion formed by the intestine to the abdominal parietes, may become less intimate; it may be relaxed in such a manner that the two ends of the gut may be placed in a more favourable situation for the passage of the fecal matters. The projection which separates the two extremities may become diminished by the dragging of the mesentery, and by the efforts made by the fæces to pass from the superior portion of the gut to the inferior. A liberal diet, as recommended by Louis; the action of purgatives, as advised by other writers; the introduction of pieces of charpie, gradually increased in size, as practised by Desault, may have the effect of enlarging the communication between the two extremities of the gut; and these, together with position or compression, and many other means proposed, have no doubt occasionally effected cures. But under what circumstances have they succeeded? It is necessary to distinguish these from cases in which such attempts must always fail; and where, consequently, it becomes necessary to adopt more efficacious means.

Almost all preternatural anuses which consist of simple perforations of a point in the circumference of the intestine, whether attended by hernia or not, are curable; they are, in fact, only fistulæ, behind which the gut is always perfect, not having suffered either loss of substance, contraction, or material change of direction.

The same means of cure will also succeed very frequently in those cases of artificial anus in which a third, or even half the circumference of the gut has been destroyed for the length of a few lines, or even an inch, although accompanied by an inversion of the intestine; but when the loss of substance embraces more than two-thirds or three-fourths of the circumference, and also includes a greater length, the cure becomes proportionally difficult; for then, from the contraction in the caliber of the gut, the buttress and partition become so prominent as to present formidable obstacles to the passage of the fecal matters from the upper to the lower portion of the intestine. The result of the cases that have occurred to me, as well as of those which I have collected from different authors, amounting each to a considerable number, is, that the proportion of artificial anuses susceptible of cure, are to those which obstinately resist every plan of treatment as 3 to 1; that is, two-thirds are cured by the ordinary methods, and the remainder require a more efficacious plan of treatment. The difficulties then that oppose themselves to the cure are the loss of substance and contraction of the gut; the adhesion of its extremities to the parietes of the abdomen; the changes produced in its direction, and in its mobility; but especially the projection and double partition placed between the two extremities.

The loss of substance cannot be repaired; it can only be supplied as we have before said, and we have already shown the limits within which this can be effected. Can the adhesions which at first formed the safety of the patient, be destroyed without producing a recurrence of the original danger?—and even if they were destroyed, the loss of substance remaining, our embarrassment would not be much lessened. It is true, in imitation of Rhamdor, the two ends may be placed within each other, maintained in that situation by suture, and then replaced in the abdomen; but the danger of this method is obvious, and the example given by this surgeon will scarcely be followed by any reasonable man.

It is necessary then, to respect this salutary adhesion, and, therefore, it only remains to attack the partition and buttress. It would seem, at first sight, that the simple section of these parts, either by the scissors or some other cutting instrument, would be sufficient to re-establish the communication between the two ends of the gut; and it must be confessed that such would be the case if the two sides of the projection adhered together; but a moment's reflection will show that this operation must produce almost immediate death, by the effusion of fecal matter into the cavity of the abdomen. It seems more prudent, therefore, to displace the buttress and the partition by pushing them into the cavity by pressure from without inwards, so as to imitate, in some degree, the effects of the dragging of the mesentery. If these trials should not succeed, they cannot, in my opinion, produce any inconvenience. Influ-

enced by this idea, I constructed an instrument which I shall not describe, since it failed in the only trial I made with it, in consequence of the impossibility of regulating its action with any certainty; thereby risking the sudden rupture of the adhesions, and consequent effusion. Being obliged to renounce this method, it only remained either to perforate or divide the projection. The idea of perforation first presented itself to my mind, but it appeared difficult to execute this without producing the same mischief that a common cutting instrument would cause—that of effusion into the peritoneum.

The two ends of the intestine which form the artificial anus, are covered on all sides by the peritoneum, and this membrane forms an uninterrupted cavity round them. This circumstance, which forms an insurmountable obstacle to an immediate division, or perforation, affords the very means by which the double partition separating the intestines may be divided without opening into the cavity.

One of the most remarkable properties of serous membrane is to form adhesions when inflamed, and when in contact; if, then, an inflammation could be excited between the two surfaces of the intestines in contact, and covered with the peritoneum, capable of producing adhesions, I conceived that I should afterwards be able to perforate and divide the parietes of these intestines, and establish a communication between the two extremities without risk of an effusion into the abdomen. But the difficulty was to find a mode of producing this preliminary adhesion of the intestines.

My first idea was to traverse the partition by a needle, which would rather pierce than divide the parts, and which would convey a thread to fill up the void that had been made; this thread, after having excited inflammation around it, might be increased in thickness, and afterwards replaced by a skein, increasing in size from day to day; so that, after some time, it might be large enough to destroy the partition between the two extremities of the gut entirely. Then their cavities would become re-united, and means might be adopted, without inconvenience, to prevent the passage of the feces by the artificial anus, and to compel them to follow their natural course. A consideration of what is often found to take place during the passage of foreign bodies, especially of pins and needles that traverse the parietes of the abdomen and the intestines, tended to confirm this idea; for the passage of these bodies through the different parts is always preceded by *the adhesive inflammation*, which is, in fact, the preservation of the patient. These suggestions were the result of observation only. I wished to strengthen them by direct experiment upon living animals. With this view, I traversed the intestinal canal of several dogs, with needles armed with threads, which I left in the wounds, putting back the intestines into the abdomen. No effusion took place in any instance; the wounds and the threads, after some time, were found sur-

rounded by adhesive inflammation; the ligatures were either voided by stool, or taken away by gently pulling both ends at the same time; the openings made by the needles, and those in the parietes of the intestine, were always found closed, adhesion having taken place between the peritoneal coat of the divided or punctured intestine, and the peritoneum of the neighbouring parts. A still more decisive experiment, attended with the same result, was made by forming an artificial anus in a dog.

My experiments had arrived at this point when, in May 1813, a man named Aucler was admitted into the Hôtel Dieu, 36 years of age, who had laboured under strangulated hernia for five days, the consequence of which was the formation of an artificial anus, the intestine having been found in a state of gangrene. Six weeks elapsed, and nature appeared to afford no prospect of a cure. At first pressure was tried, but this produced symptoms so severe, as to compel me to abandon its use. The patient, however, continuing to urge me to employ other means to relieve his miserable condition, whatever might be the result, an attentive examination showed me that the two extremities of the gut were perfectly on a level, and that their orifices were only separated by a very projecting buttress and partition. After considering the best method of perforating this partition, I determined to pass a needle across it, from as high up as possible in the cavity of the upper end of the bowel; its point was then received into the cavity of the lower end, and thus brought out: the thread with which the needle was armed was left in. The operation was short, and not very painful; it produced no unpleasant symptom. Some days afterwards a skein was carried, by means of the thread, into the opening made in the partition. Flatus began from that time to be passed by the natural anus: the size of the skein was increased at each dressing; and eight days after colic pains were felt in the abdomen, and some fæces were passed by the anus. Encouraged by this, the size of the skein was increased to such a point as one day to produce a laceration of the buttress; this produced no ill effect, but still stercoraceous matter continued to pass from the artificial anus. Considering that those parts of the partition situated above the opening made by the needle, and enlarged by the skein, might adhere together, and might be divided with as little danger as the parts situated below, this part was divided, half a line at a time, by means of a pair of blunt pointed scissors, directed upon the fore finger; this was done at intervals of three or four days: these incisions, very cautiously made, and which never passed the limits of the adhesions formed, enlarged the communication so much that all the fæces soon passed by the natural anus. Compression was then used upon the artificial anus, which would, most probably, in time have closed the opening; but the man, wishing to hasten the cure, urged me to make a fresh attempt, and I had

the weakness to yield to his entreaties. Some irregular portions situated round the aperture were tied, and then excised; I afterwards carried the division of the partition higher than it had yet been done, and in a few hours the patient was seized with acute peritonitis, which proved fatal, notwithstanding every means adopted to arrest its course. I apprehended that this inflammation might have been produced by the effusion of fæcal matter in the abdomen, but at a public examination of the body, no such effusion was found; there was no solution of continuity by which such an accident could have happened, and the cavity contained merely a quantity of purulent serosity, and albuminous flocculi, the ordinary products of acute peritonitis. The communication between the two extremities of the gut was re-established for the space of about two inches. The ends, before separated, had but one wall and one cavity; along the whole length of which, before and behind, there was a raphe, produced by the cicatrix of the section of the partition; and every thing announced, that had not this unfortunate accident intervened, this artificial anus would have been cured.\*

Chagrined at the unfortunate result of this case, I again reviewed the question; and I was confirmed in my previous opinion, that the idea of establishing a communication between the two ends of the intestine, by destroying the partition that separates them, was the only mode that promised any chance of success, and that the only defect was in the means hitherto employed. It was evident to me that, though the passage of the needle, in the above case, had produced no accident, yet by penetrating the parts before adhesion had been effected, the communication it established between the intestine and peritoneum might, in certain cases, produce an effusion in the cavity; besides it appeared difficult, if not impossible, to carry the needle and thread to such a height as to enable them to open a communication between the two ends of the gut, sufficiently large to re-establish completely the course of the fæces in every instance; finally, (and this was the most conclusive reason of all,) the needles and thread could only produce adhesion of the parts, if they were in contact with each other; and if, instead of being parallel and touching each other, the sides of the intestine should be separated at the spot where they were penetrated, a perforation without adhesion would be produced, and the result of the operation would be merely to establish a very dangerous

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\* In a note, M. Dupuytren observes, that the method he had just detailed, and which he thought peculiar to himself, had been practised in Germany by Dr. Schmalkalden, as detailed by Dr. Koreff; and in America, by Dr. Physick, as related by his son-in-law, Dr. J. Sing Dorsey, in his *Elements of Surgery*, published at Philadelphia in 1813. M. Dupuytren quotes the whole passage.

communication between the cavity of the intestine and that of the peritoneum: these considerations induced me to renounce this method of treatment. It became necessary, therefore, to devise a method of keeping the parts in contact previously to dividing them, and which would not effect their division until adhesion had taken place between them; and recollecting the phenomena that present themselves during the passing of pins and needles, &c., I perceived that, following the example afforded in those instances, it was necessary first to produce inflammation, then adhesion, and lastly the division of the parts. At length, after many trials, upon the dead body as well as upon living animals, I believed that I had discovered the instrument which I had sought for. This instrument is composed of three pieces; two branches and a screw. Each branch is about six or seven inches in length, and one which may be called the male, because it is destined to be received into the other, has a blade four inches long, three lines broad, and half a line thick at its edge, which is undulated and terminated by a spheroid button. At the union of the blade with the handle, is a mortise some lines in extent; behind this mortise is a handle, one, two, or more inches, having another mortise running nearly the whole of its length, about three or four lines broad. The female branch is not quite so long as the former; it is composed of one of its extremities of two blades of the same length, breadth, and thickness, as the male blade; between these two blades is a sort of gutter or sheath, destined to receive the other blade. At one of the ends of this blade is a cavity to receive the button of the other. At the junction of the blade with the handle, there is a moving pivot, which is to be received into the mortise of the other branch; the handle is terminated by a hole destined to receive the screw.

The third part of the instrument is a screw of several threads, an inch and a half long, terminated by a plate of an oval form; this screw is to be placed in the mortise of the male branch of the instrument, and fixed in the female branch; its use is to separate or close, at pleasure, the two blades of the instrument. This instrument I named an *Enterotome*. Its application is easily understood: two branches, which may be separated or united at pleasure, provided with blades, very blunt, and with a waving edge, are put into motion by means of a screw passing across their handle. All that these blades enclose is seized, and retained by them by means of their form, as well as by the introduction of the one into the other. The pressure which they exercise upon the parts they embrace, has the effect, at first, of placing them in contact, and it may afterwards be carried to the extent of destroying their vitality, but not of dividing them immediately, the edges being too blunt to effect this. This instrument has not since undergone any alteration, but has been applied subsequently to every case of artificial anus upon which it has been necessary to operate.

However, before I employed it upon man, I applied it to other living animals, and its effects surpassed my expectations. Upon each occasion it succeeded in dividing the parts in six or eight days; and in every case where serous membrane was confined within the branches of the instrument, these membranes, and the parts they invested, were united together from the second or third day, and consequently long before the solution of continuity, which does not happen till the seventh or eighth day. This adhesion, which extended on each side of the branches of the instrument throughout their whole length, though easily broken down at first, became so firm in five or six days as to resist considerable force; at a later period it became cellular, and afforded all the solidity of a natural attachment. What may appear astonishing is, that the action of the enterotome was never attended with severe pain; and the inflammation was always confined to the immediate vicinity of the parts laid hold of by it. This was the more remarkable, since its mode of action did not appear so simple as might have been imagined. It did not produce solution of continuity, like a cutting instrument, that is, without any loss of substance; on the contrary, it caused a real mortification of the parts embraced by it, and the loss of substance is caused by the formation of slough, which, when it separates, is always between the blades of the instrument. It was not long before I had occasion to employ it on a patient named Menage, and having proved successful, I afterwards used it in many other instances.

I will now detail the first case in which the enterotome was employed.

— Menage, æt. 26 years, had suffered from his infancy from an inguinal hernia on the right side, which had never been attended to, and became strangulated on the 2d January, 1815. At the termination of the sixth day, after vain attempts made at reduction, the operation was performed. The intestine was in a state of mortification, and the fæces passed by the wound. In spite of every means, an artificial anus became established, by which all the evacuations passed. They presented themselves at the aperture generally about an hour and a half after a meal, and they were passed, not in the order of their introduction into the stomach, or in that of their digestibility, but rather in relation to the quantity of nutritive matter contained in them; those containing but little nutriment passing out first. The man's appetite became enormous, notwithstanding which he daily lost flesh and strength. About eight weeks after the operation, he experienced violent colic, followed by evacuations per anum, and these were renewed at long intervals.

Such had been the state of Menage for a year, at the end of which time he was admitted into the Hôtel Dieu. The artificial anus was about half an inch in diameter, it was bordered by irregular tumours, arising from the puffing up of the mucous membrane of intestine, behind which appeared, whenever the pa-

tient made the least exertion, a hernia which raised it up, and carried it outwards, giving rise, sometimes, to an invagination of the intestine; the neighbouring skin was extremely irritated; the man suffered great pain, and the stench he emitted was excessive. The patient was eager to have some attempt made to cure this infirmity. My first step, after appeasing the irritation of the skin, was to determine the position of the two ends of the intestine, which were drawn downwards by the hernia situated behind the artificial anus. At length I discovered the direction of the extremities, as well as of the buttress and partition; and immediately I introduced separately the blades of the enterotome, to the greatest possible height, into each of these ends; and after having fastened them together, I closed them moderately; the patient experienced no pain; they were tightened on the following day, and some colic pains ensued. In a few days, the blades of the enterotome became a little moveable; about the sixth day there were abundant evacuations by stool; the instrument fell off on the eighth day; the blades contained nothing but a membranous band, in which all the tunics of the two adhering parietes of the gut were cognizable. The length of this membrane, which was as thin and dry as parchment, was twenty lines, by two in breadth; this was the exact measure of the depth to which the instrument had been conveyed, and consequently that of the loss of substance which the partition of the intestine had undergone. From this time all the fæces passed by the natural anus, and their escape by the artificial one could be prevented by pressure; however, although this was a good deal narrowed, it did not heal up: to obtain this desirable end, various means were employed—compression, bandaging, sticking-plaster in strips, the use of nitrate of silver, &c. Seeing the obstinacy of this opening, now little more than a line in diameter, I excised the edges, and brought them together by means of the twisted suture; and afterwards employed a particular instrument for making pressure. At length, after four months' labour, I had the pleasure of presenting this patient, entirely cured, to the Faculty of Medicine.

In order to apply the instrument, it is necessary, first, to seek for both the orifices of the intestine, and to determine exactly the direction of the canal. This is the most difficult part of the operation. The upper orifice is, indeed, easily found; but, to discover the lower, the finger, or a soft probe, must be employed often, for several days. These points being ascertained, and the patient placed upon his back, one of the blades of the enterotome is directed, by means of the index finger, towards one of the orifices of the gut; it is placed within this orifice, which it is made to penetrate, according to the nature of the case, one, two, or three inches in depth. This done this blade is given to an assistant, and the second blade is introduced, with the same precautions, and to the same height, into the other extremity of the intestine; the two blades

are then brought together, and articulated in the manner of a pair of forceps, by putting the tenon of the one into the mortise of the other. It is sufficient at first, to take hold of the intestine, and to bring the blades of the instrument together in the same way as when cutting with a pair of scissors. The action of the enterotome being intended to be slow and gradual, it can only be kept up by mechanical means. This is done by the screw. The power of the blades is such as to destroy the life of the parts embraced by them. The pressure ought to be so managed as to destroy the life of the part from the first day; it is by so doing that the pain and inflammation are prevented. This pressure is to be increased every other day, by giving the screw a turn or two. It might appear, at first sight, that an instrument carried to such a depth in the abdomen, and pressure made to such an extent as to destroy the parietes of the intestines, would produce colic, vomiting, inflammation, and other severe accidents—but such has not proved to be the case. Indeed, few of those to whom the instrument has been applied, have experienced any but very slight pain: a very small number have suffered from colics and vomiting; the fæcal matters have preserved their course; the inflammation has been confined to the portions laid hold of by the instrument, and has not been communicated either to neighbouring or distant parts. After a few days, the instrument becomes a little moveable: this mobility increases day by day, until it falls off without any pulling, pain, or bleeding; and this happens always between the seventh and tenth days. When it has fallen out, the blades are found nearly closed, containing within them a membrane similar to that above described. Maceration in water shows the nature of this membrane. It is by this loss of substance that the buttress and partition separating the two ends of the gut, are destroyed, and the proper course of the fæces thereby re-established. These evacuations sometimes precede the falling off of the instrument, and at first are white and albuminous, consisting of matters contained in the lower part of the intestine only; and afterwards, they become stercoraceous, being transmitted from the upper portion. At first they are numerous and liquid, with pain, gripings; and straining; they gradually become more solid, and less frequent; the appetite becomes moderate, and the strength and flesh are restored. The most difficult part of the cure remains—that is, to obliterate the false external opening. Many weeks are requisite to accomplish this.

The following case proves, that the above plan is equally applicable to those instances of artificial anus resulting from wounds attended with loss of substance of the intestine.

Louis Tubert, aged 42, was admitted into the Hôtel Dieu, in March 1824, with an artificial anus. This man was weak of intellect, small of stature, with a yellowish muddy complexion, extremely thin and feeble. Eighteen years prior to the date of his admission, he had produced a rupture at the ring of the left

side, in consequence of a violent exertion. The volume of the tumour increased insensibly; so that, at the end of fifteen years, it had acquired the size of an infant's head, and was, in a great measure, irreducible.

Believing himself to be an object of ridicule on account of this infirmity, Tubert conceived that he should be able to rid himself of it by an operation. Without communicating his plan to any one, he made a large incision in his scrotum, opened the hernial sac, and gave issue to a knot of intestine eighteen inches long. He then became alarmed, and sent for a surgeon, who, with some difficulty, reduced the gut, and the patient got well; but the hernia remained—for, considering a bandage as merely a palliative cure, he refused to wear one. He still continued to cherish the notion that he could cure himself by an operation; and brooding over this for about three years, at length, during the absence of his wife, on the 22d February, 1824, he made another incision into the scrotum, opened the hernial sac, and, bolder than on the former occasion, he laid hold of the intestine and cut off a portion. The pain, bleeding, and the issue of faecal matter, however, alarmed him, and he once more sent for his surgeon, who enlarged the opening in the scrotum, found the two extremities of the divided gut, and reunited them by several points of suture. These failed in uniting the intestine, but they produced inflammation of its extremities, which united them to the lips of the wound, and thus an artificial anus became formed. The part removed was a portion, two inches and a half in length, of the small intestine; it did not form a complete cylinder, but was interrupted at two points, one for the extent of about half an inch at its extremity, and the other about the centre, rather larger on its mesenteric side. On his admission into the Hôtel Dieu, there was found, on the left side of the patient's scrotum, a long tumour, extending from the ring to the bottom of the scrotum; it was hard, shining, partly reducible, and exhibited, at its lower and anterior part, a wound of a vivid red colour, formed below by the scrotum, above by the two ends of the intestine, reverted and twisted upon each other so as to make several turns. They were placed side by side: that on the right gave issue to some fluid fæces, mixed with undigested matters, such as pieces of carrot or other vegetables; this was continual and involuntary. The other end of the intestine was retraced, and did not discharge any thing. The patient was in a filthy state, rendered worse by the habit so common to maniacs—that of handling the excrementitious matter. He suffered also from colics, as well as from a fixed pain and tension in the left iliac region. After the lapse of a few days, the two reverted ends of the intestine were reduced, and a compressive bandage applied to the artificial anus; enemata were then administered, and a regulated diet established, but pressure could not be borne; it was tried several times, but always occasioned symptoms rendering it necessary to abandon its use.

The man continuing to waste, I determined to seek for the disposition and connexions of the two extremities of the gut. I found that the upper, or *stomach* end, was situated at the bottom of the scrotum, where it formed intricate circunvolutions, and that the *rectal* end led directly to the ring. This situation of the upper end of the gut, caused me to hesitate as to any attempt at a radical cure; but, at length, the urgent entreaties of the patient, who had heard that similar infirmities had been cured, induced me to make the attempt—especially since the patient's mind became so intent upon this plan, that I dreaded his making an attempt at a third operation himself. I accordingly proceeded on the 31st May, in presence of MM. Larrey, Aumont, and Sanson, to introduce the blades of the instrument, separately, into each extremity of the gut, passing them in as deeply as possible. The upper blade could only be carried to a depth of from two and a half inches to three, and in this situation I was obliged to close and fix the instrument. On the first day there was no pain; the next day there was an œdematous swelling, and some redness at the edge of the artificial anus, but still there was no pain. On the sixth and seventh days, slight colics were felt; the eighth day the instrument fell off, and the two extremities of the intestine formed but one canal. From this time, clysters were given every day; fœtid gas passed per anum; but the fæces still made their way by the artificial anus, and therefore the patient continued still to become thinner and weaker. After the lapse of a fortnight, Tubert conceived that he had passed fæces by the natural anus, and the volume of the tumour diminished. Some time after this, pains in the belly began to be felt: their violence at first threatened to exhaust the man's remaining strength;—however, the evacuations became established in the proper channel, they acquired regularity, and the patient's strength and flesh in some measure were restored. The size of the tumour gradually decreased, but still some faecal matter passed by the artificial anus: to arrest this entirely, I applied an apparatus for the purpose of holding in contact the lips of the wound. This compression is composed of two segments of a circle, very open, of equal size, a few inches long, and some lines only in breadth, placed parallel to each other, each surmounted by a shank of an inch and a half high; these shanks are united by a cross-piece fixed to one, and moveable upon the other, which receives it in a groove with which it is pierced. Beneath this cross-piece is a screw, which rests upon one of the shanks and moves upon the other; and the movements of which to the right and left, produce, as required, either the separation or approximation of the compressing arches. These being padded with linen, or stuffed, are separated—the skin in the vicinity of the artificial anus is raised up, and the fold which it forms is insinuated between the arches; a slight motion given to the screw, from right to left, bring these segments of the circle together, and thus the artificial anus be-

comes so compressed that nothing can pass through it. When this compressor was applied to Tubert, it happened as I expected; nearly all the fecal matter took its natural course; the little that still continued to ooze out was suppressed by an increased constriction of the instrument, and then, for the first time, a smile was seen upon the patient's countenance. However, the instrument sometimes got loose, and at others produced excoriations of the parts, and then the feces began again to flow from the wound; and this occurring several times, gentle and constant pressure with a bandage was substituted. From the period that the excrement passed by the natural channel, the patient rapidly recovered his flesh and strength, so that his appearance alone was sufficient to show whether there had been any discharge from the artificial anus or not. A triangular flap of skin, situate at the upper part of the artificial anus, resulting from the irregular incision made by the patient, seemed well adapted to close what remained of the aperture; this flap was therefore touched with solution of lunar caustic, as well as the edges of the opening, and it was then applied and maintained in this position by the assistance of a proper bandage. The flap united, as was expected, and completely closed the opening—thus perfecting the cure in something less than five months.

I could greatly multiply the examples of cure by the method above related, but the detail of a number of cases would add nothing to what I have already said; it will be more useful to give the general result of the operations I have performed according to the method above described. This method is not theoretical, neither are the cases of artificial anus so uncommon as to render the relation of this plan a matter of indifference. Every year a certain number of such cases is admitted into the hospitals of the capital, and doubtless many hundreds exist throughout Europe. The result, then, of the facts collected in my practice, and as well as of those communicated to me, or made public by different medical men, is, that forty-one operations for artificial anus have been performed by means of the enterotome; that is, twenty-one by myself, and twenty by other practitioners; among whom I am proud to reckon M. Lallemand, Professor at Montpellier, one of my most distinguished pupils. Three-fourths of these operations were rendered necessary in consequence of gangrene from strangulated hernia; the other fourth, in consequence of wounds, with loss of substance of the intestine, to a greater or less extent. Of these forty-one operations three only have been followed by death; one from a presumed effusion of fecal matter into the abdomen, one from indigestion, and one from peritonitis, caused no doubt by continuity of surface between the parts interested in the operation and the peritoneum. Of the thirty-eight remaining patients, by far the greater portion experienced no serious symptoms; some few were affected with nausea, vomiting, or pains in the belly, but these

were remedied by simple means. The whole number were not equally well cured. Nine have preserved, in spite of every thing that could be done, fistulous openings, more or less in extent, which have obliged them constantly to wear a bandage. *Twenty-nine have been radically cured in the space of from two to six months.* Thus, in considering the danger of the operation, it has caused death in one case out of fourteen only; and if the death by indigestion, which ought not reasonably to be attributed to the operation, be excluded, the proportion of the deaths is reduced to one in twenty.

[This memoir, although but just published, was read four years ago at the Academy of Sciences, and a considerable number of operations with the enterotome have since been performed in several countries of Europe; the result of which does not sensibly affect the conclusions here detailed.]

From the London Medical Gazette.

ESSAY ON SYPHILIS. By JOHN BACOT, lately Surgeon to the First Regiment of Guards.

The appearance of another publication on syphilis may demand some apology. It will, perhaps, be urged that the subject is exhausted—that pamphlets upon particular points of doctrine, and elaborate treatises, comprehending both the theory and practice in every form of the disease, have become so common, that nothing remains to be gathered by the industry of the most attentive gleaner in this field of inquiry.

In answer to such assertions, it may be suggested that the mass of information thus admitted to exist, is scattered through a vast variety of publications; that few writers are agreed as to the theory of the disease, or even respecting the practice to be adopted; that the late doctrine of the multiplicity of venereal poisons has materially contributed to unsettle the opinions of practitioners; and that the whole question remains at present in a state of doubt and uncertainty, which must be extremely perplexing to the younger members of the profession.

The essays now offered to the public are the result of much reading, and of an extensive experience in this class of complaints; and it is hoped that they may materially tend to save much of the valuable time of the student, by presenting to him, in one view, the result of the opinions of most of the principal writers on syphilis; by enabling him to form some conclusions as to the justice of the peculiar views entertained by Mr. Carmichael and others; as well as of those opinions respecting the non-mercurial treatment, advocated so freely in this country of late years.

It may be proper to observe, that the style adopted is necessarily plain and colloquial, having originally been delivered in the form of lectures; this will also account for the want

of references to the various quotations, which, however, are faithfully given, and may be depended upon; finally, it is hoped that if these essays should be found to be free from any practical imperfections, and calculated, by the information they contain, to supply the student with a full and sufficient guide to the treatment of the varied symptoms of the disease, the absence of all pretensions to fine writing will be pardoned.

*South Audley-Street, 1st June, 1828.*

It has often been remarked, that those who have been long engaged in any particular study or pursuit, are too much inclined to claim for the object of their choice, a greater degree of consequence than it really merits; and to expatiate upon its importance with a warmth of zeal that appears ridiculous in the estimation of the impartial or indifferent spectator. It must be my endeavour to avoid this common error; but that I consider the subject of syphilis as one of peculiar interest, demanding a greater share of attention than is usually bestowed upon it in the course of a surgical education, is sufficiently evident by my venturing once more before the public in the character of an author.

The veteran practitioner, fully aware of the extent and difficulty of the undertaking, may, perhaps, rather be disposed to censure the temerity, than applaud the courage, of one who, fully sensible of the difficulties that surround the subject, has voluntarily imposed upon himself so arduous a task; whilst the tyro in surgery, who has, perhaps, imbibed the notion that the cure of syphilis is comprised in one simple précept (the administration of mercury,) may be disposed to think that I am engaged in a most unnecessary and uncalled for pursuit. If, however, I should be so fortunate as to satisfy those whose experience and knowledge entitle them to judge of the merits or defects of my performance, it will, I trust, be no difficult matter to obviate the objections of the junior part of the profession. Never, surely, since syphilis became an object of professional inquiry, has there been a period when some positive and determinate doctrines were more imperatively called for, than the time in which we live. In proof of this assertion, I may ask the practitioner to take a survey of the present state of opinion, both public and professional, relative to this disease: scarcely can we find any two surgeons agreed as to the most simple points, either of its theory or practice. If we turn to the recorded opinions of modern writers, the confusion is not lessened: one teaches us that there are three or four venereal diseases; another, that scarcely any thing but pseudo-syphilis is now to be met with: a third goes a step farther, and asserts, that there is not now, and never has been, such a disease in existence, and that for upwards of three centuries we have been prescribing for a phantom of our imaginations; a fourth would fain persuade us that we have only been mistaken in our means of cure, and that mercury is not necessary for any form or symptom of the dis-

ease; whilst, of those who adhere to the old remedy, some are advocates for a thorough saturation of the constitution at all events, and at all hazards; whilst others think, that as an alternative, mercury may be admitted into the number of our auxiliaries, but that it is not to be relied upon alone: in short, if any one should seek for illustration of the often quoted apothegm of the father of physic, that "art is long, and life is short," he cannot choose any more appropriate than the venereal disease, which, after the lapse of more than 350 years, appears to be less understood now than at any period since it became an object of inquiry and interest.

But if the theoretical views entertained respecting this class of diseases have so materially changed, it is no less certain that the practice has undergone, within the last 20 years, a most extraordinary revolution: that good may not eventually result from the conflict of opinions, it is far from my intention to deny, or even to doubt; but it is only very recently, indeed, that the enthusiasm of novelty appears to have expended itself, and that something like a rational and systematic line of conduct begins to be followed.

In adverting to the increased, and still increasing frequency of some of the secondary forms of the venereal disease, I must, however, be allowed to explain, that although these are unquestionably the result of the recent inquiries that have been made into the natural history of the disease; yet those who set the inquiries on foot are not responsible for the evils that have ensued. Those inquiries were commenced in the true spirit of philosophical research, and if they have been too implicitly or generally acted upon, the blame must attach to those who adopted the conclusions without due discrimination. Having once suffered their faith in the powers of mercury to be shaken, they lapsed into the opposite extreme, and became infidels at once with respect to the necessity of its employment. Such, indeed, is the nature of mankind; and it is not only in the science of medicine that we may look for an illustration of this truth.

If every field yielded fruit in proportion to the labour bestowed upon it, little, indeed, would remain to be done with respect to the venereal disease. A writer of the last century has told us, that in his day upwards of 400 Treatises had been written on this disease from the period of its first invasion, and I verily believe that the number has been more than doubled since. But when we come to look over this list, and to examine the share of merit belonging to each individual author, we shall find, that when we have rejected the dreams of superstition, the plagiarisms of some authors, and the perverse attachment to system of others, that the number of standard works will be very much reduced, and the stock of our actual knowledge brought within a very narrow compass indeed.

I shall not consume any more time by prefatory remarks, but proceed at once to explain the course I intend to pursue in treating this

subject. I shall first devote one or two essays to the remote history of the disease, and then take the modern doctrines into consideration: having disposed of this question, and stated my own opinions, I shall commence with the symptoms of gonorrhœa, and then go regularly through the primary and secondary symptoms of syphilis itself, together with the treatment best adapted to each form of the complaint. The name by which a disease is designated is not, perhaps, a matter of much importance; but it may be as well to observe, that I shall generally make use of the word syphilis, as denoting the primary affection; and apply to the constitutional symptoms the term of secondary syphilis; for although I may occasionally speak of the venereal disease, or lues venerea, for the sake of varying the expression, still this latter phrase appears to be too vague and general: the etymology of the word syphilis is indeed doubtful, and, after all, not very obvious; yet still it is pretty generally understood, and is liable to no misconception.

In examining into the history of syphilis, two questions have particularly attracted the attention of authors: the first relates to the antiquity of the disease—the second, to its origin. It may, perhaps, be thought by some, that this discussion is superfluous, and that it can have no other result than that of affording an opportunity of heaping quotation upon quotation, and obtaining for me the reputation of a little reading, at the expense of a great deal of time; but I trust that it will be admitted, upon reflection, that this inquiry is one of absolute necessity, tending to complete the character of the liberal-minded and well-informed practitioner, to whom no species of knowledge should be wanting—who should never be content until he is enabled to give a satisfactory solution to all suggestions or doubts that may arise in his mind upon this or any other subject on which he may be engaged. It is by means of this knowledge that he will be enabled to obviate objections, and to surmount difficulties; or to avoid the imputation of plagiarism, by fancying he is suggesting something new, when, in fact, he is only relating or reviving what has been said or done a thousand times before.

The belief that the venereal disease was known long before the period usually assigned for its invasion, has of late been revived by a modern writer of great experience in the practical part of his subject, and whose labours have latterly made a great impression on this country. The view that this writer has taken of syphilitic complaints has rendered this belief almost a matter of necessity to him, since it smooths many of the difficulties, and explains most of the anomalies, that would otherwise encumber his path; it is, therefore, on this account also, more incumbent upon us to notice the historical part of the subject, and to give a glance at the evidence which is afforded to us by the ancient writers in support of this side of the question.

Of this evidence there are two kinds; that which we derive from the medical writers of

antiquity, and that which is to be found in the works of the poets and philosophers of the same period; for I can scarcely suppose it to be necessary to do more than to allude to the opinion that has been broached more than once, that the diseases recorded in the Bible as having affected David and Job, as well as that which is the object of some of the legal ordinances of Moses, were nothing less than lues venerea. Never, certainly, was there an assumption made upon more feeble grounds: it may have afforded some scope for critics and commentators to exercise their ingenuity, and to display their learning, but nothing can be elicited from these meagre and scattered passages that might not be as well applied to many other diseases, or that ought to arrest the attention of the candid inquirer for a moment.

Among the Pagan authorities we find, from certain passages in Hippocrates, Pliny, Celsus, &c., but more especially the latter, that ulcers, both on the male and female parts of generation, were not uncommonly met with; and Celsus, in particular, gives us many directions for their cure. But before I proceed to quote one or two of these passages, I must observe that there is no allusion whatever, in either of those writers, as to the diseases they mention being *solely* or *constantly* the product of impure connexion between the sexes; still less do we find it asserted that their cure was attended with any particular difficulty, or that any after consequences resulted from them. Thus presenting us with a strong and marked line of distinction between these complaints and the venereal disease of modern times.

Hippocrates, in several separate portions of his writings, and particularly when describing the diseases of the female, mentions ulcers of the womb and of the pudenda, warts, swellings of the groin, &c. and he directs them to be cured by the most simple applications. On examining the writings of Celsus, we shall be much struck with the very precise and clear account which he has left us of several affections of the parts of generation; and here I cannot again help remarking, how impossible it would have been for this elegant and acute writer to have omitted noticing the sequelæ of the diseases, had any such existed in his days. Neither ought we to be surprised, considering the debaucheries, the luxurious modes of living, and other concurrent causes which tend to produce diseases in other parts of the dermoid system, that the parts of generation should have been occasionally liable, among the profligate and luxurious of those days, to breaches of surface, to eruptive diseases, and to inflammation and its consequences. In the 6th book of Celsus is to be found an excellent description of a phymosis, and the method of treating it; directions are also given as to the mode of curing the sores that shall be found underneath the prepuce, when the glans is denuded. Several distinct species of ulceration are detailed. He distinguishes tubercles or *φύματα*, from *φύμιον* or verrucula. Two kinds of cancer are also mentioned, to

one of which he gives the name of phagedena; and he also speaks of rhagades or serpigenous ulcerations in the neighbourhood of the perinæum, and of condylomata or tumours about the anus, which he ascribes to the action of previous inflammation. I shall beg in this place to give you a translation of a passage from this author, which is no bad specimen of the surgery of the time in which he wrote, and which negatively at least may be considered as a pretty strong confirmation of the opinion I have ventured to pronounce, as to the total ignorance of this writer of any after consequences arising from these ulcerations, which he otherwise could not have failed to allude to in this place:—

“Therefore if, in consequence of inflammation, the penis becomes swollen, and the prepuce cannot be drawn over the glans, it is to be fomented with warm water; but when the glans cannot be denuded, a syringe must be inserted between it and the prepuce, and the parts washed out: if then the prepuce gives way, the cure will be more expeditious. If, however, the swelling prevents this, a poultice of lentils, or horehound, or olive leaves, boiled in wine, to each of which a little honey may be added, may be applied, and the penis must be again bound up against the belly, a precaution necessary to be taken in every mode of treatment of that part; and the patient must also be abstemious, and content himself with water only to quench his thirst. The next day the same means of fomentation, &c. are to be repeated in the same manner, and then a little effort may be made to draw back the prepuce; if this cannot be done, it may be slightly opened with a scalpel, the discharge consequent upon which will diminish the swelling of the part, and the skin will be drawn back more easily. In whichever way this end is accomplished, ulcers will be found either in the inner part of the prepuce or on the glans, or even beyond it, on the penis itself, which are either clean or dry, or moist and purulent.”

The different methods adapted to the treatment of these various ulcers is then detailed; but in every variety the healing of the ulcer is evidently the completion of the cure, for neither in his chapter on diseases of the skin, nor in that in which he describes ulcers of the throat and nose, do we find the slightest insinuation of such symptoms being found in connexion with ulcerations of the sexual organs. The story told by the younger Pliny, in the 24th epistle of the 6th book, is scarcely deserving of being related at length: it evidently alludes to the destruction of the penis, and it was followed by the death of the sufferer, though his death was not immediately caused by the disease. This story adds no direct strength to our argument, it is true; but it may be observed, that the relation would have afforded an opportunity for the writer to have descanted upon the disease and its consequences, had he heard of it, either in degree or kind, as we are accustomed to see it now. I might have extended this account by drawing

your attention to a passage or two in Galen bearing upon this point; but the remarks I have already made upon what Hippocrates has said, apply with equal force to his commentator.

The evidences of the antiquity of the venereal disease which have been culled from the writings of the Pagan philosophers and poets, will next demand a little of our attention. The list of these authorities is certainly formidable, both in point of number, as well as from the reputation of the authors. We find included in this list the names of Herodotus, Tacitus, Suetonius; and still later, Eusebius, the ecclesiastical historian; and Palladius, the bishop of Hellinopolis. Among the poets, Martial, Juvenal, Horace, and Ausonius, have each afforded some expressions which have been eagerly laid hold of by the supporters of this doctrine. After having enumerated this long list of great authorities, I need surely no longer insist upon the necessity of being acquainted with this branch of the subject; and how little should we be prepared to encounter an adversary armed with these learned and imposing names, unless we were in possession of the facts upon which the belief of the antiquity of syphilis is founded; but when once acquainted with them, very little explanation or argument will, I imagine, be necessary to point out the fallacy of the doctrine. For this purpose, I shall first mention what Herodotus relates concerning the spoliation of the temple of Venus Urania by the Scythians, when they invaded Palestine, on which account, says the historian, their descendants were afflicted with a disease which is called in the Latin tongue “*morbus fæmineus*,” and which the best commentators suppose to have been really a gonorrhœa, or flow of semen, in the strict sense of the word, by which their testicles became wasted, and, in fact, they lost both the powers as well as the appearance of men. So that it is clear, if this mean any thing at all, at least it has no reference to the question at issue. Suetonius, in speaking of the Emperor Augustus, says, “*Corpore ipsum fuisse maculoso, dispersis per pectus et alvum, genitiois notis in modum et ordinem ac numerum stellarum cælestis Ursæ, sed et callis quibusdam ex prurigine corporis, assiduoque et vehemens strigilis usu plurifarium concretis, ad impetiginis formam.*”

The passage from the annals of Tacitus is still less to the purpose, for we are simply informed that the Emperor Tiberius, a man infamous for his debauchery, had, in his old age, a bald head, an ulcerated face, and was completely worn out and bent double. I have already alluded to the story told by Palladius, who informs us that a certain person named Hieron, much addicted to intemperance of all kinds, whilst at Alexandria, fell into the snares of a female performer at the theatre, with whom having sinned, he was visited by the divine wrath with an anthrax on the glans penis, which terminated in the loss of all the parts of generation; after which it appears that he re-

covered, and became a miracle of penitence and piety.

The case related by Eusebius is that of a man who had, in the secret parts of his body, an abscess and a fistulous ulcer, which proved to be incurable, breeding an infinite quantity of worms, and of a most foetid and intolerable odour. Here the precise part affected is not even designated: the phrase is "*in mediis occultiorum corporis partium locis*;" and may as well apply to a fistula in the perinæum, or in the scrotum, as to any disease else. No general bodily affection is hinted at, nor are we told that the complaint originated in any improper conduct upon the part of the patient. It will surely not be necessary to pursue this phantom any farther; whoever wishes to consider the arguments which have been drawn from the Roman poets, may consult the second Satire of Juvenal, the 37th Ode of the first book of Horace, the sixth book of Lucretius, and the first book of the Epigrams of Martial;—in these passages he will find abundant evidence of local disease, but not a word that can be construed into any similarity between those affections and the lamentable consequences attending the invasion of syphilis; consequences which might have afforded the finest scope to the satirists and the moral writers of antiquity, and which, in times comparatively modern, as Dr. Friend has justly remarked, has not been alluded to in the writings of either Dante or Boccaccio—who were not a whit more likely than their predecessors to have suffered so fertile a subject to have escaped them.

I may next take a cursory view of what the Arabians have added to our stock of knowledge, relative to the antiquity of this disease; and in doing so, it is necessary to recollect that the medical writers of that extraordinary people were large borrowers from the Greek authors, and were also well acquainted with many formidable cutaneous diseases, particularly the leprosy and elephantiasis. On another account, also, the Arabian writers demand our attention, since from them the employment of mercurial preparations was unquestionably derived; the application of which afterwards, to the cure of the venereal disease, was, in all probability, the result of analogy, since their utility in many cutaneous diseases had been long recognized by the practitioners of that nation. But, before I make any further mention of the Arabians, it will be proper, in order to keep up a connected chain of evidence, to mention a few of the later Greek authors, with the dates of their respective works; showing that from them no additional arguments for the antiquity of syphilis, can be fairly adduced. Of these, I shall merely mention Aribasius, Citius, Paulus Egineta, and Actuarius. The first of these writers does little more than copy what was already known upon the subject of ulcerations, and other affections of the genitals; nor does Etius, though much more copious, add any thing to our stock of information. Paul of Egina talks of the excision of warts, under

certain circumstances; he directs them to be tied, or destroyed by the cauterium; and many precise rules are laid down for the treatment of ulcers of the parts of generation, of rhagades, condylomata, and their varieties, under five or six different appellations;—but still the great distinction remains: there is no allusion to subsequent disease as deducible from these local affections—no hint that the constitution might participate in the mischief. Four centuries later, Actuarius flourished at Constantinople. He has been supposed, by some historians, to have been bred up originally in the schools of the Arabians; at least, it is very certain that he borrowed as liberally from them as from the Greeks, and yet we shall search his writings in vain for any new or more precise information with respect to these affections; and I am induced to mention this author in particular, because he forms a fair link of connexion between the Grecian and Arabian authorities, and proves, not only by what he has said, but by his having omitted to say more than is to be found in the writings of his predecessors, that no new symptom, nor additional feature of aggravation, belonged to these complaints in the time in which he lived, than had been remarked for centuries before him.

When we speak of the authority of the Arabian writers, it will be necessary to recollect that the chief learning of that nation was originally derived from the Greeks. Alexandria was taken by the Saracens in the seventh century; and though we are told of the destruction of the famous library, by command of the Caliph Omar, there is good reason for believing that the schools of physic were still kept up in that city, even in the succeeding century, by Christian teachers. It was not until the year 767 that Bagdad was built; from which period, the seat of Arabic learning seems to have been transferred there; yet, even then, most of their philosophy appears to be borrowed from the Greeks. I have considered it necessary to enter into this explanation, lest it should be thought that I have not dwelt at sufficient length upon the writers of this nation: there are, in truth, but few of them who do not mention ulcers, warts, fistulæ, and other affections of the parts of generation; yet, excepting in the adoption of new names, there is little else to arrest our attention; for instance, in the writings of the Arabians, we first read of the Bothar, the Alsophate, the Moram, and Ignis Persicus, though the precise meaning of some of these appellations is by no means obvious; indeed many of them appear to have been synonymous terms, whilst others have been adopted from their supposed resemblance to the effects of fire, as, for example, the Ignis Persicus, "*qui dicitur ulcus carbonosum et generatur ex sanguine ferventi cum cholera mixto*;" just as the Formica was so designated, because the pain it produced was compared to the bite of an ant. Rhazes, to be sure, mentions an ulcer of the penis, which, according to him, was produced in a very strange manner, viz. by the "*accensio-*

nem mulieris supra virum;"—and Avicenna gives us reason to believe that the leprosy was sometimes communicated by connexion between the sexes; a circumstance by no means to be wondered at, considering the undoubtedly contagious nature of that disease; but he does not insinuate that there was any thing new or remarkable in this, or that the symptoms differed from those usually met with in leprous patients. He mentions, indeed, an ulcer of the penis, and heat of urine, as symptoms by no means unusual in the progress of that disease, though it is to be remarked that, some ages before his time, Citius, in speaking of the contagious nature of leprosy, warns his patient to avoid coition; nay, he declares it to be very unsafe to go near a leprous patient.

I have now brought down my history to the writers of the middle ages, and have already said enough, I trust, to convince the sceptical that there is no really well-founded reason for believing that any disease, generally affecting the constitution, or tending to the destruction of the patient, was known to the Greeks, Romans, or Arabians, as the direct consequence of connexion between the sexes; but I must now claim a little farther indulgence, in order to clear up the difficulties which the zeal of Mr. Beckett, a very staunch partizan of the antiquity of syphilis, has thrown in our path; for although Astruc has, I think, very successfully combated his arguments, and overthrown his authorities, yet, as Mr. Carmichael has, in his late publication, again brought that author into notice, and appears to give implicit credit to his assertions, it is incumbent upon me to examine a little into his pretensions; and here I cannot refrain from quoting a passage from the work of the learned and acute historian of physic, Dr. Friend, who, speaking of the first invasion of syphilis, says, "It may be proper to observe, that in the earliest appearance of this distemper, as well as since, there were many who, not being used to think, or reason, any farther than as the ancients showed them the way, took a great deal of pains to prove that the disease was known both to the Greeks and Arabians, though but imperfectly described, and represented under the names of the different kinds of leprosy, exulcerations, and other cutaneous affections. And here we have instances how the words of old authors may be wrested and perverted to serve the present purpose, and support a favourite opinion; for their method of arguing was to quote by scraps—to pick out one symptom out of one author, another out of a second, and so on, till they at last dressed up such a disease as the ancients had not the least notion of." Again, the same author very pertinently remarks, in answers to those who believed syphilis to be no other than leprosy, under a new name, that the leprosy was then a common disease, and could not want such an extraordinary phenomenon in the heavens as is related by some authors to account for the first appearance of syphilis. But to return to Mr. Beckett:—this gentleman wrote as lately as

the year 1718, and his papers are to be found in the 30th and 31st volumes of the Philosophical Transactions. The chief authorities he adduces in support of his opinions are the following:—the first is to be found in the writings of John Arden, an Englishman, who, it appears, lived at Newark in the year 1349, and afterwards removed to London.

Among other stories relating to this subject, it would appear that he mentions a disease called *arsura*, which consisted of an internal heat, with an excoriation of the urethra; but this is only a repetition of what is to be found in the Arabian writers. Beckett might have added, that he also relates cases of abscesses and schirrous tumours, that form in the penis, but not one word does he say either as to their origin or consequences; nor can we doubt that, in the first named disease, he speaks of gonorrhœa. Mr. Beckett's second argument is drawn from the ordinances of the Bishop of Winchester's Stews in Southwark, where the disease of brenning, or burning, is recognized as the product of impure connexion, and many regulations are detailed to prevent the spreading of the disease. This argument, however, I need not enlarge upon, since it is of pretty general notoriety, and has received ample confirmation from several authors of that age, particularly in a work of Dr. Boord's, published in 1546; from a tract by Dr. Bulleyn, in 1562; and also from a manuscript by one John Bayle, in the possession of Mr. Beckett himself. Another source of argument might also be derived from the statute published by Joan, Queen of the two Sicilies, and Countess of Provence, in the year 1347, the fourth article of which is to the following purport. The queen commands, that, on each Saturday, "the bayless of the brothel, and a barber deputed by the consuls, do visit all the strumpets who shall be lodged in the brothel; and if any one be found who has contracted any disease by fornication, such women shall be separated and lodged apart, in order to prevent the communication of disease to the young men." It is proper, however, to add, that there are some doubts as to the authenticity of this document. So far Mr. Beckett successfully proves that gonorrhœa was a common disease long before the siege of Naples; but that fact has not been denied, and is distinct from the question at issue. He next proceeds to relate some cases, in which the leprosy was communicated by intercourse between the sexes—a truth, the possibility of which no one could deny; but as leprosy was well known in those days, acknowledged universally to be contagious—and, moreover, as no new symptoms are recorded, and no astonishment is expressed by the relaters of these cases, as to such an occurrence, we may fairly conclude that this argument is worth little or nothing.

Mr. Beckett next quotes Theodoric, originally a Franciscan friar, and afterwards Bishop of Cervia, who wrote in the twelfth century. This author is remarkable on several accounts: first, as describing the same disease, the *arsura*, as arising from impure connexion with a leprous

woman; and secondly, as having been the first who introduced the use of mercurial preparations into practice. The effect of mercurial inunction upon the mouth seems to have been well known to him; and this knowledge he plainly appears to have derived from the Arabians, among whom several formulæ for the preparation of these remedies are to be met with, and which they applied to the cure of many cutaneous diseases. Theodoric is copied largely by our countrymen, Gibbertus Anglicanus, and John of Gaddesden; the latter of whom recommends the following extraordinary mode of cure to the female patient, who is directed to leap backwards down stairs. Such are the principal facts adduced by Mr. Beckett in his first paper. Two years afterwards, he published a second; in which he brings forward two additional testimonies in support of his former opinions. The one is a manuscript in Lincoln College, Oxford; wherein it is asserted that Thomas Gascoigne, Doctor of Theology, the author of the manuscript, was acquainted with several persons who had died of the putrefaction of their genitals, and of their whole body, in consequence of illicit connexion; adding, that John of Gaunt died of this same disease; although, from the context it would appear plainly that no particular disease was alluded to, in his instance at least, but that the death of that prince was owing to "*frequentationem mulierum, magnus enim fornicator fuit.*" The other testimony is that of John Arden, spoken of above; to which there is, therefore, no occasion to revert. It is strange, however, that Mr. Beckett, zealous as he was, should have overlooked one or two authorities, at least equally strong, or stronger, than any he has produced; but as it is my intention not to conceal any thing, I shall point out these authorities, though I conceive they admit of the same explanation which has been given to the previous quotations. The passages I allude to are found in Lanfranc, Gordonius, and in Gulielmus de Saliceto: the two latter, indeed, only repeat what the former has said, but that is, at least, a proof that the subject matter was believed and recognized in their days. The former of these writers, who lived in the middle of the thirteenth century, speaks of a bubo, which, he says, may arise from a corruption in the the penis of a man in consequence of lying with a foul woman, or from other causes; and the reason he gives for it is curious enough:—he says, that the corruption is multiplied, and retained in the yard, "*unde non potest natura mundificare virgam aut locum, primo propter strictam viam illius loci, unde redet et regurgitat materia ad locum inguinum propter habitatem loci illius ad recipiendum superfluitatem qualibet; et propter affinitatem quam habent hæc loca ad virgam.*" Gordonius, whose work appeared in 1305, acknowledges abscesses and ulcers in the penis as the consequence of lying with a woman whose womb is foul, and full of virulent sanies. To these descriptions Lanfranc adds, that whoever wishes to preserve himself from corruption, when he has had con-

nexion with a female suspected of foulness, (*immunditia*) should wash the penis with vinegar and water. Now these three authorities appear, upon the first glance, to give strength to Mr. Beckett's arguments, but, in fact, they do no such thing; they prove, I think, in the strongest manner that negative evidence can do, that beyond a gonorrhœal discharge, and ulcers or pustules on the parts of generation, no other evil consequences were to be dreaded from impure connexion, and we shall soon see how strong and marked the contrast becomes; not by imperceptible shades and gradations—not by one author adding one symptom, and a successive writer another—but that *all at once*, towards the conclusion of the fifteenth century, the public become the victims of a train of symptoms altogether unusual and undescribed; rebellious to every mode of treatment then in use—not attacking the poor only—not confined to those whose circumstances and situation in life exposed them to more than the ordinary chances of disease, and deprived of the ordinary means of cure—but exhibiting all its rage, and exerting its baneful powers, over princes, cardinals, nobles, of both sexes; thus proving, not only the severity, but the extensive progress of this hitherto unknown scourge. Surely then, I may be allowed to say, that if there is any single historical fact that can be said to be proved, it is that of the origin of syphilis being referable to the latter years of the fifteenth century; for I cannot understand otherwise, why, at that precise period, we all at once hear of ulcers on the parts of generation in both sexes, followed speedily by excruciating nocturnal pains, by corroding ulcers over the whole body, by affections of the throat and nose, and very frequently by death; when not one word, that can be construed into any similar affection, is to be met with distinctly stated in any writer before that period. I think we have no right to impute so much dulness to our forefathers; they offer no parallel instance of any such gross error in mere matters of fact. Whatever their faults as theorists may have been, as careful observers of what was passing under their eyes every day, so gross a mistake could not have occurred to them; and had such a dogma as the novelty of a really old and well-known disease been started by any interested or ignorant writer of that age, we cannot but believe that it would have met with an instantaneous refutation and denial. I here, then, beg leave to declare my belief that the venereal disease was not known to the writers of antiquity; and, secondly, that, in the course of our research, it has appeared very clearly that a disease resembling gonorrhœa had been prevalent in Europe several centuries earlier, and most probably had been known from the remotest periods of history. But of this, more hereafter.

Having now disposed of this first part of my inquiry, I come to consider the origin of syphilis; in other words, whether it is a disease imported from the West Indies or not? This belief has been so generally adopted, that an

attempt to controvert it may, perhaps, startle the majority of my readers; nevertheless, it seems to me to be an opinion formed too hastily, directly at variance with historical evidence, and acquiesced in, probably, more on account of the apparently useless nature of the inquiry, than from any intrinsic force in the arguments by which it is supported. It may not, indeed, be a matter of much moment to the practitioner, to ascertain whether the common account of the origin of syphilis be true or not; but as a matter of literary research, it is surely deserving of some moments of our attention; and we surely need not refuse to occupy ourselves for a short time in an inquiry which Sydenham thought worthy of his consideration, and which has attracted the notice of Swediaur, Sprengel, and other eminent practitioners. Among those who have doubted the commonly received account of the birth-place of the venereal disease, I might mention the name of the late Mr. Pearson, from whom, indeed, my scepticism upon this point was originally derived, and who has recorded his doubts in his *Treatise on "the Effects of various Articles of the Materia Medica in the Cure of Lues Venerea."*

To return to my subject; I shall not rely much upon what Sydenham has urged on this part of my argument, since he does not enter deeply into the inquiry, but proceed at once to mention the works of Dr. Sanchez Riberio, of Hensler, Sprengel, and Swediaur, with the authorities they have adduced: Dr. Sanchez published his opinions at two separate periods, his first work appearing in 1765; to this, Dr. Robertson, who, though not a medical man, may be supposed to be a competent judge of an historical fact, is not disposed to attach much importance, but he afterwards says, having seen the second edition of Sanchez's book, "it contains several additional facts in confirmation of his opinion, (that is, that lues was not imported from America) which is supported with such plausible arguments as render it a subject of inquiry well deserving the attention of learned physicians. Dr. Hensler's work was published at Altona, in the years 1783 and 1789; and from these authors may be collected a train of facts and reasonings founded upon them that are well worthy of our consideration. In the first place, they call to our recollection that Columbus returned from his first voyage of discovery in the month of March 1493, to the port of Palos in the Mediterranean sea; (they ought to have added that he first put into the Tagus, and remained at Lisbon for five days;) from Palos he went to Barcelona, where Ferdinand and Isabella then held their court: he was accompanied by some of his crew, and six Indians whom he had brought with him from the island of Hispaniola: the remainder of the ship's company continued at Palos or Seville, and we do not hear that they communicated any disorder in either of those places: how then can we reconcile these dates with what Baptist Fulgocius has related; who asserts, that two years before king Charles's invasion of Italy, that is, in 1492, a new disorder

broke out, for which the physicians knew no remedy? In France it obtained the name of the Neapolitan disease; whilst at Naples it was called the French disease. The testimonies of De Isla and Oviedo, though they both affirm the West Indian origin of syphilis, appear to be deserving of little credit; (indeed the authority of Gonzalez de Oviedo is worth nothing, for he affirms that the disorder was conveyed into Italy by Cordova's fleet, which, however, did not arrive at Messina until 1495, and consequently not till two years after the disease had existed there;) but in truth both these authorities sink into insignificance when contrasted with the silence of Peter Martyr, who was physician to the King of Spain, and actually at Barcelona when Columbus made his appearance there after his first voyage, and where he remained until nearly the end of the same year, and yet he (Peter Martyr) does not say one word as to the importation of this disease in any of his writings; but this is not all, for the same author, in a letter addressed to Arius Lusitanus, the Greek professor at Salamanca, and which letter is dated in the year 1488, that is, five years before the return of Columbus from America, has the following decisive passage. "*In peculiarem nostram tempestatis morbum, qui appellatione Hispanâ bubarum dicitur, (ab Italis, Morbus Gallicus, Medicorum Elephantiasin alii, alii aliter appellant) incidisse precipitem, libero me scribes pede. Lugubri autem elego calamitatem ærumnasque gemis tuas, articularum impedimentum, internodiorum hebitudinem, juctuarium omnium dolores esse proclamas, ulcerum et oris fœditatem superaddis."* To this, I shall subjoin what Leo, the African, says:—"This (the French disease) was not known in Africa before the time that King Ferdinand drove the Jews out of Spain; it is looked upon as an undoubted fact that it was brought from that country;" and he farther asserts, that it took its origin from the commerce which the natives of Africa had with the wives of the banished Jews.

Another historical fact throws some additional light upon this subject: it appears that when Grenada was taken by Ferdinand and Isabella that many of the Moors fled into Italy, and they are distinctly accused by Infessura as having imported the disease into that country. In the month of June, in the following year, that is 1493, the Spanish ambassador complained that the Pope (Alexander the Sixth) had received these fugitives into the city of Rome. In October it is announced that a Cardinal had died of this new plague; and early in 1494, the Pope wrote to Charles the Eighth of France, who was then preparing for his Italian expedition, that he had better delay his journey, as a great and new plague was then raging in Rome. But perhaps a still stronger proof of the position I maintain, may be gathered from the silence of Columbus himself, as well as that of his son Ferdinand, who wrote the history of his father's life, in which he gives a description of all the diseases which afflicted the Spanish adventurers up to

the year 1496, but there is no mention of such a disease as syphilis to be found in his work; neither do any writers on America, for the first 35 years, make any such assertion—the account given by Lopez de Gomara relating to a period long subsequent to this. I shall beg leave to add one more consideration: among the numerous names given to the disease, upon its first invasion, no one ever thought of calling it the American disease, a most singular omission, if the persuasion of its West Indian origin had been so universal as it was afterwards asserted to have been. I am unwilling to extend this part of my subject unnecessarily, and I shall therefore only observe, in reply to those who draw an argument for the contrary belief from the knowledge which the natives of Hispaniola had obtained of the virtues of the Guaiacum wood, and the cures they were enabled to perform by its means. Upon inquiry, we shall find that the first knowledge of this remedy in Europe was not obtained until the year 1508, rendering it very probable that the discovery of the virtues of the Guaiacum was but recently made by the Indians; for had they known it previously, there can be little doubt that the knowledge of the disease, (granting its American origin,) and its remedy, would have been communicated nearly at the same time. Upon this, Swediaur remarks very properly, that should the natives of Otaheite discover some remedy for the venereal disease, it would be quite as fair to conclude that, therefore, it must have been endemial in that island. It would be easy to produce many other passages from contemporary authors, tending to establish the position that I have assumed, namely, that syphilis was known in Italy several years prior to the return of Columbus from America; and that the invasion of Italy by the French, followed by the siege and capture of Naples, where the troops of several nations were assembled in great numbers, served only to render its progress more rapid, and to spread it in every direction throughout Europe; for we find, that in 1497, it had acquired so much importance, and become so serious an evil in Paris, as to give rise to an *arrêt* of the parliament of that city, by which, among other regulations, it is ordered that every person not actually residing in Paris, should, when seized with the disorder, after the date of the proclamation, go out of the city, to the country or place of their birth, under the penalty of death; and a few months later, that in September, James the Fourth of Scotland found it necessary to issue a proclamation, banishing all persons afflicted with the *grand gore* to the island of Inch Keith, over against the town of Leith. I have now said enough, I trust, to show that there are sound reasons for doubting the commonly received opinion of the American origin of syphilis; but it may very reasonably be asked, from whence then did the disorder proceed, since it is quite evident that its origin must be dated at no very great distance of time from that period, and that it then first began to excite a great, but well-founded, alarm

among all the nations of Europe? This, then, will form, in part, the subject of my next Essay.

From the Glasgow Medical Journal.

ON IRRITATION OF THE SPINAL NERVES. By THOMAS BROWN, M. D. Senior Physician to the Royal Infirmary of Glasgow.\*

In the following essay, I purpose to offer a few observations on the symptoms and treatment of some diseases of the spine, which are by no means of rare occurrence, but which appear occasionally to be overlooked or neglected.

I allude chiefly to those morbid affections of the spinal nerves so often met with in young females, and occasionally also, although much less frequently, in women of more advanced life, and in males.

In most instances, certainly, this irritated state of the nerves is not entitled to the name of serious disease, since the symptoms are not acute, and since they are easily removed or prevented; but when it is allowed to remain for any considerable length of time, it often produces nervous complaints and general bad health; and, of course, it becomes infinitely more unmanageable.

It occasionally happens, too, that this affection of the spinal nerves, even from its commencement, is much more serious in its nature, and instead of being local or strictly confined to one spot, extends to other parts of the spine, or even to distant organs, and assumes an appearance particularly distressing and obstinate. Still, however, as the symptoms which attended this more formidable disease arise from a local affection of the nerves, I have ventured to treat of it in this essay. This form is peculiar to females.

Although I have had numerous opportunities of attending to the symptoms and treatment of these affections, in the Lock Hospital of this city, as well as in the Royal Infirmary, and in private practice, yet I find that this essay must remain exceedingly imperfect. I trust, however, that the involved and vague nature of every subject connected with nervous diseases will serve as a sufficient excuse.

I find some difficulty in giving a name to this disease, but as it consists, perhaps, in a state of increased irritability in some of the spinal nerves, we may name it SPINAL IRRITATION.

It is proper to remark, in a very general way, that the complaints at present under consideration differ materially from some diseases which are exceedingly apt to be confounded with them, and which occasionally affect the spine along with spinal irritation. I allude to lateral curvature, and to inflammation of a portion of the spine, either from accident or

\* Five years since, viz. in January 1823, the following Essay, nearly, was read to the Medical Society of this city.

from other causes. These diseases of the vertebræ are by no means uncommon at the age which is most liable to this other complaint, and instances do occur in which the two diseases are combined. In C. D., in particular there was ultimately decided lateral curvature of the spine, as well as spinal irritation; and I have seen the same combination in other patients. I conceive that this fact may explain some of the anomalies of this perplexing disease.

After I have given some examples of spinal irritation, I shall mention two or three other cases, in which, as well as in that disease, the chief pain was felt at the sentient extremities of the spinal nerves, but in which the local disease in the spine appeared to be different from that which existed in the other complaints.

In the first place, I shall give an account of one case out of a great number, in which spinal irritation occupied its most usual situation, and was marked by its most common symptoms. I shall then notice a few instances in which it was more complicated and varied in its form, though it occupied nearly the same part of the spine. I shall afterwards detail some cases in which the morbid affection occupied some other than the common situation, and was attended by unusual symptoms; and I shall conclude with giving some general remarks on these diseases, and on their treatment.

Miss C. aged 17 (September 1822,) of a robust make, and apparently in good health, for more than a year has complained of pain, situated below the left mamma. This has been fixed to one spot for nearly the whole time. It is a gnawing bruised feeling, increased materially by fatigue of any kind; and, after fatigue, it is attended with restlessness. It is relieved by reclining in the horizontal posture. It is not sore to the touch. The complaint has been treated by a surgeon in the country as a case of rheumatism. She has been bled and blistered for it, but without any good effect; and at last it has occasioned so much anxiety in the minds of her relations, that she is brought to Glasgow, from a considerable distance, for the benefit of farther medical advice.

On examining the spine, it is found to be perfectly of the natural shape and appearance; but when pressure is made on it, about the 7th or 8th dorsal vertebra, she complains of a considerable feeling of tenderness, amounting even to pain: and she finds that the uneasy sensation shoots forward exactly to the affected part of the breast. She had not paid any attention to this tender part of the spine; indeed, she had no idea that there was any thing faulty there, till her attention was called to it by the examination.

After I saw her, she had a dose of physic; ten leeches were applied to the pained part of the spine, followed afterwards by a small blister; and the horizontal posture was enjoined. She was nearly free of pain in a few days, and returned home, with directions to repeat the blister, and to avoid fatigue.

It would be needless to bring forward any

other example of the most common form and site of this disease, than the one I have mentioned. From the journals of the Lock Hospital, and from my notes of private practice, I could, I am confident, quote fifty cases nearly similar. Such instances occur frequently in the experience of every medical practitioner. There are, however, several *varieties* in those symptoms which attend the disease, when seated, as in the former case, in the lower part of the dorsal vertebræ. I shall mention those merely that I have seen.

The site of the pain in the *breast* varies much. It is needless to mention, that it is occasionally in one side of the chest, occasionally in the other; but I am confident that it is much more frequently in the right than in the left. It is sometimes within a few inches of the spine, but much more frequently it is nearer to the sternum, and occasionally it is immediately under this bone. It is generally described to be a weary or bruised pain. It is seldom increased by the touch, but sometimes, though rarely, it is tender when pressed. It is usually, though not always, relieved or even removed by the horizontal posture.

The tender part of the *spine*, on the other hand, in a great number of instances, is not attended to. It is not thought of till the affected part of the back is pressed, or till a sponge dipped in hot water be applied to it. In either case, a very sensible pain is felt, which, especially when the sponge is used, is occasionally acute and continues for some time. In general, pressing the spine not only occasions pain in that part, but the pain penetrates to the affected spot of the chest, thus distinctly proving the connexion between the two. It often happens that pressure on the spine occasions a feeling of oppression more than of pain in the chest.

The pained part of the spine, in general, does not exceed an inch in diameter, though it occasionally happens that the uneasiness extends either above or below the tender part, appearing to radiate from it as from a centre, sometimes to a considerable distance. This pain of the side, excited by pressure, is almost in every instance felt on the same side of the spine as the pain in the chest. That is, if the patient have pain in the left side of the chest, then the left side of the spinous process of the vertebra is more tender than the right, and *vice versa*.

There is seldom any fever with these symptoms, although I have repeatedly seen instances in which there was a considerable degree of febrile irritation, requiring bleeding and other evacuations.

Cough is not often present, although there is occasionally violent convulsive cough, very noisy, and not attended with any expectoration.

In a girl, a patient in the Lock Hospital, there was pain on the left side of the sternum, as if the part had been bruised. This was distinctly connected with a tender state of the spine below the middle of the dorsal vertebræ. Along with these common symptoms, there was severe pain and numbness extending down

the left side of the abdomen near the spine to the groin, and down the thigh, following nearly the direction of the anterior crural nerve. There was also a considerable degree of febrile irritation.\* In two other cases, along with the tenderness of the spine, there was an extended and superficial feeling of rawness and pain over one side of the thorax, which even extended to the abdomen. It was described as being a feeling as if the skin were abraded. This pain was permanent. Blisters, fomentations, and frictions, had been tried in vain. It yielded, though not till after months, along with the complaint in the spine, to the horizontal posture, and to an issue in the back.

In each of the instances already mentioned, this affection of the spinal nerves was situated nearly about the 8th or 9th dorsal vertebræ, in which situation, as far as my experience proves, the disease is more frequently met with than in any other. It is obvious, however, that the symptoms attending a complaint of this description will be quite influenced by the portion of the spine which is affected, since the nerves have different distributions and functions, according to their origin.

Next in frequency to the middle of the back, I have observed this disease in the upper part of the neck, about the 2d or 3d cervical vertebra.

In one very complicated case, there appeared to be two sets of symptoms, each of which radiated from a tender part of the spine, as if from a centre. In this young lady, Miss M., there was pain in the left side, and numbness and pain of the left arm and leg, distinctly connected with tenderness situated about the lower of the dorsal vertebræ. This partial palsy, however, was strictly confined to one side. There was neither the pain along the insertion of the diaphragm, nor the paralysis of both of the lower extremities, which frequently attend caries or protrusion of the spine. There was also, after some time, a similar state of the 2d or 3d cervical vertebra, which occasioned pain in the left side of the neck, commencing between the angle of the jaw and the mastoid process. The pain, with great acuteness, also extended upwards from the neck to the back part of the head, and even to the forehead, following the ramification of the 2d or 3d cervical nerve; and on several occasions, probably by some nervous communication or sympathy, it induced an alarming degree of torpor or insensibility, which continued even for days. These two sets of symptoms, connected with two different portions of the spine, were so much mixed together that it was difficult to determine which belonged to one irritated part, and which to another.

\* A. S., a girl of 17 years of age, is at present (December 1827) a patient in the Glasgow Royal Infirmary. Her case is a well marked one of spinal irritation, chiefly in the middle of the back; but besides this, there is numbness and weakness of one arm, nearly amounting to palsy.

Posture alone was tried, but it was not found to be sufficient. As the paralysis of the arm and leg were the most urgent symptoms, issues were opened in the back, on each side of the pained part. The pain of the back and of the chest, with the palsy of the arm and leg, soon yielded. The headach, pain of the neck and shoulders, and fits of stupor, also disappeared, and it was presumed that the dorsal irritation had been the source of the whole disease. After reclining in the horizontal posture for three months, she was allowed to get up from her confinement. On the first, or at most, on the second day, however, after sitting up for a short period, it appeared that the cervical nerves were still in fault, the morbid symptoms arising from which had been merely suspended by the horizontal posture, but were again called into activity by the change to the erect position. The left side of the cervical vertebræ, near the head, again became acutely tender to the touch. The mastoid muscle on each side was rigid and projecting, so as forcibly to attract attention. The pain extended downwards to the left shoulder, and even along the back, nearly to where the issue had been, so as to create the decided impression, in the minds of the lady and of her friends, that the whole disease had returned. The pain also radiated upwards, over the integuments of the head, even to the forehead. The neck was altogether so tender, that it was not easy to say exactly where the morbid affection was situated, for neither in this, nor in any of the former cases related, was there the slightest inequality in the form of the spine, as far as could be discovered by the touch. As this patient had a decided objection to the use of blisters, from repeated experience of their inefficacy in removing her complaints, and from feverish feelings which they excited, and as she had been materially relieved by the issue in the back, a kidney-bean was inserted into an incision on each side of the spine, near the head. These issues occasioned great irritation. The original pain, shooting up and down, continued, and to this were added nervous feelings, and at last she got into a state of insensibility, which continued for two days. It was necessary to remove the issues. Soon after, I ventured to apply a blister to the nape of the neck, but, contrary to former experience, it acted like a charm, and relieved her from a state of suffering which had continued for a month. It was now discovered that the dorsal vertebræ were quite free from pain, and that the pain which had extended to the back had merely radiated from the neck. After the blister healed, the affected portion of the spine seemed to be limited to a small spot, on the left side of the 2d or 3d cervical vertebra. The pain again returned severely, shooting up over the head. A blister now had no effect in removing it. It was so acute, that it was judged necessary to pass a cord through the part. In a few days, erysipelas commenced around the seton, but almost immediately the severe pain of the neighbouring parts yielded. The erysipelas

extended over the head and neck, with smart fever. It then attacked the lips, and entered the mouth. The tongue became inflamed, swelled, and hard, so as to be nearly immovable. Immediately after, cough came on, as if the inflammation had extended down the windpipe to the lungs. This ended in phthisis pulmonalis, of which she died, after suffering in the usual way for several months. It was remarkable, that whenever the erysipelas commenced, the spinal irritation subsided, and she continued perfectly free of pain of back, or of any of her former ailments, till her death.

I have given an account merely of those symptoms which had been present in this patient for 18 months preceding her decease. She had suffered for a great length of time before that period under various anomalous symptoms, many of which, I am confident, had been connected with irritation of one portion or other of the spine. At one time, caustic issues in the loins had been of great use in removing a number of uneasy feelings, which appeared to radiate from that part of the spine; but I am confident, that for a much longer period the neck had been in fault, and that various nervous feelings, with repeated fits of insensibility, followed by total blindness, which lasted for several days, were connected with that irritation, although she had so many other complaints, that it was difficult to say what was the leading disease. I am certain, however, that confinement to the horizontal posture had the effect of giving great relief, and of simplifying the disease. It rendered the symptoms so much milder, that a number of nervous sympathies were prevented, and in this way allowed the attention to be fixed on those points of the spine, from which, in my opinion, all her other complaints, except the last fatal one, emanated. It should have been remarked, that in this case the digestive organs were healthy. Her appetite and general appearance were good. Her catamenia were quite regular, till phthisis pulmonalis came on. It should be noticed, too, that there was not the slightest curvature or projection of the spine.

When the affection of the spinal nerves is situated about the middle of the lumbar vertebræ, it is apt to occasion severe pain in some part of the abdomen. I have seen it of a spasmodic nature, attended with flatulency, and occupying apparently the arch of the colon; and in one case, it seemed to be fixed in the caput coli. In these cases, the severity of the complaint may appear to call for powerful evacuations, from the apprehension that inflammation exists; whereas, if the attention be directed to the share which nervous irritation from the spine has in increasing the acuteness of the pain, very active depletion will not appear to be essential.

M. M. was a patient in the Lock Hospital, in May 1821. During her confinement under a mercurial course, she complained of acute pain nearly in the situation of the caput coli, with costiveness, and occasional vomiting, a

quick pulse, and other feverish feelings. She was repeatedly bled, blistered, and purged, and took magnesia in large doses, with great benefit. This complaint, however, returned severely every two or three days; but at last it was ascertained that there was a marked pain about the middle of the lumbar vertebræ, and that whenever this part was pressed, the uneasiness extended forwards, exactly to the affected part of the abdomen. Attention was now paid to the spine. The part was cupped and blistered, with immediate relief to the pain. The horizontal posture was rigidly enforced. She had afterwards a return of this complaint. The same means of cure were employed, and she got well, much more rapidly than when powerful evacuations were used. I should have mentioned, that the tenderness of the abdomen to the touch, appeared to confirm the idea that the part was in a state of inflammation.

In the following very complicated case of spinal irritation, there was not the least appearance of protrusion or of irregularity in the shape of any of the vertebræ; but there were three, or even four separate parts of the spine in fault. Sometimes one of these parts was in an irritated state, sometimes another, sometimes two of these were affected at one time; and according to the situation of the irritation, the symptoms varied.

A. R., aged 23, was admitted into the Lock Hospital, in May 1821. She had primary venereal complaints of little consequence; but besides these, she had completely lost her clear voice for several months. She could only speak in whispers. After being in the hospital for some time, this symptom was attended with pain at the larynx, and with dyspnoea. It was ascertained that one of the upper cervical vertebræ was tender to the touch, and when this was pressed, the pain extended forwards, so as to increase the pain in the larynx. Leeches and a blister were applied to the neck, and in two days the pain of the larynx and the dyspnoea were removed, and she at the same time recovered her clear voice, which had been lost for three months.

It would be needless to enumerate all the minutiae which occurred for eight months in a most complicated case; but I shall mention merely the general results.—About the same time with the former complaints, the lumbar part of the spine was affected in two separate points. There was pain within the abdomen, extending upwards and downwards, which became severe whenever the spine was firmly pressed. There was also pain in voiding urine, pain in both groins, palsy of one lower extremity, and at last complete retention of urine, so as to render the use of the catheter necessary for three weeks. These last symptoms seemed to be connected with a different part of the spine from that formerly mentioned, since pressure on this other tender portion of the spine excited the pain in the groin and in the urethra. The retention of urine returned occasionally during the eight months she was a patient in the Lock Hospital.

At other times, one of the dorsal vertebrae was affected, with the usual well-marked pain in the thorax, and with violent pain in the stomach and abdomen. At last, the neck became again irritated, and soon after that the head. She was quite deranged, requiring the strait-jacket. She then became comatose for some days. She recovered from this state, by bleeding and blistering. For a time she remained quite blind of both eyes, after that of one eye; but ultimately she recovered the use of both. The catamenia now returned profusely, after having been suppressed for a year. There was also the discharge of much coagulated blood from the uterus. After this she gradually recovered strength, and was dismissed on the 3d of December almost well.

These various affections, all immediately arising either from disease in the spine or in the head, returned more than once, and they gave rise to a strange combination of nervous and other complaints. Copious bleeding, blistering, caustic issues in two separate parts of the spine, purging, and other means, were necessary at the time, and occasionally they were of service. These complaints ultimately subsided, after the return of the menstrual flow, which had been suppressed for a year.

Nearly allied to the case of A. R. is that of C. D., though it differs in some respects. In this girl, there was decided lateral curvature of the spine, though not to any remarkable degree. She was admitted into the Lock Hospital, in July 1822, with primary venereal sores. She had a singular pink complexion, which augured some peculiarity of constitution. She was easily flushed and made feverish, but was in a tolerably good state of general health. Her catamenia were quite regular, and in this respect she differed materially from A. R.\*

She soon began to complain of pain in two separate parts of the spine. *First*, in the 9th or 10th dorsal vertebra. When this was pressed, the pain extended up the spine, even as far as the head. She had also occasionally a burning shooting pain in the head. *Second*, in the 2d or 3d lumbar vertebra. This was really a very severe complaint. The pain extended forwards to the edge of the ribs on the left side, and down the outside of the thigh. There was also a painful feeling of numbness of the whole of the left lower extremity, producing complete lameness. These symptoms, in some respects, resembled nephritic pains, but there was no stomach complaint, nor apparent disorder of the urinary organs. She was fixed to one posture in bed by this

last complaint. The slightest attempt to move was agonizing. By various means, these complaints were materially abated; but on the 24th of November, when confined to bed in the horizontal posture, a new part of the spine became irritated. She complained of pain in the right side of the third cervical vertebra, extending down the neck, which was actually tender to the touch, to the top of the right shoulder, and along the arm to the wrist. This pain also extended upwards to the crown of the head, on the same side. It did not affect the left side, either of the neck or head. She described this pain as more severe than any she had ever before felt; and even worse than the burning pain of hospital gangrene. The pain was always present, but the severity of it increased five or six times through the night, and during this exacerbation, which continued for about fifteen minutes, she became quite blind. The sight returned gradually, with the appearance of luminous stars floating before her. After this new pain attacked her, she vomited, with a bitter taste, every thing she swallowed, and even with such straining, as to be mixed with blood. By copious bleeding, the severity of these symptoms abated, though the vomiting occasionally returned. A seton was passed through the neck.

It is worth remarking, that by the report of the 14th December, it appears that she and her fellow-patients observed that she had an unusual degree of rigidity and projection of both of the mastoid muscles, and that this spasmodic action preceded the attack of vomiting, and the increase of pain down the shoulders.

On the 27th of December, the day after a number of leeches had been applied to the neck, on the idea that the vessels of the spine were in an enlarged state, she became much worse. The pain spread over all the head, with flushing of the face. The stounding pain in the head appeared to threaten an effusion on the brain. Erysipelas came on over the face. The cord began to discharge, and she became very materially relieved.

This, then, is another instance of material relief, arising, in a severe complaint of this description, from an attack of erysipelas. It may be remarked, that at different times, whenever the irritation in the neck was increased, she rejected from her stomach any thing she had swallowed. This, however, in general, was merely eructation without nausea; though occasionally there was nausea and vomiting, with severe straining.

This patient at last recovered so much, that she left the Hospital in a state of moderate ease. Her back was weak, and there was distinct lateral curvature of the spine, though not to any great degree.

*Remarks.*—I am fully sensible of the difficulties which surround every inquiry where the nervous system is concerned, and of the little chance that exists of our ever being able to form views on this subject, materially more correct than those which we possess at present.

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\* I may here mention, though it is not much connected with this subject, unless to prove the peculiarity of her constitution, that although her bed was placed in an airy and large ward, and close to a window exposed to the west, she soon took hospital gangrene. It attacked a boil on the arm. This made rapid progress, and was attended with all its usual characters. This sore had never before appeared in the Lock Hospital.

But although the precise nature of the connexion that exists between these nervous fibres and the other parts of the animal system, be quite unknown, yet we are certain that this union is absolutely necessary for sensation, for motion, for the performance of every function, and, of course, for the existence of life. No one doubts that all those necessary actions, which are constantly going on in the viscera of the thorax and of the abdomen, as well as in every other part of the body, are immediately and necessarily under the influence of the nerves.

To guard against those dangers, which would arise from the interruption of this necessary influence of the nerves, there are no doubt numerous communications and networks; but still we are certain, that each individual nerve is of some use, either of greater or less importance.

We can therefore easily conceive that pressure, or any other irritation, applied to the spinal nerves, should produce very sensible effects, either in occasioning pain, or in deranging the healthy action of the muscles, or of the viscera, to which they are distributed. We find accordingly, that in the cases related, such derangements did result from those causes.

The highly important results which have followed from the brilliant discoveries of Mr. Charles Bell, on the functions of nerves, have already thrown much light on many points connected with the nervous system, which before his time were involved in obscurity. It is exceedingly probable that these cases afford examples of diseased states of the two sets of spinal nerves, and that future experience may enable us to arrange these into separate classes. I merely wished, on the present occasion, to place in contact with each other, the different varieties of these diseases, chiefly in females, which have occurred in the course of my own practice, within a short period, as a sort of nucleus around which, in future, I might dispose other examples.

In several instances, in males, I have seen a disease in some respects similar to the one which is here named spinal irritation, arising, as in that complaint, from irritation applied to the origin of a single nerve at the spine, and corresponding also with it in several other particulars. This, however, in males, is a rare disease, and when it does occur, we can generally, though not always (since it occasionally arises from acidity of the stomach or of the duodenum) trace it to curvature of the spine, or to inflammation resulting from some injury which the spine has sustained.

These affections of the spinal nerves appear to divide themselves into two classes:—

1. Those in which there is merely a morbid sensibility of a single nerve.
2. Those in which there is a more general and constitutional irritability, in which the irritation is apt to affect different parts of the spine in succession, and to occasion a whole train of singular symptoms.

This last form, I believe, is confined strictly to females, the other is occasionally met with in males. Those two forms of spinal disease, though generally separate, are occasionally found to be so much combined, that it is difficult to say what symptoms belong to one form, and what to the other.

A great proportion of the examples of the first class which I have seen, have been in female patients in the Lock Hospital. Perhaps nine-tenths of the inmates are young women, between 15 and 20 years of age. At this important period of life, when the constitution has only lately changed, and when the growth of the body is perhaps more rapid than at almost any other, it is obvious that intemperance in all its forms, and want of proper air and exercise, will have most material influence in deranging the health. A great number of these females, many of them slender and delicate, and without proper clothing, are exposed to the inclemency of the weather at all seasons and hours, perhaps in a state of intoxication; or not unlikely, they remain at home ruining their health by hard living, and suffering as much by confinement to a close room, as they would by exposure to cold. Under the operation of these various noxious causes, we are not to wonder if the spine should frequently become affected, and we accordingly find that a number of those females who are admitted for the cure of venereal diseases, suffer from affections of this nature. This complaint is rarely met with in country girls, whose muscles and solids are invigorated by their mode of life. We can easily conceive, indeed, that the muscles arranged along the spine should be most materially strengthened by bodily exercise, since on every exertion they must be constantly and powerfully in action, in altering or even in preserving the erect position of the body.

I have to repeat what was formerly briefly observed, that I consider these cases as very materially different from those which are known to arise from inflammation or caries of the vertebræ. These latter diseases are met with more usually at a much earlier period of life than the former. They occur perhaps with equal frequency in either sex. With them, there is often paraplegia, or perhaps a rigid contraction or spasm of the muscles of the lower extremities. The urinary bladder, or bowels, are frequently torpid or paralytic. The spine protrudes or is irregular in its shape, and there is a feeling of debility, or of pain, extending along the insertion of the diaphragm to the ribs, which is very characteristic of a diseased vertebra in the most usual situation. In the disease, however, which forms the subject of this essay, and which we have named Irritation of the Spinal Nerves, there is no alteration in the shape of the spine, as far as can be observed by the eye, or by the touch. There is rarely any paralytic tendency in the limbs, bladder, or bowels; and if there should be any weakness of the extremities, which is rare, this is confined to one side of the body. It is besides, in general, a disease of greatly less danger than the former, though an ex-

treme case of the second class is certainly a very tedious and formidable complaint.

In this disease of the spinal nerves, I have rarely seen the pain of the chest in both sides of the thorax at one time; and it differs quite from that feeling of pain or of fatigue, which extends across the whole thorax, where the diaphragm is attached, and which is so frequently present when the vertebræ are inflamed or carious.

Since these two states of the spine differ so much from each other, we have next to inquire what is the nature of this difference.

I conceive that in the carious spine, the morbid symptoms arise from compression or inflammation of the whole spinal cord; whereas, in this other disease, the spinal marrow is uninjured, but we have some degree of pressure applied to the spinal nerves, as they issue from the vertebræ. We accordingly find that in cases of protruded spine, we have all those symptoms which arise from injury of the whole mass of the spinal marrow; whereas in the other disease, in its most simple form, we have only those complaints which arise from the compression of a single nerve. We have merely pain at the sentient extremity of this nerve, somewhere in the thorax or in the abdomen; and pain in the back, occasioned by the irritation of those nervous twigs which have the same origin as the larger branch, but which pass backwards to be distributed on the muscles on the posterior part of the spine. In cases of longer continuance, or of greater irritability, from nervous communications with the great sympathetic nerve, or with other nerves in the thorax or in the abdomen, various distant sympathies and anomalous nervous feelings are brought into action; and, since the proper action of every organ depends on the influence of the nerves, ultimately the organs of digestion, and the various glands in the abdomen, are deranged in function, and at last in structure. We accordingly often find that such changes are met with in severe cases of this disease.

The frequency with which this painful affection occurs in delicate constitutions, were growth is rapid, can perhaps be explained if we keep in mind the complex and varied structure of the spine. This organ is composed of a series of joints, furnished with numerous ligaments and muscles; for we are to consider each bundle of muscular fibres as a muscle, with an independent action, although the anatomist brings whole classes of them under one name. There are many separate muscles, for instance, comprehended in the multifidus spinæ, and in the sacro-lumbalis. The motions of the spine, too, are frequent, and it has, independently of occasional and great exertions, the weight of the upper parts of the body to support. It is apt to suffer from sprains, and from wrong posture of any kind. Even when lying in bed, it is not exempted from the effects of over-exertion, or of wrong position. How often do we find these complaints brought on in delicate girls, or increased by the unequal surface of a feather bed, or of a

bolster which is too high and firm! Even in many adults, who are by no means feeble, we find that a soft feather bed occasions pain in the back and in the neck; but if a feeble growing girl be confined to bed by any cause, the spine will be much more liable to become sprained, or perhaps even misshapen, from any inequality in its surface. It is reasonable to suppose, that from any such cause, especially if frequently repeated, or long continued, one or more of the ligaments or muscles of the spine should be stretched or strained. A slight inclination to one side, probably only at one joint, will be the effect, and in this way the nerves issuing from one side of the spine, surrounded with a bundle of fat and cellular membrane, will be compressed and irritated.

That this slight inclination to one side is the cause of the pain, in the *simple form* of the disease, appears to be proved by the fact, that only one of the spinal nerves is affected at the commencement of this disease. There is pain merely in a small space, on one side of the thorax, and there is more uneasiness on that side of the spine which corresponds with this pain, than on the other. This distinctly proves the limited nature of the disease at first. Most likely, after some time, some degree of inflammatory action may result from the pressure on the nerve. Lymph will be effused, and thus the pressure on the nerve will be increased. Besides, the muscles and ligaments of the spine become more strained. The intervertebral cartilage on one side becomes diminished by pressure and by absorption; on the other side it is proportionably thickened. We may conceive, therefore, that after a certain time, even some degree of irregularity in the shape of the spine may be occasioned. The disease becomes more serious, and more difficult to remove; and a greater number of nervous sympathies are brought into action.

As a farther confirmation that this view of the nature and origin of this most common form of the disease is correct, it is observed, that at the commencement the pain of the breast goes off whenever the patient lies down in the horizontal posture, but that after the complaint has been fixed for some time a change of posture alone does not so certainly relieve the uneasiness in the breast.

We have found that the affections of the spinal nerves occurs much more frequently in the lower of the dorsal, or in the upper of the lumbar vertebræ, than in any other situation; and by looking to the form of the skeleton, we can readily account for the greater liability of this part of the spine to this disease. The ribs, united to the dorsal vertebræ and to the sternum, form the whole chest, as it were, into one piece. From this union of the different parts, we observe that there is but little motion in that portion of the spine which forms the posterior part of the chest. On the other hand, the lower lumbar vertebræ are larger than those above, and are fortified by strong muscles. Besides, the motions of the lower lumbar vertebræ are very limited, from

being fixed to the pelvis, which may be regarded as a firm support. There is, therefore, a greater extent of motion in that portion of the spine which is placed between these two fixed points, than in any other, and we accordingly find that affections of this nature are much more frequently met with about the lower dorsal, or upper lumbar vertebræ, than in any other part. This weak and moveable portion of the spine has also to support the whole weight of the upper part of the body. It may be considered as the centre of motion.

The nerves of the second or third cervical vertebræ also frequently suffer from diseases of this nature. This part of the spine is nearly similarly situated as the upper part of the lumbar vertebræ, since we are here to consider the chest as the fixed point, corresponding to the pelvis. To this the lower of the cervical vertebræ, surrounded by strong muscles, are fixed, while the upper part of the neck has a more free and unrestrained motion.

It is exceedingly probable, too, that the abrupt shape of our bolsters and pillows increases the disposition to disease at this part of the spine.

We shall now revise the various cases, and make some observations on the different symptoms which occurred in each.

When the disease is fixed in its most usual situation, namely, near the lower part of the dorsal vertebræ, the pain is felt at a distance from the spine, in some part of the thorax. Occasionally, however, there is merely a feeling of oppression in the chest. This distant pain is certainly connected with the distribution of the sentient extremities of the affected nerve; for, in other instances, irritation of the trunk of a nerve is felt where the nerve is ramified. In many other instances, then, in these affections of the dorsal nerves, we find that when the origin of a nerve is irritated, the pain is chiefly felt at the sentient extremity of it; and a knowledge of this fact is frequently of great use in rendering our practice efficient. We find, indeed, that local applications to the seat of the pain have nothing but temporary effect; whereas, when our attention is directed to the distant cause, we often succeed by the most simple means.

Some time since, I was requested by a medical friend in town to visit, with him, the captain of a coasting vessel, who ten days before had received a severe blow on one of his thighs. The pain and shock were considerable at the time; but he was out of bed, and apparently recovering, till four days before I saw him. Then he complained of a burning feeling in the groin of the bruised thigh. This had become very acute, and was attended with spasms, apparently of the sartorius muscle, excited by the most trifling motion. He was literally fixed to one posture in bed, from the agonizing pain and spasm, produced by any attempt to move. Leeches in great numbers had been applied to the groin. He had been freely bled, and had used opium, laxatives, &c. without any relief. On examining the spine near the sacrum, a considerable degree of ten-

derness was detected there, and when this was touched, the pain extended down to the affected groin. Leeches and a blister to the sacrum gave immediate and complete relief. In fact, one of the lumbar nerves, distributed to the sartorius and neighbouring muscles, was irritated and inflamed, at its origin from the spine. The pain was felt at the sentient extremity of the nerve; but local means, applied to the spine, where alone efficient in removing the complaint.

When there is merely the feeling of oppression in the chest, this may perhaps be connected with pressure applied to the nerve of motion, instead of the nerve of feeling; since, from the experiments of Mr. C. Bell, we know that the two nerves which arise from each part of the spine have different functions. The relief of this pain by the horizontal posture is readily explained, by the removal of pressure from the trunk of the nerve, as it passes between the vertebræ.

It has been remarked, that the pain which is perceived when the back is pressed, is connected with that branch of the spinal nerve which passes backwards to be distributed to the muscles of the spine. I have seen several instances in which all the symptoms of this disease were present, with the exception only, that there was no pain when the spine was pressed. There was local pain in the side, increased by fatigue, and relieved by posture, and applications to the spine. In these cases, I was inclined to theorize, in supposing that the nerve of feeling which passes forwards was irritated; but that the branch which passes backwards to the muscles of the spine, was free of irritation. The complaint which forms the subject of this essay is often so strictly local, that this supposition does not appear to me to be unreasonable.

Occasionally disorder of the stomach and bowels is joined to this affection of the spinal nerves, and appears to arise from it, since it yields, if the irritation of the spine is removed, and returns whenever this irritation is renewed. On the other hand, however, I have seen the spinal irritation, with pain of the breast, yield at once to the removal of a load from the bowels by an active purgative, showing distinctly the immediate connexion that exists between the spinal nerves and the intestines; and proving that, on some occasions, the affection of the spine disorders the digestion, but that, in other instances, indigestion is the primary disease, and the spine merely sympathizes with it. In several instances, there was an uncommon degree of acidity of stomach joined to this disease. This had been treated without effect in the way that was effectual after attention was paid to the spine, as well as to the stomach, so that I was disposed to conclude that the excessive acidity was connected with the spinal irritation.

We occasionally meet with examples of dry, loud, convulsive cough in females whose spinal nerves are irritated. I believe, however, that this cough is not occasioned directly by the irritation of the spine, but that the stomach and

bowels are deranged by this irritation, and that the cough is the effect of this derangement. I form this conclusion from a number of instances of nervous or convulsive cough, which have occurred to me, in most of which, though not in all, a fault in the digestive organs appeared to be the immediate cause of this symptom.

In irritable constitutions, this local complaint is often the cause of a whole train of nervous feelings and sympathies, especially if, from inattention to the source of the disease, an attempt is made to invigorate the body by exercise. It is plain, that in such cases, we can scarcely expect to remove these symptoms, unless we attend to their origin, and that exercise, instead of removing the complaint, will increase and confirm it.

We find that many painful affections can be referred to pressure or irritation, applied to the trunk of a nerve at a distance from the pain. Some of these pains are confounded with rheumatism, or they get the vague name of nervous pains. It is of great consequence, to be aware how frequently these complaints depend on a distant cause. When numbness, and want of power of moving the arm or leg, are joined to this disease, we conclude that these symptoms arise from the local irritation of the nerve, spreading to communicating nerves; not from any general affection of the head or of the spinal marrow. At least, in several instances that I have seen, I have not been able to discover any other cause. The palsy yielded to the treatment of the local disease in the spine. Paralytic complaints of this description afford a much more favourable prognosis, and are surely very different from that paralytic affection, which arises from injury of the whole of the spinal marrow, or from that which depends on injury of the brain.\*

\* Since writing the above, I have had one well marked case of spinal irritation in the Infirmary, (A. S., a girl of 17 years of age,) in whom there was, in addition to the usual symptoms of this disease, palsy nearly complete of the left upper extremity. The arm was numb, and she could only move one of the fingers. She also lost the power of swallowing any thing but liquids. These two symptoms remained, though almost every other yielded to the means employed. She had no pain in the head at this time, at least no fixed pain, nor any apparent affection of the mind. At one time, the sight was dim and the pupils dilated; but I conceived that these, as in other cases, were owing to spinal irritation of the nerves of the neck, not to any affection of the brain. The obstinacy of these symptoms, however, (for the arm had been paralytic for six weeks,) induced me to apply a large blister to the head, and certainly immediately afterwards the local palsy, the dysphagia, and the other symptoms of spinal irritation, which were lurking about her, yielded completely, and she very soon left the Hospital nearly recovered in strength, after having been three months a

We occasionally observe singular pains and feelings, and spasmodic twitches, about the neck and shoulders, arising, no doubt, from the nervous communication of these parts. I recollect to have seen two cases in which the cervical nerves were affected, in which the prominent feeling was that of a hair, stretched across the throat, occasioning much irritation and frequent inclination to retch.

Some years ago I attended a medical gentleman, whose symptoms depended on local irritation of the nerves of the neck, by a cause different from spinal irritation, but corresponding with it in producing action at a distance. In this gentleman, there was a most violent convulsive action of the right trapezius muscle, occasioned by a small tumour, irritating the accessory nerve of Willis, as it passes through the sterno-mastoid muscle. This spasmodic action or convulsion began in the right trapezius muscle, but it set other muscles into action, and the effect was most singular and distressing. He shrugged up his shoulders with such force and frequency, that in a short time he became quite exhausted by the painful exertion. This disease continued for many years, occurring at uncertain times, both day and night, rendering confinement to the house, large doses of opium, and retirement from business, absolutely necessary. At last it

patient in the house. From the result in this instance, I shall certainly in future, in these anomalous cases, direct my attention more particularly to the head, and, if obstinate, have no hesitation in bleeding and blistering this part. The head and the spine are so immediately connected, that affections of the head occasionally imitate those of the spine. Dr. Baillie, for instance, has ascertained, that paraplegia, in many examples, may depend on an affection of the head; and on the other hand, we occasionally find that a disease of the spine produces derangement of the functions of the brain. From this analogy, and from what occurred in the case of A. S., I would have no hesitation in these cases, in directing my attention more particularly than I have formerly done to the head. We know that fluids on the surface of the brain readily pass into the theca vertebralis. In a case at present in the Infirmary (H. K.,) the head was affected to an alarming degree with stupor, tinnitus aurium, and pain, for ten months. The primary disease, however, appeared to have been seated in the lumbar spine, and accordingly all these affections of the head gradually yielded to an issue on the diseased vertebra. I therefore again repeat, that in these cases of spinal irritation, we ought to be aware that the whole train of symptoms may depend on disease of the head, as well as of the spine. The affection of the sight, so common in these complaints, will be well explained by this view of the subject, and perhaps even the partial spasm of the muscles fixed to the spine, which we shall afterwards point out as being much connected with these ailments, will receive illustration by the same idea.

gradually subsided; and I believe that this gentleman has been free of the complaint for several years.

There is no part more frequently pained, from nervous irritation, than the pericranium or scalp. The pain is often acute, and if we examine the parts minutely, we find that it shoots along the various nerves which are ramified over this tendinous membrane. If we press immediately behind the mastoid process, the pain passes upwards along the cervical nerve which takes that direction, and ramifies even as far forwards as the forehead. The frontal branch of the fifth pair of nerves is often similarly affected, from cold or from disorder in the stomach; and the temporal branch of the fifth pair, when the salivary glands are irritated by mercury. I allude chiefly, however, to the first mentioned of these painful affections of the scalp, viz. that which shoots upwards from the neck, and passes behind the mastoid process over the head. This is a frequent complaint in delicate girls, and is occasionally acute. It often takes its origin from irritation at the origin of the nerves between the vertebrae, in consequence of the sudden bend of the neck from lying on a high bolster, and is remedied by very simple means. It is occasionally rheumatic, and is owing to exposure to cold. Sometimes this pain of the neck and scalp is peculiarly painful and obstinate, as in some of the cases mentioned.

In a great number of these cases, in which the cervical nerves are in a state of irritation, the sight is for the time injured. When the pain shoots from the neck over the head, there is generally either great sensibility, or in other instances great insensibility to light, with dilatation of one or of both pupils. The excessive sensibility of the eye is much more frequent than the opposite state, and it often becomes necessary in these cases to exclude the light entirely. I know of one young lady, who for years has been obliged nearly completely to darken the room; and I recollect of another similar instance, in which there is now a complete recovery. Indeed a great proportion of the cases suffer pain from the exposure of the eye to light. This sensibility and insensibility to light are apt to alternate in the same individual. In the lady mentioned above, as being much disturbed by excess of sensibility, the opposite state occasionally prevails; when every object looks dim and diminished in size, as if it were placed at a great distance. Some of these patients occasionally have the sensation of a green or of a blue light before their eyes; and they remark, that the intolerance of light is more remarkable, when the irritation in the neck is most severe.

It was stated, that in one case the pain was followed by fits of insensibility, which even remained for days.

It was also mentioned, that in another case, a severe attack of pain of the scalp was followed by complete blindness. This state continued for some time; after which it subsided.

These fits of pain and blindness occurred even so frequently as five times in the course of the night. In a young medical gentleman, who consulted me some time since, the pain of the scalp had nearly the same direction as in the former cases. It came on in fits, and during these he became blind. After some time the severity of the pain went off, and along with it the affection of the sight.

It was formerly remarked, too, that Miss M. and A. R. were completely blind for some time. It appears, therefore, that this symptom is not unusual in cases of spinal irritation. I cannot, however, explain, by any nervous communication, how this should happen. I can only say, that I have observed partial or total blindness in several cases, in which the upper cervical nerves were in an irritated state; and from the frequency of its occurrence, I must suppose that it was connected with this irritation, and arose from it.

As this symptom occurred, in general, during my absence, I am not sure whether, in all the cases, the pupils were dilated or not, during the state of blindness. In A. R.'s case, and in that of A. S. they were dilated, and I presume that such was also the case in the others, as that was the character of the eye when I saw it next day.

In at least two of these severe cases, there was almost immediate vomiting of every thing that was swallowed. Perhaps eructation is a better term than vomiting, since there was nausea only occasionally. This symptom was most likely connected with irritation of the 8th pair of nerves, or with that of the phrenic. The food was eructated quite unchanged in its appearance. From having seen this symptom present during the acute stage of these diseases, and subside whenever the irritation abated, I should feel inclined to examine the spine carefully in other instances, where eructation of the food or vomiting was a prominent symptom. In the case of C. D., every sort of food for days together was almost immediately rejected from the stomach.\*

In taking a review of the different cases, which have been brought forward, there appear to be two marked varieties. We have, in the first place, a simple form of the disease, of very frequent occurrence, and easily removed, in which the morbid affection seems to be limited to a minute space. There is pressure merely of those nerves which pass

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\* I have lately seen two cases of spinal irritation, in which vomiting was a very prominent and urgent symptom. In the case of E. M., who is still (1828) a patient in the Infirmary, there was constant and even alarming vomiting of every thing swallowed, joined to irritation of various parts of the spine, and ultimately of the neck. This last was relieved by a blister, but the vomiting was not. It resisted anodynes, blistering the stomach, &c., for weeks, but at last it yielded at once to an anodyne enema, which was occasionally repeated to prevent the return of the vomiting.

out between two of the vertebrae. But there was another form of spinal irritation, which varied considerably from the other, and was infinitely more serious and obstinate. I allude to that more extensive tendency to disease, which has been observed in several of the cases.

Even in these, however, although the irritation wandered from one part of the spine to another, still the morbid symptoms at the time seemed to be confined to individual nerves, as they issued from the spine, not to be extended to the entire body of the spinal marrow. In several instances, these two complaints were so much combined, that we were unable to separate them, and we had only to give the history of the case, with all its anomalous symptoms.

We may entertain different notions regarding the nature of this singular form of spinal irritation, and it need scarcely be remarked, that it is of great consequence that the theory we form should be correct, since our practice may be materially influenced by it.

1. We might suppose that it is connected with a plethoric, or varicose state of the blood vessels of the spinal marrow. It is accordingly more frequently observed at the monthly period in women, than at any other time, and at this period we know that the blood-vessels of the lumbar region, at least, are in a distended and active state. We know, too, that this disease is remarkably moderated by local bleeding. The local and limited nature of the irritation, however, induces us to conclude that this theory is not the correct one. We find that it is confined chiefly to two parts of the spine, viz. to the middle part of the back, and to the upper part of the neck; and in these situations, frequently even to a single nerve with its ramifications. If, however, the disease depended on a plethoric state of the whole spinal marrow, or of a part of it, we ought to have an affection, much more general and extensive.

2. We might say that this disease was connected with relaxation of the muscles and ligaments of the whole spine, but that this weak state of the solids would be chiefly perceived in those parts where the motion is the greatest. This notion, however, will not account for the phenomena of these diseases. We often find great emaciation and debility of all the solids, without there being any tendency of this nature. In short, the disease is not confined to states of debility and emaciation. A. W. suffered severely from this disease, at last she was attacked with typhus fever, and was much reduced in strength by it. The spinal irritation went off whenever the fever came on, and did not return.

It is exceedingly probable that there may be some slight inequality in the position of one vertebra on another, as appears to take place in the more common and mild form of irritation of the spinal nerves; but still in this more serious form, there seems to be something added to this, which leads us to the supposition;

3. That the disease depends chiefly on pecu-

liar sensibility and irritability of the spinal nerves. We can easily conceive that this should be chiefly, or perhaps solely felt, at those parts of the spine, where the motion of one vertebra on another is the greatest, viz. at the loins and in the back. In the sound and healthy state of the nerves, the slight pressure occasioned by the motion of one of the vertebrae on another, is not sufficient to occasion pain; but when the nerve is too sensible, then this same pressure is sufficient to produce pain and irritation, varying according to the excess of sensibility. In very irritable constitutions, even the slightest motion may excite violent effects. In the cases described, indeed, the pain was occasionally very acute, even independently of motion; and was most likely connected with the spasmodic action of the muscles of the spine, drawing it to one side, and in this way increasing the pressure on the irritable nerve. In confirmation of this notion it may be stated, that in several of the cases, I have observed a marked stiffness and spasmodic state of the muscles of the neck. In two cases, it was remarked that the mastoid muscles, at one time, were rigid and projecting like boards.\*

The more simple form of spinal irritation is not entirely confined to females; but it is much more frequently met with in them than in males. It is connected with the more delicate frame of females, with their mode of life, their want of exercise, and with certain peculiarities in their dress and education. Occasionally, however, we do meet with it in males of delicate constitution, or in those who have strained, or otherwise injured the spine.

The other form of spinal disease, I conceive, is quite peculiar to females. It is a most formidable and obstinate complaint, and depends most probably on the peculiar action of the

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\* Within these few weeks (January 1828,) I have had two cases of serious disease from spinal irritation in the Infirmary here, and one in private practice, in which the pain, shooting from behind the mastoid process over the scalp of one side of the head, appeared to be connected with spasmodic stiffness of the mastoid and other muscles of the neck of the same side. This connexion seemed to be quite decided in these cases of spinal irritation of the neck; but it is not so easy to ascertain whether or not partial spasm of the muscles, arranged along the spine, exists in cases where the dorsal or lumbar spine is irritated. I have not as yet paid attention to it in these situations; but from the analogy with similar affections of the cervical nerves, I conceive it probable that it does exist. An irregular practitioner in England is celebrated for his success in the treatment of these cases. His practice consists solely in long continued friction of the affected parts of the spine, joined to posture. Now, it is easy to conceive that such practice may have influence in abating a partial spasm of the muscles of the spine, and that the regular surgeon may improve his treatment of these complaints by following a similar plan.

uterine system. In one of the cases related, and in two at present (1827) in the Infirmary, the catamenia were suppressed for a long time, and in the former these complaints went off whenever the catamenia re-appeared, which was profusely. In most of the other examples, however, the catamenia were quite regular, so that there is no necessary connexion between amenorrhœa and this disease. Still, however, it is so purely a female disease, that I conceive some unknown change in the uterus must be present; perhaps a plethoric state of that viscus, with a similar condition of the blood-vessels of the spine.

There are several other diseases different from those which have been mentioned, in which also the spinal nerves are irritated. I shall take merely a short notice of these.

In painful affections of the throat, we often find that the cervical vertebræ are tender, and that there is a distinct connexion between the pain of the one and the tenderness of the other. When we touch the spine, the pain extends forward to the throat. This is remarkable in cynanche maligna, in which disease the patient often complains severely of the neck, and in unfavourable cases, there is great stiffness, as well as pain.

In several instances of ulcerated sore throat from lues venerea, or from mercury, I have seen this pain in the neck a very prominent symptom; and when the neck was touched, the pain extended forwards to the throat. In cases of this description, a blister applied to the nape of the neck is more effectual than to the throat. The propriety of applying a blister to the nape of the neck, in these cases, should probably be determined by the presence or absence of pain in that part. Where this is present, I have seen the most marked improvement follow the blister.

I have repeatedly seen a very painful affection extending from the 3d or 4th dorsal vertebra, forwards below the scapula, and down the arm. The pain is felt entirely in the arm, but when we press on the spine between the scapulæ, there is pain of the spine, distinctly connected with, and increasing the pain of the arm.

In one of these cases, H. L. (December 1823,) this pain totally prevented sleep for several nights. It affected the left mamma, and the whole of the left arm. When pressure was made near the spine, the pain extended through all the affected parts. Whenever a small blister near the spine began to rise, the pain was immediately removed, and did not return. I have seen many similar cases, in some of which percussion, applied near the spine, speedily removed the pain.

Pains in the spine, nearly allied to the subject of this essay, are frequently connected with the uterus, either during the state of menstruation or in diseases of that viscus. These are usually about the 2d or 3d lumbar vertebra. Pains in the back are well known to be frequent, during the monthly period in women, and we often at the same time observe severe spasmodic affections, in some part of

the abdomen, most usually of the arch of the colon or of the caput coli. I have often been able at once to trace these pains to irritation of the spinal nerves, by the usual examination of the spine. Immediate relief was generally attained by a blister of a minute size, or some stimulating application made to that part.

During my attendance on the Lock Hospital, I saw repeated instances of inflamed uterus, apparently connected with the peculiar profession of the patient, and in each of these, there was a very decided and marked affection of the spinal nerves. In these patients, the os uteri was prominent, hard, and acutely painful to the touch, and the connexion between this part, the spine, and the groins, where the round ligaments terminate, was rather singular. In the case of J. S., who had acute hysteritis, when the pained part of the spine was pressed on the right side, the pain extended to the right groin, when the left side was pressed to the left groin, and when the middle of the spine was touched, the pain was felt above the pubis, apparently in the uterus. In the other cases of hysteritis, this singular union was not looked for, but in the whole, there was a marked connexion between the spine, the uterus, and the groins. I found too that local means, especially blisters to the sacrum, afforded very marked benefit in abating the severity of the disease.

Some time ago, along with two medical friends, I visited a gentleman in whom there was pain around the anus, increased by going to stool, by motion, or by the slightest touch, so severe that he was fixed by it to one posture in bed. The pain could not be attributed to piles, or to any obvious cause. It was much increased by purging. It yielded in part to the application of leeches around the anus, but it completely subsided, whenever a blister applied to the sacrum began to act. It seems probable that the nerves arising from the sacrum were irritated in consequence of enlargement of the blood-vessels of the part, and that this state was removed by the leeches and blister.

I have observed in a great proportion of the numerous cases of painful and diseased liver that I have met with, that there was a distinct painful tenderness of the spine, about the 9th or 10th dorsal vertebra. Pressure there immediately excites pain in the region of the liver. This pain is almost always on the right side of the spine. There are a few exceptions from this general rule, but where there is much pain in the liver, there is generally also pain of the spine. Pain in the right side from spinal irritation alone, is often a source of error in the treatment of diseases of the liver and of the stomach. It is often mistaken for disease in the liver, and where it has been originally connected with a fault in that viscus, it may continue from spinal irritation alone, after the diseased state of the liver has been removed.

I have also repeatedly observed this pain of the spinal nerves, in cases where the stomach was much disordered, especially where it was

very acid. In such a case the pain shooting forward from the spine to the region of the liver, may give rise to the idea that the liver is diseased. We ought to recollect, however, that acidity in the stomach, or perhaps in the duodenum, is sufficient to occasion irritation of a spinal nerve, and that, as in other instances, the pain is felt at the extremity of this nerve, somewhat in the side, and often exactly in the right hypochondrium. The immediate cause of this pain is at once ascertained by the usual examination of the dorsal spine.

Reasoning on the various cases which we have detailed, in which distant pains and other morbid affections appeared to yield most readily to local means, applied to the spine, we should feel inclined to expect that in many complaints in the thorax and abdomen, where blisters and other local applications were indicated, we would produce a more powerful and direct influence, by applying these to the spine than to any other part. Blisters applied to the back are perhaps more inconvenient than to the breast or abdomen, but very probably they are more efficient.

The nerves which supply the viscera of the thorax, and of the abdomen, enter from behind, and a number of these proceed directly from the spine to the viscus to which they are distributed. Of course in inflammatory and other affections of these organs, blisters and other applications to the back are perhaps more efficient than we are aware of. At any rate, if in these diseases we were to discover any degree of tenderness in the spine, extending to the affected viscus, we would expect considerable benefit from a blister on the spot. The old-fashioned plasters on the back, are perhaps more useful than modern practitioners suppose.

I have now given a general account of those complaints, in which for some time past I have observed that the nerves of the spine were irritable. In a great proportion of these, some distant pain was distinctly occasioned by this cause; and in many of them this immediately subsided, by directing the attention to the spine. In several of them, however, the spinal nerves instead of being primarily affected, appeared to sympathize with some other organ, but in the whole of them, I am confident that the state of the spine was entitled to great attention, and that we did more good by local applications to this nervous centre, than to the pained part at a distance from it.

*Treatment.*—In the simple form of this disease, where the irritation is limited to one nerve, the recumbent posture for a few days, is frequently sufficient to remove the pain. If the uneasy weakness continue in spite of this, we ought to apply leeches, and probably a blister to the affected part of the spine. There is no advantage in applying a large blister. If the disease has been long fixed to one spot, it is likely that the ligaments of the part have become thickened, and that even a caustic issue on the spot, kept open for some time, be necessary.

For the removal of the more formidable dis-

ease, other measures are needed, and in fact we often find that a long time passes before there is much improvement.

If the disease be at all violent, the recumbent posture is absolutely necessary. It is quite obvious, indeed, that, without this, even the most active treatment will fail in producing any permanent effect, since it is only by removing pressure, and avoiding motion, that the nerves and muscles can be allowed to recover their powers. Besides, when the nerves are in an over-sensitive state, which is a frequent occurrence in severe cases of this disease, motion of any kind, by irritating the nerves, is apt to occasion, or increase, partial spasm of the neighbouring muscles, and these spasmodic contractions, by forcibly drawing the vertebrae to one side, increase the pressure on the too sensible nerves, and in this way the pain may become agonizing. Total rest, therefore, in severe cases, is absolutely necessary. Leeches should be freely applied, or blood taken by cupping the pained part of the spine.

After the pain has relaxed, I conceive that we ought to give a fair trial to long continued friction, having previously fomented the pained part. We may either rub the part with wheat flour, or starch powder, as is now so frequently practised in various local diseases; or we may employ the anodyne balsam, or some oily substance, combined with an aromatic, such as the oil of thyme. Perhaps there may be an indication to use a combination of the extract of belladonna with the oil or lard, for we know that this drug has a powerful effect in relaxing muscular fibres, and we have some reason to believe that partial spasm of the muscles arranged along the spine, has very material influence in occasioning pressure on the nerves as they issue from the spinal marrow.\* We ought also to consider, that by long continued friction, we prevent the muscles of the

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\* An itinerant practitioner, already referred to, much celebrated for the success of his practice in cases of this description, directs the back to be well rubbed where there is any pain, for twenty minutes; the patient sitting with his back opposite a fire. The friction is employed morning and evening, *across* the spine from behind forward, not up and down the spine, as is usually done. He confines the friction to three parts of the spine, viz. to the upper part of the neck, the middle of the back, and the middle of the lumbar spine. He employs a nostrum of his own, which seems to be merely some oily substance combined with oil of thyme. This is probably a good stimulant, and ought to be more employed by regular medical practitioners. This same practitioner recommends a nourishing diet, with attention to the bowels, but he prescribes no internal medicine of any description. He leaves this entirely to the regular medical attendant of the patient. He orders the patient to lie on a firm mattress, in the horizontal posture, and occasionally to employ as much exercise as she can without exciting pain.

back from becoming weak, from want of exertion. The attempts at motion and exertion, however, must be made with considerable caution. They must be proportioned to the feelings of the patient.

It is an object, also, gradually to increase the slope of the sofa, according to the feelings of the patient, for by doing so, we give the muscles of the spine more room for exertion.

If these measures are not sufficient to remove this tedious and troublesome complaint, we must have recourse to other means. Blisters are often of use. They need not be large. If they merely cover the pained part, the size is quite sufficient. A small blister behind the ear or to the temple, is often effectual in removing severe pain, shooting over the head. I have found the antimonial irritation to be well suited for the removal of these spinal diseases. In two instances we found that obstinate disease subsided whenever erysipelas appeared on the affected part, and surely the antimonial eruption is much allied to this other inflammatory affection. We can apply an antimonial plaster to the pained part of the spine, or we may rub with the antimonial ointment morning and evening.

It is most likely, that the febrile excitement, which occasionally attends the eruption from antimony, may be of material service in removing the affection of the spine. In one well marked case in the Infirmary here (A. W.,) who had been a patient for a long time, with irritation of different portions of the spine, this disease went off completely, on being seized with typhus fever. She is at present (February 1828) convalescent from fever, and quite free of spinal irritation. If we had any means of exciting fever, we should perhaps remove the affection of the spinal nerves.

If these different means were to fail, I would have no hesitation in applying a blister to the head, even although no partial palsy or affection of the sight were present. If, however, there had been weakness of any part, or want of power, or intolerance of light, or dimness of sight, although there had not been any pain of the head, I would apply a blister to the head more early in the disease. I conceive that on many occasions my attention ought to have been more decidedly directed to the state of the head than it was; since I am now persuaded that in these diseases the brain is more frequently in fault than we are aware of.

In the treatment of this disease, it is of infinite consequence that the general health of our patient should be thoroughly attended to. The bowels of course must be properly regulated; the appetite improved by tonics, especially by the preparations of iron. Perhaps emmenagogues, with the hip-bath, if the catamenia are faulty, may be useful.

Minute doses of the blue pill may do good, if the biliary secretions be in fault, or if the urine be deficient; and, especially when joined with other diuretics, mercury may relieve vascular fulness in the head, by producing a flow of urine.

I conceive that I have seen the Prussic acid or valerian of use, in correcting excessive irritability of constitution.

The shower bath, or sponging with cold water and vinegar, are also occasionally proper. The clothing should be comfortable, and the diet nourishing.

*Postscript.*—It being some time since I wrote this long essay on a subject which I conceive to be of some interest, and which certainly has proved to me extremely puzzling and complicated, it may be useful to mention briefly my present impressions regarding its nature.

These are—that the immediate cause of the pain of the back and breast in spasm of one or other of the muscles, arranged along the spine, altering the position of the vertebræ, or otherwise compressing the nerves as they issue from the spinal marrow.

That this spasm, in many instances, is strictly a *local* disease produced by fatigue, wrong posture, or other causes, and quite unconnected with the state of the brain, spinal marrow, or nervous system in general.

But that, in other formidable instances, this partial, spasmodic, or wrong action of these muscles, is owing to a faulty state, perhaps an enlargement of the vessels of the brain, or spinal marrow. This state of the brain, as in many other diseases, gives rise to spasm, or even to convulsion of certain muscles; which partial symptom from its severity attracts the chief attention. This local affection is confined to those portions of the spine where there is the greatest motion, and where of course the muscles having the greatest activity, are most liable to deranged action or spasm. I imagine that this view of the subject is illustrated, and perhaps confirmed by various symptoms, which were observed in the different cases, and which without it, were very incomprehensible. The partial palsy, the affection of the sight, the giddiness of the head (for I find that this was a prominent symptom in several cases, especially in that of A. S.,) all give some confirmation to the notion that the brain is affected in these severe cases.

I have only to add farther, that if we pay attention to the number of the muscles, arranged along the spine, and to their functions, we shall see some reason for their being peculiarly liable to spasm. The variety of separate muscles in this situation is very great; and it ought to be kept in mind, that these are more constantly active than any other muscles, except the involuntary ones, since they are in a state of action in preserving the body in an erect state, as well as in every motion of the trunk.

This state of the muscles, as being the immediate cause of the pain, and of various uneasy feelings, is certainly entitled to marked attention, and we find it much moderated by posture, by local application, and especially by friction; but my conviction now is, that we ought to direct our attention at the same time to the state of the brain, as being the source on which severe cases of this description depend.

Glasgow, 13th February, 1828.

From the Archives Generales de Medecine.

RECHERCHES MEDICO-LEGALES POU-  
VANT SERVIR A DETERMINER,  
MEME LONG-TEMPS APRES LA  
MORT, s' il y a eu empoisonnement, et à  
faire connaître la nature de la substance vé-  
néneuse. Par MM. ORFILA et A. LESSUEUR,  
docteurs en Médecine. [Memoire lu à l'Aca-  
démie de Médecine.]

In the greater number of cases, the physi-  
cian charged with ascertaining the cause of  
sudden death, is called before the inhumation  
of the body; but it may also happen, that he  
is not consulted for several days, and even  
months afterwards. Is it possible to discover  
the presence of a poisonous substance, by  
subjecting to analysis the matters found in the  
digestive canal of a body, ten, twelve, fifteen,  
and eighteen months after its interment?  
Upon questions of this nature, judicial tri-  
bunals may demand information, and medical  
jurisprudence does not yet possess any work  
where that information may be found. We  
cannot more forcibly illustrate the necessity of  
these researches, than by alluding to the diffi-  
culty experienced by the eminent gentlemen  
whose opinion was requested in the case of  
Castaing: those gentlemen were, MM. Vau-  
quelin, Barruel, Chaussier, Laennec, Lermi-  
nier, Magendie, Pelletan, Segalas, and Orfila.  
Among other questions by the king's attor-  
ney, upon several points of legal medicine,  
relative to poisoning, it was demanded "whe-  
ther the acetate of morphine, which had been  
*vainly* sought for in the fluids arising from the  
washings of the alimentary canal of Ballet,  
might not have been decomposed by its mix-  
ture with the putrid animal substances, the  
more especially, that the proper chemical  
researches were not instituted until several  
days after death?" The commission, unable  
to invoke experience upon this subject, and  
guided only by analogy, after having long  
agitated the question, were unanimously of  
opinion, that the acetate of morphine might  
be decomposed, and that decomposition would  
affect at the same time, the acetic acid and  
the morphine. We shall hereafter see that  
this assertion is contrary to truth.

The solution of the problem under con-  
sideration, has appeared to us to depend upon  
two orders of experiments: 1st, poisonous  
mineral and vegetable substances dissolved in  
about a pint of water, in varying proportions,  
were mixed with animal matters, and exposed  
to the open air in large uncovered vessels,  
during ten, fifteen, and eighteen months, care  
being taken to renew the water as it evapo-  
rated; 2d, the same substances, mixed with  
albumen, gelatine, meat, &c., were introduced  
into stomachs and intestines, and the whole  
included in deal boxes, which were accurately  
closed, and buried at a depth of two feet and  
a half. Several months afterwards the boxes  
were disinterred, and the substances contained  
in the above mentioned viscera, analysed.

On the other hand, human bodies, buried in  
deal coffins at a depth of four feet, were dis-

interred one or more months after inhumation,  
in order to determine how long after death  
traces of the digestive canal could be observed,  
and the presence of poisonous substances, in-  
troduced into the stomach or intestines during  
life, demonstrated. These latter experiments,  
which should be made the subject of a particu-  
lar memoir, have taught us, that several months,  
and even years after death, when none of the  
soft parts are longer recognizable, there may  
yet be found, upon the sides of the verte-  
bral column, and in the abdominal region, a  
kind of brownish mould, evidently the re-  
mains of the alimentary canal, and in which  
may exist a portion of the poisonous substance,  
either changed, or in its natural condition.

The poisons which have been made the sub-  
ject of experiment, are the sulphuric, nitric,  
and arsenious acids, corrosive sublimate, the  
acid tartrate of potass and antimony, the ace-  
tate of lead, the proto-hydrochlorate of tin,  
the sulphate of copper, the nitrate of silver,  
the hydro-chlorate of gold, the acetate of mor-  
phine, the hydro-chlorate of brucine, the ace-  
tate of strychnine, opium, and cantharides.  
We shall consider them in the order in which  
they are mentioned.

*Concentrated Sulphuric Acid.*—On the 12th  
March, 1826, six ounces of concentrated sul-  
phuric acid, the fourth part of a human liver,  
divided into small pieces, and a portion of in-  
testine, were exposed to the air in a large  
mouthed glass vessel. The 15th of the same  
month, the contents were reduced into a kind  
of pulaceous mass of a blackish brown colour,  
exhaling a nauseous acid odour; it strongly  
reddened litmus paper, and threw down, on  
the addition of the hydro-chlorate of barytes,  
an abundant precipitate of the white sulphate  
of that mineral, insoluble in boiling water and  
in nitric acid. Heated in a vial, with copper  
in its metallic state, some time elapsed ere  
sulphurous acid was disengaged, owing prob-  
ably to the acid having been weakened by  
the water contained in the animal substances;  
by continuing the heat, however, a consider-  
able quantity of this gas was obtained, and a  
sulphate of copper was formed. On the 26th  
May, 1827, twenty-two and a half months after  
the commencement of the experiment, the  
mixture appeared under the form of a black  
pulaceous mass, possessing all the characters  
above indicated; mercury, which was substi-  
tuted for the copper, to disengage the sul-  
phurous acid, was changed into a proto-sul-  
phate. During the time intervening between  
the two periods just mentioned, the contents  
were examined at least twenty times, and al-  
ways with the same result.

*Dilute Sulphuric Acid.*—July 18th, 1826,  
twenty grains of concentrated sulphuric acid, a  
pint and a half of water, and about the third of  
an intestine, taken from a human subject, were  
introduced into a vessel similar to that em-  
ployed in the first experiment. On the 12th  
of the following August, the liquor was of a  
yellowish white colour, *strongly reddened* an  
infusion of litmus, and, treated by the soluble  
salts of barytes, threw down a white precipi-

tate insoluble in water and nitric acid. We wished to ascertain whether by boiling the liquid with mercury, sulphurous acid might be obtained, but by reason of the large quantity of animal matter it contained, it foamed and evaporated ere the gas was perceptible. May 21st, 1827, nine months and three days after the commencement of the experiment, the mixture exhaled an insupportable odour; diluted with water and filtered, it *scarcely reddened* litmus paper; the greater part of the sulphuric acid having been saturated by the ammonia evolved during the putrefactive process; the sulphate of ammonia was decomposed, and the ammonia evolved in great quantity, when the liquid was boiled with quicklime. With the barytic salts it afforded a copious white precipitate of the sulphate of barytes, insoluble in water and nitric acid; concentrated by evaporation and boiled with mercury, it gave out no sulphurous acid gas, notwithstanding it was reduced almost to a state of aridity. Perceiving that it was impossible by this method to prove the identity of the free acid of the liquor with sulphuric acid, the following process was instituted. A portion of the liquor was heated at a low temperature with *pure* subcarbonate of lime, which had been previously boiled in distilled water, and did not contain a particle of the sulphate; there was no effervescence; after keeping it in a state of agitation about ten minutes, it was filtered. The white mass left upon the filter, washed with distilled water to remove any sulphuric acid and sulphate of ammonia which it might contain, was dried and treated in a vial with boiling distilled water. The filtered solution was not disturbed by the hydro-chlorate of barytes, nor by the oxalate of ammonia, and therefore did not contain sulphate of lime. It is evident, then, that the free sulphuric acid which existed in this liquor, was so weak as scarcely to form any sulphate of lime, and the little which was produced, was dissolved in the water employed in washing the precipitate.

One drachm of *concentrated sulphuric acid* was introduced on the 10th November, 1826, with a portion of intestine, into a porcelain vessel, which was enclosed in a deal box, and buried at a depth of two feet and a half. It was disinterred on the 30th April, 1828, seventeen months and twenty days after the inhumation. The intestine had a slightly yellowish tint, and appeared to swim in a grayish and somewhat turbid liquid, which reddened litmus paper, effervesced when thrown upon the pavement, with the salts of barytes threw down a white precipitate insoluble in water and nitric acid, and when boiled with mercury, gave out sulphurous acid gas; it contained therefore free sulphuric acid; it was necessary, however, in order to ascertain its existence, to prolong the ebullition almost to dryness, probably owing to the acid having been much weakened by the humidity of the intestines.

It is proved therefore, 1st, that it is possible to determine the presence of *concentrated sul-*

phuric acid, several months, and even years after its admixture with animal substances; 2d, that if this acid have been *greatly diluted*, and mixed with substances which, in undergoing decomposition, evolve a large quantity of ammonia, it is saturated by this alkali, so that none, or very little of the acid remains in an uncombined state after the lapse of a few months; 3d, that in this case, the circumstance of poisoning by sulphuric acid cannot be proved; that however, from the existence of the sulphate of ammonia, which we suppose to have been obtained in a crystalline and well characterized state, there will be a presumption that such has been the case, as this compound does not ordinarily form a part of the aliment, nor enter into the composition of the tissues belonging to the digestive canal; 4th, that if there should still remain a quantity of free sulphuric acid, the best method of determining unequivocally its existence, would be to treat the liquor with *pure* sub-carbonate of lime, when sulphate of lime will be formed at the expiration of a few seconds; while the sulphate of ammonia, treated with the sub-carbonate at the ordinary temperature, is not decomposed till after a longer time.

*Concentrated Nitric Acid.*—On the 12th March, 1826, ten ounces of the nitric acid of commerce, and portions of intestine and liver taken from a human subject, and divided into small pieces, were introduced into a large mouthed glass vessel, and exposed to the air. The 19th of the same month the mixture had a yellow colour; the transparent liquid strongly reddened litmus paper, and *did not act* upon copper, either at the ordinary temperature or when raised to *ebullition*. To obtain the orange coloured nitric acid gas, it was necessary to evaporate the liquor to dryness, and decompose, by means of heat, the nitrate of copper which had been formed. Mixed with a small quantity of solid potash prepared with alcohol, it became red immediately, and furnished by evaporation a residuum of the same colour, which fused, like nitrate of potash, when heated with charcoal, and placed in contact with copper, sulphuric acid, and a few drops of water immediately furnished orange coloured nitrous acid gas. May 26, 1827, fourteen months and a half from the commencement of the experiment, the liquor treated with solid potash, ignited charcoal, copper and sulphuric acid, in the manner just mentioned, gave the same results.

*Dilute Nitric Acid.*—On the 18th July, 1826, a vessel like those employed in the former experiments, containing a pint and a half of water, twenty grains of nitric acid, and nearly a third of an adult intestine, was exposed under similar circumstances. The 12th of the following August, the fluid had a yellowish tinge, *reddened litmus paper*, and, evaporated to dryness with caustic potash, left a reddish residuum, which, thrown upon ignited charcoal, did not fuse, but became carbonized, exhaled an odour like burning horn, and, in a word, comported itself as a substance rich in animal matter; treated with

copper and sulphuric acid, it effervesced, but the odour and colour of nitrous acid gas, were with difficulty distinguishable. May 23d, 1827, six months and four days from the commencement of the experiment, the mixture exhaled a very fœtid odour: the filtered liquor, far from reddening litmus paper, *changed to a blue* the colour of paper reddened by the action of an acid, which was due to the presence of a quantity of ammonia; treated with potash at a boiling temperature, much ammoniacal gas was disengaged, and nitrate of potash was formed; since, when evaporated to dryness, and the residuum agitated a short time with distilled water, a liquid was obtained, which being filtered and evaporated, furnished a salt, with potash for its base, which fused upon ignited charcoal, and gave out nitrous acid gas when mixed with copper, sulphuric acid, and a little water.

A drachm of *concentrated nitric acid* having been placed, with a portion of intestine, in a small porcelain vessel, was enclosed in a deal box, and buried at a depth of two feet and a half, on the 10th November, 1826. It was disinterred on the 30th April, 1828, seventeen months and twenty days afterwards; the intestine had not assumed a yellow colour; in the vessel was found about three drachms of a turbid, grayish liquid, which reddened litmus paper, effervesced when thrown upon the pavement, did not act upon copper at the ordinary temperature, and which, saturated with potash and evaporated to dryness, left a residuum that fused when thrown upon ignited charcoal like the nitrate of potash, and disengaged vapours of orange coloured nitrous acid, when mixed with copper, and treated with sulphuric acid diluted with a small quantity of water. It contained therefore nitric acid.

These experiments prove incontestibly, 1st, that the presence of concentrated nitric acid may be demonstrated several months after its admixture with animal substances, and when the putrefactive process has arrived at its maximum; 2d, that for this purpose, potash is preferable in the first instance, to metallic copper; 3d, that the acid cannot be detected when it has been considerably diluted with water, and employed in small quantity—the ammonia which results from the putrefaction of the animal substances, being sufficient to saturate all the acid; 4th, that in this case we cannot do more than establish the presence of the nitrate of ammonia, which does not necessarily infer poisoning by nitric acid, since this nitrate might possibly have been formed during the putrefaction of the animal substances.\*

*Arsenious Acid.*—March 8th, 1826, a glass

vessel, containing a pint and a half of water, holding in solution three drachms of arsenious acid, and pieces of muscle, brain, and intestine, was exposed to the air as in the former experiments. The contents were examined on the second day of the following August, nearly five months afterwards, and *exhaled no disagreeable odour*; the filtered liquor, treated by the hydro-sulphuric acid, by the ammoniacal sulphate of copper and by lime water, furnished results similar to those obtained from a solution of arsenious acid in pure water.

*Arsenious Acid, much more diluted with Water.*—Six grains of arsenious acid, dissolved in a pint and a half of water, were exposed, July 18th, 1826, in a glass vessel, with about one-third of an adult intestine. On the 12th of August following, the mixture *scarcely exhaled* any disagreeable odour; the filtered liquor neither assumed a yellow hue, nor threw down a precipitate on the addition of hydro-sulphuric acid; the sulphate of copper occasioned no change; during evaporation there was a considerable coagulation of animal matter, which was removed as it formed; the product of evaporation, treated with boiling distilled water for the space of four or five minutes, contained arsenious acid, since the solution reddened by the action of hydro-sulphuric acid, and on the addition of one drop of hydro-chloric acid, threw down a precipitate of the yellow sulphuret of arsenic, soluble in ammonia. The yellow colour, and precipitate, resulting from the action of the hydro-sulphuric acid, were much less sensible, when, instead of operating in the manner above mentioned, the reagent was added to the liquid simply heated to the boiling point, and filtered, in order to coagulate the animal matter. On the 5th of May, 1827, nine months and a half from the commencement of the experiment, the mixture exhaled a fœtid odour; it filtered with difficulty, by reason of the great quantity of animal matter it contained in solution; it *rapidly* changed to a blue, the colour of litmus paper which had been reddened by exposure to an acid; hydro-sulphuric acid, and the ammoniacal sulphate of copper, occasioned *no alteration*, although they demonstrated the presence of arsenious acid when, after being evaporated to dryness in order to separate the animal matter, the residuum was treated with boiling distilled water.

The same experiment, repeated on the 27th of February, and the liquid examined on the 27th of April following, furnished analogous results.\*

\* We may observe, however, that having macerated an intestine in distilled water, from the 27th February until the 23d of April following, there was no formation of nitrate of ammonia, notwithstanding the vessel was constantly exposed to the air, and the putrefactive process had reached its height.

\* We wish particularly to call the attention of the reader to the fact, that by mixture with animal matter in a state of solution, arsenious acid may be *masked* so completely, as not even to develop a yellow colour by the addition of hydro-sulphuric acid; but we may also remark, that in these cases, it is only necessary to evaporate the liquor to dryness, and dissolve the product in boiling water, to obtain a solution, in which the hydro-sulphuric, mixed

*Solid Arsenious Acid.*—November 8th, 1826, the white of an egg, portions of meat, bread, and twenty grains of solid arsenious acid, were introduced into a portion of large intestine taken from an adult, and the whole included in a small deal box, which, after having been perfectly closed, was buried at a depth of two feet. August 14th of the following year, nine months and six days afterwards, the box was disinterred, and the contents of the intestine agitated in distilled water; after the lapse of a few minutes, it was filtered, and the addition of hydro-sulphuric acid evinced the presence of a large quantity of arsenious acid.

Having sprinkled two thick slices of the lean of veal with arsenious acid, M. Dubuc enclosed them in a strong oaken box, which he buried in a soil sufficiently permeable to water. Upon exhumation about six years afterwards, a kind of mould was found, which crumbled beneath the fingers, and contained so much arsenic, that 24 grains thrown upon ignited charcoal, infected, by its alliaceous odour, the air of a large laboratory. (*Journal de Chimie Médicales*, tom. ii. p. 278.)

It follows from the preceding facts, 1st, that the presence of arsenic which has been mixed with animal matters, may be detected even after the lapse of several years; 2d, that it is nevertheless necessary in many cases to remove a part of these matters, which is readily effected by evaporating to dryness, and agitating the product for several minutes with distilled water; 3d, that if the arsenious acid have been employed in the solid state, it will be sometimes possible, even long after inhumation, to discover here and there particles of the mineral, which present all the characters of this poison; 4th, it is indubitable, that in proportion as ammonia is disengaged, the arsenious acid is transformed into an arsenite of ammonia, which is *much more soluble* than arsenious acid; so that it may happen, that

with a drop of hydro-chloric acid, will precipitate all the arsenious acid in form of the yellow sulphuret. This important fact, the correctness of which, one of us has already several times had occasion to verify before judicial tribunals, in cases of poisoning by arsenious acid proved a few days after death, evinces how much the difficulties attending the discovery of this poison when mixed with animal substances, have been exaggerated. If writers, who have given much more complicated methods than that which we propose, have not found the arsenious acid in the liquids ejected by vomiting, and those contained in the stomach and intestines, it is because they have not followed exactly our footsteps, and especially because they have not made use of the hydro-sulphuric and hydro-chloric acids as reagents, but have employed the deuto-sulphate of ammoniacal copper, which is an unfaithful test, as we have already elsewhere established. (*Vide Orfila, Leçons de Médecine-légale*, tom. iii. pag. 112, deuxième édition.)

after a lapse of several years, we may not be able to demonstrate the presence of arsenious acid, when it would have been easy to have done so, some months after inhumation; because this acid, once transformed into the arsenite of ammonia, will have become soluble, and have filtered into the earth through foramina, which are often found in the parietes of the coffin, when putrefaction has made great progress; 5th, that if arsenious acid, when largely employed, arrests the putrefaction of animal substances, such is not the case when the quantity is very small.

*Corrosive Sublimate.*—March 8th, 1826, three drachms of corrosive sublimate dissolved in two ounces of boiling water, were introduced into a large mouthed glass vessel containing two pints and a half of water, together with meat, cerebral matter, and portions of intestine. On the nineteenth, the mixture *did not exhale a fœtid odour*; the animal substances were indurated, and as if tanned; the filtered liquor hardly assumed a brown colour by the action of hydro-sulphuric acid; potash and ammonia, at most, only rendered it opaline, but a plate of gold, covered in a spiral form by a leaf of tin, was immediately covered with a layer of metallic mercury upon being plunged into the liquid, to which a few drops of hydro-chloric acid were at the same time added. From the animal substances, washed and well dried, metallic mercury was obtained by calcination, in a retort or small glass tube. Examined on the 18th June, 1827, the results were precisely similar.

On the 18th April, 1826, half of the liquid under consideration, and which contained so small a quantity of the sublimate, that the existence of the latter could only be detected by means of a plate of gold, was mixed with other animal substances, (liver, spleen, and intestines.) On the 20th of the same month, the mixture exhaled an excessively fœtid odour, and the addition of potash, ammonia, and hydro-sulphuric acid, occasioned no change of colour in the liquor. The plate of gold, employed as above mentioned, was not whitened, even after the lapse of an hour.

*Corrosive Sublimate diluted with a large quantity of Water.*—On the 18th of July, 1826, six grains of corrosive sublimate, together with a portion of intestine, were introduced into a glass vessel containing a pint and a half of water. The mixture, examined on the 2d of August following, exhaled a *very fœtid odour*; hydro-sulphuric acid, the hydro-sulphates, potash and ammonia, neither threw down any precipitate from the filtered liquor, nor rendered it cloudy; a plate of gold immersed into it, did not assume a white colour till after several hours. The intestine, dried and calcined with potash, furnished metallic mercury.

These experiments lead us to the conclusion, 1st, that corrosive sublimate, dissolved in water, is decomposed by animal substances with sufficient rapidity to render it impossible, after some days, to demonstrate its presence otherwise than by a plate of gold covered spirally by a leaf of tin, and assisted by the

action of hydro-chloric acid; 2d, that the quantity of the sublimate decomposed, is proportionate to that of the animal matter employed; 3d, it would appear, however, that the animal matter was not capable of decomposing the whole of the sublimate, since after the lapse of several hours, *an atom* of metallic mercury was obtained from a solution of six grains of the sublimate, mixed with a *large quantity* of animal matter; 4th, that in all these cases, by subjecting the animal matter to the action of charcoal, assisted by an elevated temperature, metallic mercury may be obtained even after an interval of several years. Now, the presence of this metal, if it do not prove the existence of corrosive sublimate, at least evinces that of some mercurial preparation.

*Acid Tartrate of Potash and Antimony.*—Three drachms of tartarised antimony, dissolved in two pints of water, the fourth of a human liver, and a portion of intestine, were exposed to the air in a wide mouthed glass vessel, March 29, 1826. The mixture was found in a putrid condition on the ninth of the following April; the filtered liquor, treated by hydro-sulphuric and sulphuric acids, lime water, and an infusion of galls, furnished the same results as a solution of emetic tartar. April 28th, hydro-sulphuric acid and the hydro-sulphates, no longer threw down any precipitate, proving that the liquor did not contain tartarized antimony, or rather, that if it were present, the animal matter held in solution, prevented its detection by those reagents; sulphuric acid, and the infusion of galls, produced a grayish white precipitate, evidently the product of the action of these agents upon the animal matter held in solution.

The liquor being filtered and evaporated to dryness at a gentle heat, a residuum was obtained, which, by agitation for a few minutes with warm distilled water, formed a solution, from which a precipitate of the hydro-sulphate of antimony was thrown down, on the addition of hydro-sulphuric acid. The sixth of June of the same year, the liquor no longer contained antimony, no change resulting from the action of hydro-sulphuric acid, even after it had been evaporated, and the residue treated with water; but metallic antimony was obtained from the animal matters, after they had been dried and sufficiently calcined.

*Tartrate of Potash and Antimony diluted with a large quantity of Water.*—July 18th, 1826, six grains of tartarized antimony, dissolved in a pint and a half of water, and about the third part of an intestine, were exposed together in a glass vessel. Examined on the second of August, the hydro-sulphuric acid and the hydro-sulphates, occasioned no disturbance in the liquor. The solid matter, upon being dried and calcined, furnished metallic antimony.

It follows from the preceding facts, 1st, that tartarized antimony, when mixed with animal matter, is decomposed after the lapse of a few days, the tartaric acid being destroyed,

and an oxide of antimony precipitated; 2d, that it is then impossible to demonstrate its presence in the liquor, by the tests ordinarily used to detect the salts of antimony, but that this metal in its metallic state may be extracted from the solid substances, even after the lapse of several months; 3d, that the decomposition is rather the result of the action of air and water upon the salt, than of the animal substances, for it has been proved by experiment, that a solution of three drachms of tartarized antimony in a pint and a half of distilled water, undergoes similar decomposition when exposed to the air, and that the antimonial salt cannot be detected in it after a period, varying in summer from thirty to forty days, any more than it could in a similar solution, to which albumen and gelatine had been added.

*Acetate of Lead.*—March 29th, 1826, two pints of water, holding in solution three drachms of the acetate of lead, were exposed in a large mouthed vessel, with muscle, liver, and portions of intestine. On the ninth of April following, the solution no longer contained acetate of lead, for when filtered, it did not change its colour on the addition of hydro-sulphuric acid; but lead in its metallic state, was obtained by drying and strongly calcining a blackish gray precipitate which had been thrown down, and the animal substances above mentioned.

*Acetate of Lead greatly diluted with Water.*—Six grains of the acetate of lead, in a pint and a half of water, were exposed, with about the third part of an intestine, July 18th, 1826. Four days afterwards, no trace of the salt could be discovered in the solution, but the solid matters being calcined, yielded a sensible quantity of lead.

It is evident, therefore, that we should not search in the liquid for the acetate of lead, which after solution has been placed in contact with animal matter; and it appears moreover, that a very short time is required to effect its decomposition.

*Proto-hydrochlorate of Tin.*—On the 10th July, 1826, two drachms of this salt, dissolved in a pint and a half of water, and the third part of an intestine, were exposed under the same circumstances as in the preceding experiments. The mixture exhaled a very fetid odour, when examined on the second of the following August. The liquid, filtered, and treated with hydro-sulphuric acid and the hydro-sulphates, remained colourless, while by drying separately, and calcining the intestine and a flocculent grayish matter which had precipitated, metallic tin was obtained; from which it follows, that a very short time is required for the entire decomposition of an aqueous solution of the proto-hydrochlorate of tin by animal matter.

*Sulphate of Copper.*—March 12th, 1826, an intestine, plunged into a solution of three drachms of the deuto-sulphate of copper in two pints of water, was exposed to the air in a wide mouthed glass vessel. On the 18th of the following June, the mixture gave out an excessively fetid odour; the filtered liquid

had a dirty bluish green colour, and threw down a chesnut brown precipitate when treated by the ferro-prussiate of potash, and a black precipitate, by the soluble hydro-sulphates; it changed to a blue, on the addition of ammonia. Desirous to ascertain to what point the solution retained all the sulphate of copper which had been added, a portion was diluted with fifteen times its volume of water, and it was ascertained that it was scarcely affected by the above mentioned reagents, while another portion of the same solution, which had been set apart *prior to its admixture with the intestine*, instantly afforded a precipitate, though diluted with two hundred times its volume of water. It became therefore indispensable to ascertain whether the oxide of copper, which appeared to have separated from the solution, was not contained in the solid matter. The latter, after being carefully washed to remove any sulphate of copper which might be mixed with it, was dried and calcined; the residuum, besides presenting here and there reddish particles of metallic copper, furnished, when treated by hot nitric acid, a perceptible quantity of the nitrate of copper.

*Sulphate of Copper greatly diluted with Water.*—On the 18th July, 1826, six grains of the deuto-sulphate of copper, dissolved in a pint and a half of water, were exposed in a vessel containing intestine. August 2d, the mixture exhaled a very fœtid odour, the liquor was *nearly colourless*, and contained none of the cupreous salt, its colour remaining unchanged on the addition of the ferro-prussiate of potash, ammonia, and hydro-sulphuric acid. Nitrate of copper was obtained by treating the carbon, resulting from the calcination of the intestine, with nitric acid.

These experiments prove, 1st, that by admixture with animal matter, the deuto-sulphate of copper is decomposed, so that after the lapse of a certain time, none of the salt remains in solution; 2d, that however, this decomposition is not so rapid, that a portion of the salt may not be found in solution, even after several months, if the experiment be made on several drachms of the salt; 3d, that in all cases in which the cupreous salt cannot be discovered in the liquor, the solid substances should be dried and calcined, in order to extract the metallic copper, while a portion of the carbon should be subjected to the action of nitric acid, that a nitrate of copper may be formed.

*Verdigris.*—November 8th, 1826, a stomach, containing twelve grains of verdigris, pieces of meat, the white of an egg, and a quantity of *soupe maigre*, was enclosed in a thin deal box, and buried at a depth of two feet and a half. It was disinterred on the seventh of August, 1827. The contents of the stomach were green; after they had been cut into small fragments and boiled in distilled water, the filtered liquor afforded, with the usual tests, no indications of the salts of copper; this was also the case with the liquor obtained from boiling the stomach in water. Dilute hydro-

chloric acid being placed in contact with all the green portions, the latter assumed an *unctuous and grayish aspect*; after being agitated for a few minutes, the solution was filtered; it was of a greenish blue colour, and afforded a chesnut brown, a black, and a blue precipitate, when treated respectively by the ferro-prussiate of potash, hydro-sulphuric acid, potash, and soda; ammonia imparted to it a blue colour, as it does to the salts of copper. It follows from what has been stated, 1st, that by its sojourn with animal matter under ground, verdigris is decomposed, and the deuto-oxide of copper forms with the fat of the dead body a kind of soapy matter, insoluble in water; 2d, that in a case of poisoning by this substance, it would be possible to demonstrate the presence of the deuto-oxide of copper, by means of hydro-chloric acid and calcination, several months, and even years, after inhumation.

*Nitrate of Silver.*—July 12th, 1826, a drachm of the nitrate of silver, dissolved in a pint and a half of distilled water, was exposed, as in the former experiments, with a portion, of intestine. When examined on the second day of the following month, the mixture had already become very fœtid; the colour of the filtered liquor underwent no change by the addition of hydro-sulphuric acid, and was scarcely disturbed by the hydro-chloric acid and its salts. Upon drying and calcining separately the intestine, and a flocculent brownish precipitate which had been formed, metallic silver was obtained. Nitrate of silver, in solution, is then rapidly and completely decomposed by animal substances; so that, if called to give an opinion in a case of poisoning by this salt, several days after inhumation, it would probably be necessary to endeavour to extract the metal from the solid matters.

*Hydro-chlorate of Gold.*—July 10th, 1826, pieces of liver, intestine, and a pint of water holding in solution thirty-six grains of the hydro-chlorate of gold, were exposed to the air in a wide mouthed glass vessel. On the second of August, the mixture had become very fœtid; the filtered liquor, treated by the hydro-sulphuric acid, the hydro-sulphates, and ammonia, afforded no trace of the salt; gold however was obtained by calcination of the solid substances; in fact these substances, dried and reduced to carbon by heat, gave a yellowish solution when treated by aqua regalis, from which the proto-hydrochlorate of tin threw down a purple, ammonia, a yellow, and hydro-sulphuric acid, and the proto-sulphate of iron, a brown precipitate; the same result was obtained from a grayish precipitate which had formed, and was carefully separated from the intestines in order to be calcined. The carbon, moreover, resulting from these calcinations, presented here and there brilliant reddish particles, which were evidently metallic gold. We shall say then, in relation to poisoning by the hydro-chlorate of gold, what we have already established when speaking of the nitrate of silver.

*Acetate of Morphine.*—A drachm and a half

of the acetate of morphine, dissolved in a pint of water, was mixed in a large mouthed vessel with *soupe maigre*, rich broth, fat, and several portions of intestine, and the whole exposed to the air on the 8th of March, 1826. Examined on the 26th of the same month, the mixture already exhaled a very fœtid odour; ammonia threw down from the filtered liquor, a grayish white precipitate; evaporated to dryness, a yellowish product was obtained, which changed to a *beautiful red colour* by the action of nitric acid, and to a blue, when treated with a slightly acidulous trito-hydrochlorate of iron; this latter shade, however, was less intense than that produced by the same reagent, upon a quantity of morphine equal to that of the product employed; there were, moreover, several *greenish spots* scattered here and there, the consequence of the admixture of the blue colour above mentioned, with the yellow tint of the product. The 9th of April following, the filtered liquor threw down a grayish white precipitate by ammonia, and furnished by evaporation a product which strongly *reddened* by nitric acid, and changed to a *green* by the trito salt of iron; in strictness, this green colour had a slight shade of *blue*, and afterwards of *brown*. On the 16th of April, the same characters were presented, with the difference, that the salt of iron produced with the residuum of evaporation an *olive green colour*, without any bluish shade. Similar results were obtained on the 18th of June, when the putrefactive process had arrived at its height.\*

A portion of the liquor was filtered, on the 1st of August, 1826, and treated with ammonia, which threw down a brownish gray precipitate of *morphine*; in fact, by treating this precipitate by nitric acid, and rendering the solution colourless by the action of animal charcoal, a solid grayish product was obtained by evaporation, which nitric acid changed to a red, and the hydro-chlorate of the trito-oxide of iron, to a greenish blue colour. Another portion of the liquor, evaporated to dryness, yielded a yellowish brown residuum, which was treated with boiling alcohol; the alcoholic solution was evaporated to dryness, and the product treated by distilled water, and subsequently by the sub-acetate of lead, by hydro-sulphuric acid, and by purified animal charcoal, as advised by M. Lassaigne,—a liquid

was obtained, which, evaporated in a sand bath, left a slight residuum of a yellowish white colour, which assumed a *very beautiful red* under the action of nitric acid, and a *greenish blue* under that of the trito-hydrochlorate of iron.

May 18th, 1827, fourteen months and six days from the commencement of the experiment, the liquor was excessively fœtid and strongly alkaline, restoring instantly the blue colour of litmus paper which had been reddened by an acid; about six ounces remained, the greater part having been employed in the several essays already described;\* this liquor was divided into two portions, A and B. The portion A was evaporated, and treated successively by alcohol, the sub-acetate of lead, hydro-sulphuric acid, and animal charcoal, according to the method of M. Lassaigne; a solid product was obtained, slightly *yellowish*, which *reddened* by nitric acid, and assumed under the action of the hydro-chlorate of the trito-oxide of iron, a *red or brown* instead of a *blue* colour; this solid product, treated with distilled water at the ordinary temperature, was not entirely soluble, the portion dissolved, filtered, and evaporated to dryness, *reddened* under the action of nitric acid and the *salt of iron*, when it should have been changed to a *blue* by the latter test; the portion remaining on the filter, also *reddened* when treated with nitric acid, and became *blue* when the salt of iron was employed. The portion B, of the liquor, instead of being treated according to the process of M. Lassaigne, was simply filtered and evaporated to dryness; the product, which was of a *very brown colour*, was boiled several minutes in concentrated alcohol, to which it imparted its deep brown hue; the solution was treated with animal charcoal *purified* by means of hydro-chloric acid, and carefully washed, it was then filtered several times through another portion of charcoal, till it became nearly colourless; evaporated in a sand bath it left a yellowish product which strongly *reddened* on the addition of nitric acid, and became *blue* when treated with the salt of iron in solution, not however, unless the latter were employed in very minute quantity, as otherwise it produced a reddish colour. The result furnished by the portion B, compared with that obtained from A, proves evidently, that the sub-acetate of lead and hydro-sulphuric acid may be advantageously omitted, when endeavouring to detect the presence of morphine.

*Acetate of Morphine diluted with Water.*—On the 18th of July, 1826, six grains of the acetate of morphine, dissolved in a pint and a half of water, were exposed with about the third of an intestine, as already mentioned. The putrefactive process was at its height when examined on the 21st May, 1827, six months and three days from the commencement of the experiment. The liquor having

\* Fearing lest the beautiful blue colour, developed by the action of nitric acid upon the product of evaporation, might be the result of its action upon the putrid animal matter, rather than upon the acetate of morphine, we evaporated to dryness an excessively fœtid liquor containing none of the acetate, and ascertained that the product of evaporation became *only yellow* when treated with nitric acid. The liquor in question was obtained by exposing in an open vessel, from March 8th till the 18th of June following, a pint of water, mixed with soup, broth, fat, and intestine.

\* It is almost unnecessary to state that water was added, in proportion as it was dissipated by evaporation.

been filtered, and evaporated at a gentle heat, the product, which was of a brown colour inclining to black, was treated with boiling alcohol; the alcoholic solution, evaporated to dryness, furnished a residuum which was subjected to the action of distilled water, sharpened with acetic acid. This new solution was rendered colourless by animal charcoal, and again evaporated to dryness; the product, which possessed a bitter taste, *reddened* by the action of nitric acid, but did not become *blue* when treated by the trito-hydrochlorate of iron; this test also imparted to it a reddish colour.

These experiments establishing satisfactorily, that the morphine was not destroyed, even several months after the admixture of its acetate with animal matter, we wished to ascertain what would take place in an aqueous solution of this salt exposed to the air, and it was observed that the acetate was *partly decomposed*, that the *acetic acid* of the decomposed portion was destroyed, while the *morphine of the same portion* was precipitated, if not entirely, at least the greater part. The following facts place beyond a doubt, the truth of these assertions.

1st. A drachm and a half of the acetate of morphine was dissolved in two pints of water. After ten months' exposure to the air, the liquor, which had long been covered with mouldiness, was turbid, and floated above a copious precipitate; filtered, and evaporated to dryness, it yielded a yellowish powder, which assumed a red, and a blue, colour when treated respectively, by nitric acid, and the salt of iron above mentioned. The precipitate which remained upon the filter, after having been washed several times with boiling water to remove any soluble matter which it might contain, was treated by boiling alcohol; the solution thus obtained being evaporated, deposited a notable quantity of crystals of *morphine*.

2d. May, 19th, 1827, twenty-four grains of the acetate of morphine were dissolved in a pint of distilled water; the filtered and *transparent* liquor, slightly reddened litmus paper, and was abandoned to the air in a large mouthed vessel. Eight days afterwards, floculi of mould were observed in the liquor; they had become much more numerous when examined on the third of August, though the liquor still retained its transparency. It restored the blue colour of litmus paper, had no sensible odour, and when a feather moistened with hydro-chloric acid was brought near its surface, gave out no white vapours, which would have been formed had there been any disengagement of ammonia. On the 27th of February, 1828, the liquor was turbid, and the sides of the vessel lined with yellowish and strongly adherent crystals. It had a yellowish amber colour when filtered, and after being evaporated to dryness, yielded a yellowish gray product, which nitric acid changed to a *red*, and the salt of iron to a *blue* colour; it was almost entirely soluble in distilled water, and appeared to consist of the acetate of mor-

phine, mixed with a very little foreign matter. The mould and other flocculent matters which remained upon the filter, yielded a similarly coloured residuum, which evinced the same phenomena when treated with the reagents just mentioned. After repeated boiling in distilled water, to remove any soluble matter they might contain, they were dried and boiled in alcohol of 38°, which dissolved only a part; the solution slowly restored the blue colour of litmus paper, and furnished on evaporation crystals of *morphine*. The matter adhering to the sides and bottom of the vessel, having been detached, and exhausted by means of boiling water, was dried, and boiled with alcohol of 40°, which dissolved it almost entirely; the solution was slightly alkaline, and yielded by evaporation a considerable quantity of perfectly crystallized *morphine*.

This decomposition of the acetate of morphine in water, has been likewise observed by M. Dublanc; and Geiger had already remarked, that it underwent an analogous decomposition in alcohol; but as stated by M. Dublanc, this spontaneous decomposition has its limits, and may be prevented by maintaining the liquor in an acid state. (*Journal de Pharmacie*, 1827, p. 264.)

It follows from all these facts, 1st, that in a case of juridical exhumation, it is possible to detect, several months after death, the presence of the acetate of morphine, or of morphine in the alimentary canal of an individual poisoned by a preparation of this nature; 2d, that for this purpose it is necessary to operate not only upon the liquids, but also upon the suspected solid matters, because if the poisoning had been effected by an aqueous solution of the acetate, this will have been decomposed, and the morphine in part precipitated; 3d, that there will be a smaller quantity of morphine precipitated, than might be at first view imagined, a part being re-dissolved by the ammonia formed during putrefaction; 4th, that to obtain the morphine which may exist in the solid matters, it is necessary in the first instance, to treat them repeatedly with alcohol, evaporate the solutions, and to act upon the residuum with water sharpened by acetic acid; without this precaution, it would be difficult to separate the morphine from the fat which is formed *abundantly*, during the sojourn of the body under ground; that the liquor may, if necessary, be rendered colourless, by heating it with *purified* animal charcoal, and repeated filtration through the same substance, without resorting to the sub-acetate of lead and hydro-sulphuric acid, the use of which has appeared to us to be at least useless; 5th, that it is apparent, in comparing the action of the nitric acid and of the trito-hydrochlorate of iron, upon the substances which were the object of the preceding experiments, that they were *invariably* reddened by the former, even when slightly discoloured, while the latter communicated a *blue colour*, in general, only when they were entirely colourless, and in certain cases imparted a reddish tint, even

when they were so; 6th, that in a case of juridical exhumation, it would be rashness to pronounce *affirmatively* that the person had been poisoned by a preparation of morphine, *merely* from observing the *red* and *blue* colour of which we have just spoken; that these phenomena afford, at most, only a slight presumption that such has been the case; 7th, that there will be no room for doubt if crystallized morphine be obtained, insoluble in water and ether, soluble in alcohol and nitric acid, fusible at a gentle heat, *reddening* on the addition of nitric acid, and assuming a blue colour when treated with the salt of iron, — possessing, in a word, all the known characters of this base; in such a case, the matter under consideration may be *affirmed* to be morphine.

*Hydro-chlorate of Brucine.*—On the 29th of March, 1826, eighteen grains of this salt, dissolved in a pint and a half of water, were introduced with intestine into a wide mouthed vessel, and exposed to the air. July 10th, the liquor, which from the beginning of April had exhaled a very fetid odour, having been filtered, and precipitated by ammonia, yielded upon evaporation a product of a white colour, slightly inclining to yellow, which *reddened* strongly under the action of nitric acid. On the 12th of May, 1827, thirteen months and a half from the commencement of the experiment, the liquor restored the colour of litmus paper which had been reddened by an acid; it was turbid and brownish; assumed a dirty yellow colour when filtered, and furnished, when evaporated at a gentle heat, a solid yellowish product, which changed to a beautiful *red* colour on the addition of nitric acid; the portion thus reddened, passed to a *violet* when gently heated with a little proto-hydrochlorate of tin. This solid product was partly soluble in cold water; the filtered solution had a yellowish colour, and bitter taste; it was decomposed by ammonia, which precipitated *brucine* in a perfectly recognizable state.

*Hydro-chlorate of Brucine greatly diluted with Water.*—July 18th, 1826, six grains of the above salt, dissolved in a pint of water, were exposed with intestine, in the manner already mentioned. On the 13th May, 1827, ten months afterwards, the liquor was filtered, and deprived of its colour by heating it with animal charcoal, through which it was also passed repeatedly; evaporated to dryness at a very gentle heat, it yielded a slightly coloured product, which assumed a beautiful red when treated with nitric acid, and a violet colour, with the proto-hydrochlorate of tin.

*Solid Hydro-chlorate of Brucine.*—November 8th, 1826, twelve grains of the solid hydro-chlorate of brucine, meat, the white of an egg, and *soupe maigre*, were introduced into an intestine, and the whole enclosed in a thin deal box, and buried at a depth of two feet and a half. At the expiration of ten months it was disinterred, and the contents of the intestine repeatedly treated with boiling alcohol; the solutions were afterwards mixed together, evaporated to dryness, and the product agi-

tated with water, sharpened by acetic acid, in order that it might dissolve all the brucine, and not act upon the fatty matter; the solution, rendered colourless by charcoal and evaporated to dryness, afforded a bitter, yellowish residuum, which assumed a beautiful red colour by the action of nitric acid, and passed to a violet when subsequently treated with the proto-hydrochlorate of tin.

These experiments prove that the presence of brucine and of its hydro-chlorate, in the intestinal canal, may be demonstrated in a case of juridical exhumation, even several months after death. But here, as with acetate of morphine, the colours developed by nitric acid and the salt of tin, should be considered only as presumptive of poisoning, and in order to *affirm* that such has been the case, it will be necessary to separate brucine or its salt, and demonstrate its different characters.

*Acetate of Strychnine.*—May 11th, 1827, six grains of this salt, dissolved in a pint and a half of water, was exposed with intestine, as in the preceding case. On the 8th of August following, the mixture exhaled a fetid odour; it was filtered, evaporated to dryness, and the product treated by alcohol, deprived of its colour by animal charcoal, and again evaporated, yielded a yellowish residuum, which acquired a *very beautiful red colour* on exposure to nitric acid, and possessed an insupportable *bitterness*, analogous to that of the salts of strychnine. It appears, therefore, that a salt of strychnine may be detected several months after its admixture with animal matters, even when the mixture has been exposed to the air.

*Hydro-cyanic Acid.*—The experiments of M. Lassaigne have proved, that minute quantities of this acid cannot be detected by chemical means, three days after death. Its disappearance in this case, is owing to its decomposition. (*Journal de Chimie Medicale*, Memoire de M. Lassaigne, tom. ii. p. 261.)

*Opium.*—A drachm of opium divided into pieces, a pint and a half of water, and several portions of intestine, were exposed together in a large mouthed vessel, May 16th, 1827. The sixth of August following, the mixture, which had become excessively fetid, was filtered; in the matter remaining on the filter, fragments of a reddish brown colour were observed, which at first sight might have been taken for opium, but they possessed neither the odour nor texture of that article. The filtered liquor had a brownish colour, and strongly reddened litmus paper; it was treated with magnesia, alcohol, and animal charcoal, as in the process for separating morphine, and a solid, yellowish white, bitter product was obtained, to which nitric acid imparted a *beautiful red colour*; the trito-hydrochlorate of iron, however, changed it to a red, instead of a blue colour.

November 8th, 1826, a large intestine, containing ten grains of the aqueous extract of opium, the white of an egg, meat, and *soupe maigre*, was enclosed in a thin deal box, and buried at a depth of two feet and a half. On

the 18th July, 1827, nine months and ten days afterwards, it was disinterred, and the contents of the intestine treated repeatedly with warm distilled water, and subsequently with magnesia, alcohol, and animal charcoal, furnished a light residuum of a gray colour, slightly verging to yellow, which had a bitterish taste, and assumed a *clear orange red, but not very deep colour*, when treated with nitric acid; the trito-hydrochlorate of iron did not convert it to a blue.

It follows from these experiments, 1st, that the morphine which exists in opium, is not decomposed by contact with animal matter, any more than that which forms part of an acetate or other salt; 2d, that there is, nevertheless, more difficulty in demonstrating the presence of this base, in a case of exhumation of a dead body, into the alimentary canal of which opium has been introduced, than when the question relates simply to a salt of morphine; 3d, that in any case, we cannot pronounce *affirmatively* as to the fact of poisoning by opium, unless this article be recognized with its *physical and chemical properties*, which may be done even several days after death, or if this be impossible, unless morphine, possessing all the characters above indicated, have been extracted,—and even in this case, we are not absolutely to conclude that the poisoning has been effected by opium in substance, but either by opium, morphine, or a salt of morphine.

*Cantharides*.—An intestine, containing a drachm of pulverized cantharides, the white of an egg, and pieces of meat, was buried in a deal box, on the 8th of November, 1826. Disinterred on the 13th of August, 1827, the contents of the intestine were converted into a fatty matter, scattered over with a multitude of brilliant points, of a beautiful green colour, which were formed by the powdered cantharides. Treated by boiling water, the fat melted, and swam upon the surface in form of an oily layer, while the shining particles fell to the bottom; a sufficient quantity was thus collected, to place their nature beyond doubt.

We will not conclude this memoir, without resolving a question which might be asked. "The poisons which you have discovered in these different exhumations," it may be said, "were not brought into contact with our organs until after death; are we to infer, therefore, that they would be discovered in like manner, in researches upon the bodies of individuals who had been poisoned during life?" We shall reply in the affirmative, provided at the moment of death, there remain in the alimentary canal, a quantity of the poisonous substance, appreciable by chemical means. What does it signify whether the action of a poison upon our tissues, during life or after death, is or is not the same, or whether a portion of this poison has been absorbed, rejected from the stomach, or evacuated by stool, during the life of the individual? the capital point is to determine, whether a *quantity of poisonous matter*, which might have been discovered upon opening a dead body

24 hours after death, can be detected ten, fifteen, or twenty months after inhumation. Now our experiments leave no doubt on this subject, since these poisonous substances do not comport themselves otherwise in the alimentary canal of an interred body, than when mixed with articles of food, and enclosed in the stomach and intestines, as in the experiments narrated.

From the Glasgow Medical Journal.

CASE OF ASCITES, *in which the Abdomen was tapped through the Fundus of the Bladder, and an attempt made to establish a Fistulous Communication between the Bladder and Abdomen.* By ANDREW BUCHANAN, M. D., one of the Surgeons to the City Poor.

The mode of operating, and the subsequent treatment adopted in the following case, are I believe new; at least, I have not been able to find any traces of them in the surgical works I have looked into, nor in the course of the inquiries I have made among my professional friends. In the first volume of the Medical Communications (p. 361,) Mr. Henry Watson describes an operation for ascites, which he had practised three times successfully. Finding the vagina forming an external protrusion from the pressure of the fluid above, he introduced the trocar into the abdomen in that situation, preferring it to one higher up, as ensuring the more complete evacuation of the dropsical fluid. Mr. Watson's operation more nearly resembles the one here proposed, than any other I have read or heard of. My researches, however, upon this subject, have not been very minute, conceiving it of little moment. It is far more important to inquire, whether the operation described below be in any respect superior to the one commonly practised, and whether the plan of treatment subsequently adopted be calculated to supersede the necessity of repeating the operation; but it will be best, before entering upon these inquiries, to give the history of the case which suggested them.

Mrs. H. a middled-aged, married woman, in October 1826, was put upon the list of sick paupers, for ascites. By her account, the swelling of her belly had commenced about 18 months before, and after increasing to such a size as to induce her to consider herself pregnant, had gradually subsided, without the employment of any medicinal means. Twelve months ago, however, the swelling again appeared, and soon after became attended with anasarca of the legs.

When I first saw her, the abdomen was very much distended with fluid, the legs anasarcaous, and the urine scanty, with much debility and emaciation. Deriving no relief from the purgative and diuretic medicines prescribed for her, she was sent to the Infirmary. Medicinal treatment was there in like manner unavailing, and she was tapped before leaving the house.

On the 10th of November she again came under my charge; and as tapping was the only mode of affording her relief, that operation was performed upon her in the usual way, three times successively, each time about 28 pints of aluminous fluid being drawn off.

When the belly was emptied of fluid, and the hand applied to the left side, the spleen was felt very much enlarged. I was therefore led to think, that the liver was most probably diminished in size, and its substance converted into yellow tubercles, that being a combination I have frequently met with, and always found to occasion an incurable ascites.

The poor woman's case seemed thus utterly hopeless; for it was clear she could not long survive, if harassed by the continual re-accumulation of the water, and the repetition of the tapping. I was therefore induced to try an operation, which, if it succeeded according to my wish, would procure her more permanent relief. This operation consisted in tapping the belly through the upper and fore part of the urinary bladder. I conceived, that in the first place, the water might be drawn off in this way much more completely, than by the usual operation; for the puncture being made in the most depending situation, the whole fluid could be drawn off; whereas, when the puncture is made higher up, a considerable portion must remain lodged in the cavity of the pelvis. In the next place, I imagined, that the re-accumulation of the fluid might possibly be prevented, by the wound remaining in a fistulous state, being prevented from closing by the secreted serum constantly oozing downward into the bladder, to be thence discharged along with the urine. I had, however, well considered the possible danger and inconvenience that might be produced. I did not apprehend any difficulty in performing the operation; for it appeared to be, as I have since found it, both safe and simple. What I was most afraid of, was the risk of the urine getting into the cavity of the abdomen. On consideration, however, that risk did not appear so great as I at first supposed. If the contraction of the bladder were the sole expulsive power by which the urine is evacuated, that power, it is clear, would act with equal effect in forcing the urine through a hole in the fundus of the bladder, as through the orifice of the urethra. The principal agents, however, in expelling the urine, are the diaphragm and abdominal muscles, the action of which must tell chiefly on the lower part of the bladder, while it tends to shut up any opening in the fundus. This reasoning so far satisfied me, that I judged it right to attempt the operation, as the chance of doing good to the patient in this way had, in my mind, a decided preponderance. I therefore consulted my friend Mr. Watson on the subject, and had the satisfaction to find that his opinion coincided with my own. With this coincidence, I was the more gratified, as, from the extreme exhaustion of the patient, it was very probable, that whatever operation was performed, death might soon after ensue, and it might then have been supposed

that the mode of treatment had accelerated her fate.

Next day (Thursday, January 4th, 1827,) I proceeded to perform the operation, assisted by Mr. Alexander. The patient lay upon her back across the bed, her feet resting on a stool placed at the bedside. The instrument employed was the curved trocar, for puncturing the bladder in case of retention of urine. The canula was first introduced along the urethra, and the point of it carried towards the upper and fore part of the bladder, pushing it as far up as possible, so as to put the coats of the bladder over its orifice upon the stretch. The stilette was then passed along the canula, and made to pierce the bladder, which it did with such ease, that no sensible resistance was experienced. On withdrawing the stilette, the water flowed in a full stream, till nearly as much had been drawn off as on any former occasion. Some interruption then took place from time to time, seemingly from the fluid subsiding below the level of the orifice of the canula, or from portions of intestine covering it up. By drawing the instrument downwards, however, and shifting it about, the water continued to flow freely, and upon the whole, a greater quantity was drawn off than had ever been done before, although a shorter period intervened between this and the last operation, than on any former occasion. Some pain and bleeding were produced on passing the canula along the urethra, the orifice of which was obstructed by large carunculæ; pain was also excited by the stretching the bladder, and the piercing of it.

Felt much relieved by the operation. Passed the night without pain, but was restless and slept little. Vomited this morning, and still feels squeamish. No pain of abdomen on pressure. Pulse, as for some days past, about 100. Tongue also as before, pretty natural after drinking, but soon becoming dry in the middle. Several loose stools. Urine passed freely, and in much larger quantities than before the operation, containing some clots of blood, most probably from urethra. Ordered some soup.

January 6th.—Sickness diminished, and no pain any where. Moans, however, as if uneasy. Belly tympanitic. Soup, of which she had taken about four times the quantity ordered, had refreshed her, but produced purging, probably also the tympany just mentioned. Urine passed in a forcible stream, and, on introducing the catheter immediately after, bladder quite empty. Castor oil and a little solid food.

7th.—Vomiting returned; only of ingesta. No pain. Tympany diminished. Pulse 108, softer than yesterday. Tongue moist. Urine comes freely. Castor oil not taken. Relished food. Common bolus.

8th.—Pulse 104, soft. No vomiting since 10 last night. Less urine. Still tympany.

9th.—Pulse 100, soft. Tongue moist and pretty clean. Vomiting occasionally; also pain of stomach, relieved by some pepper taken in warm milk. Urine more scanty.

10th.—Pulse 108. Tongue clean and moist. Much debility. Belly fluctuates. Urine very scanty.

14th.—Pulse 84. Tongue moist. Abdomen larger, with distinct fluctuation. Urine scanty.

20th.—On the evening of the 17th, the swelling and tension of the abdomen had increased so much, that, at her own urgent request, the water was again drawn off. The operation was performed exactly in the same way as before. She seemed to suffer very little pain, except from passing the instrument along the urethra. When about 24 pints had come away, the canula was withdrawn, although there was still sensible fluctuation in the abdomen. This was done with the view of facilitating the formation of a fistulous opening into the bladder. Next day, when I called to see her, she was out of bed, sitting by the fire-side, which she had not been able to do since the previous operation. Urine passed with ease, but in small quantity, much less than the day after the first operation. On examining it, and comparing it with some voided before the tapping, it had exactly the same appearance. It was high coloured, and deposited on cooling a copious pink sediment, *resoluble* on applying heat. No trace of albumen could be detected in it, either by heat, or nitric acid. When visited to-day, had gone out.

February 8th.—On the 23d ult., the swelling had increased so much, that at her own request, the tapping was again resorted to. As, from the result of the two trials already made, there did not seem much chance of a fistulous opening being established by the mere oozing of the dropsical fluid, I began to think of some other means of ensuring the accomplishment of the object. Mr. Watson suggested the introduction of an elastic catheter, to which, however, I preferred a catgut bougie of the smallest size, which would not prevent the transmission of the urine along the urethra. When the water had been nearly drawn off, I intended to introduce the bougie through the canula, and then withdrawing the canula, to leave the bougie in its place, to be there fastened, and kept for some days, occasionally moving it a little.

In performing the operation on the two previous occasions, the principal pain was produced by the sharp edge of the canula irritating the sides of the urethra. To prevent this, I passed along the canula, before introducing it, a white bougie, the end of which exactly fitted the upper orifice of the canula, while the body was shaven down so as to admit of its being passed. By this means the canula glided along the urethra without producing any pain. The bougie being then withdrawn the stilette was introduced, and the fundus of the bladder pierced as before. The water did not come away so freely as formerly; and it was likewise tinged with blood. These appearances induced me to think, that the end of the canula had not passed fairly into the abdominal cavity, but was overlapped

in part by a fold of the bladder. I have seen the very same appearances produced by the end of the canula not passing fairly beyond the parietes of the abdomen. When about four pints had been drawn off, we were obliged to desist by a diarrhoea, to which the patient had been subject for some days previous.

This accident prevented the trial of the proposed plan of introducing the bougie, and the patient conceiving an aversion to the new mode of operating, the water was drawn off in the usual way, on Sunday the 27th ult. Fully more than the usual quantity came away.

February 9th.—Yesterday the tension of the belly again producing the most distressing suffering, the patient wished the water drawn off; and though her debility was now so extreme, that I entertained no hope of her long surviving, yet as I had already more than once been deceived in my prognostic regarding her, I judged it right, to give her the small chance, which the use of the bougie held out, of obviating the necessity of repeating the tapping. The patient was easily induced to consent to the trial, her only objection to the *bladder operation*, arising from the exposure of her person, while she preferred it as giving her less pain than the other operation, the cut required for performing which, on the 27th ult., was not yet healed.

The canula being introduced armed with the bougie as on the last occasion, the water flowed away in a full stream till nearly the usual quantity was obtained. Then, while there was still distinct fluctuation in the belly, and the stream of fluid uninterrupted, the catgut bougie was passed along the canula, till it had gone a couple of inches beyond the upper orifice into the abdomen, as was ascertained by two marks on the bougie, one the length of the canula from its upper extremity, and the other two inches farther off. The bougie being now held firmly, the canula was drawn over it, as also over another larger one, applied to the end of the small one, for the purpose of keeping it steady in its place, till the canula was completely disengaged. Tapes were fastened to the bougie in the same way as for keeping a catheter in the bladder. The debility of the patient was so great, that I was afraid more than once, she would not outlive the operation.

During the night, about a quart of fluid came away. To-day about 3 P. M., some water was obtained for examination, but it gave no trace of albumen. The patient had, however, previously been out of bed in a room adjoining her sleeping room, so that the end of the bougie might have been displaced. Says she feels easy. Pulse quick and feeble.

February 24th.—When visited on the 10th inst. the day after last report had gone out, and that day, as she afterwards stated, the bougie came away. On the 20th, the water having re-accumulated so as to produce more swelling than on any former occasion, the fundus of the bladder was again penetrated, and 48 pints drawn off. The stream not flowing freely at first, I withdrew the canula and made a second

puncture. The increasing debility and wretched accommodation of the patient, prevented any farther attempt at the establishment of a fistulous opening. Next day felt much relieved, but very feeble. To-day, when visited, was sitting by the fire, in a room adjoining that where she slept. Swelling beginning to return, but not yet productive of any uneasiness.

March 3d.—On the 27th ult., I was very much surprised at receiving a visit from my patient. She came to inform me of the re-accumulation of the water, and I promised to draw it off for her next day. Her extreme emaciation, hollow eyes, and ghastly appearance, contrasted with the extraordinary prominence of the abdomen, excited the attention of the passengers, as she walked along, with no other support than that of a stick. As this visit showed a degree of vigour, I had no idea she possessed, I resolved to attempt once more the establishment of a fistulous opening in the bladder. The mode of doing it, I had been thinking of some time before, but not expecting so soon to have an opportunity of trying it, I had only adverted to the general principles of the operation, without attending sufficiently to the mode of reducing them to practice. I proposed, before the fluid in the abdomen was fully evacuated, to introduce through the canula a small inflated bladder of a longish shape. This bladder, being introduced into the canula according to its long diameter, would admit, by a little compression, of being forced along, and, when again free to expand in the abdominal cavity, would float in the dropsical fluid, while its size and buoyancy together, would prevent it from descending through the opening made by the trocar. Attached to this floating bladder a silk thread was to pass into the urinary bladder, and thence along the urethra, to be fastened externally. The essential part of the apparatus, however, was to consist of three or four thick worsted threads, having the same attachment, at the upper end, as the silk one, and passing along with it into the bladder, but no farther, their lower ends being fastened to the portion of the silk thread within the bladder. Along these worsted threads, I presumed, the dropsical fluid would ooze down into the bladder, partly by gravity, and partly by capillary attraction, in the same way as in the well known experiment, by which a tumbler of water may be emptied by similar threads passing over its side. When this apparatus had remained a sufficient length of time to prevent the wound of the bladder from closing, or if, from the occurrence of irritation, or any other cause, it were thought advisable to withdraw it, that could at once be effected by re-introducing the canula, and guiding it upward along the silk thread, till it came to the small bladder, which being punctured with a fine stilette, the whole could be withdrawn. If, on the contrary, no irritation supervened, and it were not judged prudent to withdraw the apparatus, it might perhaps be possible to dispense altogether with the silk thread, passing along the urethra, that

thread terminating like the worsted ones in the urinary bladder, and being there attached, exactly as at the other end, to a small inflated bladder, which would prevent the threads from being drawn up into the abdomen, and probably also, by floating in the urine, render them less apt to be crusted with saline matter.

A small bladder, strong enough to admit of the necessary compression being the only thing wanted, I first thought of the swim-bladder of a fish. Next day I procured some fish, but found the swim-bladder, and also the bowels, far too weak for the purpose. I delayed another day, that I might try the intestines of some birds, but these being also insufficient, I thought I could no longer protract the suffering of the patient by deferring the operation. I accordingly punctured the bladder upon the 1st inst., drawing off about the usual quantity of fluid.

March 8th.—When visited, the day after the operation of the 1st inst., she had gone out, and I heard no intelligence of her till the 5th, when her husband came to inform me of her being very ill. That afternoon I visited her and found her speechless, and exhibiting no mark of sensibility, except moaning when moved from one position to another. By the account of her husband, she had been nearly in the same state since the 3d inst. She got some whisky toddy, and survived till yesterday morning.

*Dissection.*—There was a good deal of swelling of the belly, arising partly from a tympanitic state of the upper part of the colon, and partly from the re-accumulation of the dropsical fluid, with which the pelvis, and about a third of the abdomen, were filled. The liver was shrunk into a very small compass in the right hypochondrium. It was nodulated, and uneven on the surface, and completely tuberculated throughout. Spleen considerably enlarged. Kidneys healthy. The peritonæum had everywhere its usual glistening aspect, totally free from any appearance of inflammation, as might indeed have been inferred from the copious secretion of serum, which could not have gone on, had an attack of inflammation supervened on the last tapping.

The state of the bladder was the point I was most anxious to ascertain. On the fundus of it five marks from puncturing could be distinctly recognized. They were all between the uterus and the symphysis pubis, three of them within a few lines of each other, and the other two about an inch distant. The last two punctures could be most easily recognized. The most recent, made six days before death, had left a spot of a deep red, or rather grayish black appearance, rather larger than the orifice of the canula. It had very much the appearance of a spot produced on the skin by blood extravasated under the cuticle, being distinctly circumscribed, and the surrounding peritonæum perfectly healthy, and free of inflammation. The muscular coat had adhered, the opening being perfectly closed. The wound from the second last puncture, made fifteen days before death, had exactly the tri-

cuspidate shape, and the size of a leech bite, three minute ulcerated lines meeting in the centre, and the ulceration not extending deeper than the peritonæal coat. Nothing could be more exact than the resemblance of this wound to a leech bite, which had been prevented from healing by the application of a poultice, only there was neither surrounding redness nor swelling. In a third wound, the tricuspidate shape could no longer be recognized, but a very small superficial ulcer was still distinctly visible.\* In a fourth wound, most probably made 27 days before death, I was at a loss to say whether any ulceration remained, but I was inclined to think there was still a little. The fifth puncture was completely cicatrized, and observed only when minutely inspected. As the operation was performed six times, and at one of them the bladder was twice punctured, the situation of two of the wounds remained undiscovered. I wished to bring away the bladder entire, that I might examine it more minutely at leisure, but this the poor woman's husband would by no means permit; and while endeavouring to persuade him to allow it, I regret to say, that I forgot, while it was yet in my power, to examine the state of the mucous coat of the bladder.

The history of the preceding case has been so minute, and the discussion of the most important parts of it so blended with the historical details, that there is little room left for general observations. I shall, however, offer a few remarks, first, on the operation of tapping through the fundus of the bladder; and, second, on the establishment of a fistulous communication between the bladder and abdomen.

The operation of tapping the abdomen through the fundus of the bladder presents no difficulty in the performance, to any one who can introduce the catheter, and knows the relations of the bladder to the other viscera of the abdomen. For operating on the female, perhaps no better instrument could be devised than the armed canula, and stilette employed in the preceding case. In the male, the operation would be more difficult, from the length and curvature of the urethra, and the want of a proper instrument; it would, however, I think, require little ingenuity to adapt such an apparatus to a male catheter, as would perfectly answer the object in view. Of hæmorrhage, there seems to be less chance in this operation than in the ordinary one; and as for wounding the viscera of the abdomen, I should think it scarcely possible, with an ordinary degree of care; for not only are the intestines kept out of the way by the position of the patient, and the accumulation of fluid in front of them, but if the canula be carried fairly up to the fundus of the bladder, the stilette will pierce the membrane stretched over the mouth

of the canula, with as much ease as a bit of paper, and it may therefore be introduced with so much caution, that a noose of intestine accidentally above it (if not actually adherent) would be pushed aside without being wounded. The pain produced by puncturing the bladder is very inconsiderable, not greater, I should think, than that occasioned by opening a vein of the arm. The dropsical fluid is more completely evacuated than by the ordinary operation. The wound inflicted heals readily, and does not occasion any inconvenience while it remains open. The patient in the preceding case was generally out of bed, and felt no pain the day after the tapping; whereas, when the ordinary operation was practised, the wound was yet painful and unclosed, when it became necessary to repeat the operation. Lastly, if, as is thought by many, the inflammation of the peritonæum after tapping is in some degree to be ascribed to the access of the atmospheric air to the abdominal cavity, that cause of irritation is avoided by operating through the bladder. To counterbalance these advantages must be placed the risk of introducing urine into the abdominal cavity; the amount of that risk, however, can only be determined by experience; and so far as the experience derived from the preceding case entitles us to judge, it does not constitute a serious ground of apprehension.

As for establishing a fistulous opening between the bladder and abdomen, by some such apparatus as that described in the report of March 3d, I think the objects to be obtained of sufficient importance to warrant the experiment. Many objections to it have indeed occurred to myself, or been mentioned to me by my professional friends, as the insufficiency of the apparatus, owing to the collapse of the bladder from the pressure of the superincumbent fluid—the danger of the influx of urine into the abdomen—the irritation of a foreign body in the bladder and abdomen—the formation of a hernia, and the furnishing a nucleus for urinary concretions. Objections like the last, founded on circumstances which can only become formidable in the course of years, can scarcely be urged against a plan, intended to relieve a disease, which threatens to be speedily fatal. What weight should be attached to the other objections, it would, as appears to me, be idle to examine, since the most ingenious reasoning, derived from general principles, can never produce conviction in questions which experiment alone can decide. If the operation of M. Dupuytren for the cure of fistula lachrymalis had been first announced to the world, not under the sanction of experience, but in a speculative form, there can be little doubt, that critics would have contended in denouncing it as visionary and impracticable. I cannot but think, therefore, that to form a decided opinion of the practicability of the plan here proposed from abstract arguments alone, would be little consistent with the spirit of the inductive philosophy. How the bladder will comport itself in the circumstances here supposed, and how the adjacent

\* This wound I was disposed to think of the same date as the last, two punctures having been made at the operation of the 20th ult.

parts will be affected, we do not know, because we have not tried. Whatever conjectures we may form as to the probable result of the experiment, it is by trying it alone that a certain judgment can be obtained. I should be glad, therefore, that this paper were to fall into the hands of medical gentlemen having the charge of hospitals, or other public institutions for the cure of the sick, that if any of them were disposed to take the same views as myself of the mode of treatment proposed above, a trial might be made of its efficacy.

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**CASE OF MONOMANIA, CAUSED APPARENTLY BY CIRCUMSCRIBED CHRONIC MENINGITIS, WITH REMARKS. By DAVID SCOTT, M. D.**

John Anderson, M. D., aged 41, of fair complexion, sanguine temperament, and endowed by nature with a healthy constitution, was, after the general peace in 1815, placed on the half pay of the navy, in which for many years he maintained the character of an active and zealous member. He came to reside in Cupar, his native town, where, from the extent and variety of his information, the sociableness of his habits, and urbanity of his manners, his company and conversation were generally courted. He contemplated settling somewhere, but his designs in this respect were always frustrated; and from a repetition of disappointments, conjoined with domestic causes, he became liable to occasional fits of despondency. About the year 1820 his intimate friends began to remark a peculiarity of manner, and singularity of ideas; but these were only occasional, and during times of temporary excitement. Addicted to study from early youth, and distinguished for his acquirements in learning, he continued to read much both of English and French authors; and in the course of his reading he met with some articles that treated of animal magnetism, which struck his attention, and made a great impression on his mind. Pondering long on this subject, and beguiled by the subtle and speculative reasoning of these authors, he began first to admit the possibility of animal magnetism, and then imagined that he himself was subject to its influence. Soon afterwards, from an occasional incoherence of action, and strangeness of exclamation, it became apparent to his friends that this opinion was assuming an ascendancy over his mind. They endeavoured of course to scout it as nonsense, and to rail at him for allowing so vague a chimera to disturb his thoughts; but still the idea gained ground, and in a few years took so firm a hold of his imagination, that what was formerly merely admitted as possible, now became matter of serious truth and cause of apprehension. He no longer attempted to conceal his belief, but avowed it openly, and even accused some of his best friends and acquaintances of being accessory to keeping him under its influence,

and holding him in a continual state of alarm. His nights in consequence became greatly disturbed, and he seldom obtained any satisfactory rest. If he did sleep, he was tormented by oppressive dreams and other strange phantasms. His notion of animal magnetism was, that certain individuals, who had an antipathy to him, could wield at will an influence over him of so malignant a nature as to deprive him of every kind of enjoyment, and keep him in a continual state of discomfort and anxiety. He invested these *invisibles*, as he called them, with vast power. No place was proof against their malignity, nor could distance restrain it. He went to Paris in the year 1822, with the view of escaping from it, but he found its influence there as great as at home. He frequently during the night could hear his enemies planning schemes for his annoyance. In his imagination they had recourse to every kind of torment which the most wicked and inquisitorial minds could invent, and were inexorable and persevering in their attacks. In the night-time, for example, they would rest on his breast with the weight of a millstone, deprive him of sleep, disturb his digestion, lock up his bowels, &c. &c.; at other times they would stimulate the bladder and rectum so powerfully and so immediately, that he had not time to undress himself; and on some occasions they would take such unwarrantable liberties with him, that he was compelled, in self-defence, to roar out loudly, by which he thought he obtained a relaxation of annoyance. Several times he made application to the local authorities to control their malignity, and even took bond from some of his acquaintances that they should cease to disturb him. On all other subjects, saving animal magnetism, his judgment was sound, and indeed in reasoning he evinced much acuteness; a stranger, in short, when the peculiar subject was not agitated, could not detect any thing unusual about him. Latterly, however, he complained of an impairment in memory, and that of a peculiar kind. He could not recollect the words he was accustomed to express his ideas in; he would therefore stop during speech and labor for them; he seemed to perceive them as it were in his mind, but could not get his recollection to lay hold on them. He continued in this state for several years, without apparently suffering in health, or losing much flesh.

About a year before his death, he had several attacks of pneumonia of the left side. On these occasions, though seriously ill, there was considerable difficulty in getting him to submit to medical treatment. Having a great aversion to medicine, he considered it in his case as useless and inert, and by no means calculated to remove the cause. At these times the only chance of success with him was, to admit his premises, but to draw very opposite conclusions. With this view we acceded to his notions about animal magnetism, and held, that, so long as its influence was applied to the body generally, its effects were innocuous; but so soon as so powerful a stimulant

was for any length of time directed upon any single organ, the result could not be otherwise than hurtful, and would terminate in inflammation. In this manner, though he pertinaciously retained his opinion as to its cause, he would allow us to combat its supposed effects, and submitted to be bled, blistered, &c. &c.

For several months before he died he had a short dry cough, accompanied with a sibilous sonorous respiration; he complained also of a severe pain in the back, with an oppression and tightness across the chest, which he compared to an iron girdle. He was greatly dispirited, and confined himself very much to the house. His pulse, (unless when he was labouring under one of his inflammatory attacks, when it was hard and quick,) felt always soft, equable, deep-seated, and feeble, but never intermittent. The sound of the heart was hardly perceptible, but its contractions appeared regular. The lower portion of the left breast and back sounded fleshy on percussion, and the respiration in these places was not perceptible by the stethoscope; but higher up, and opposite the bifurcation of the trachea, it was sibilant and sonorous. It was considered that, in addition to monomania, he laboured under chronic bronchitis, with some hepatization of the lower portion of the left lung; and from the treatment applicable to these he derived relief, though the noisy respiration never left him.

On the day of his death he had invited a few friends to dine with him. They had assembled, and all was ready; when suddenly their worthy host was seized with coughing, hawking, and spitting of blood, which increased so rapidly, and came forth in such vast quantity, that in a few minutes, being unable to expel it, he died suffocated.

*Dissection.*—The extreme suddenness and singularity of his death created a desire on the part of his acquaintances, that the cause of it should be ascertained; I therefore examined the body in company with Drs. Grace and Spens. The body was not emaciated, there being a considerable quantity of fat in the cellular membrane. In the cavity of the abdomen we could discover nothing unusual, unless that the liver was considered larger than natural, but not diseased in structure. In the chest, after the sternum and cartilages of the ribs were removed, we found about a pint of fluid blood in each of its cavities. There was interlobular adhesion of the left lung, but none between the *pleura costalis* and *pulmonalis*. Its inferior lobe felt hepatized and completely gorged with blood, the bronchii and air cells being filled with it. After tying the blood-vessels in the neighbourhood of the heart, we endeavoured to remove the trachea, bronchii, and both lungs from the cavity of the chest, in order to give us more room to trace the source of blood. In doing this we discovered a large aneurism of the descending thoracic aorta. It had its seat in that vessel immediately after it gives off the left subclavian, contained about a pound of fibrinous matter in concentric layers, occupied the

whole cylinder of the tube, and pressed strongly on the roots of the bronchii, and on the vertebræ of the back, so that a considerable portion of the vertebræ was eroded and absorbed. On minutely examining its connexions with the surrounding parts, we found a large opening communicating with the left bronchial branch of the trachea, the rupture into which sufficiently explained the suddenness of death, and the presence of blood in that tube and other parts of the lungs. The right lung, though filled with blood, was otherwise perfectly healthy.

We opened the cavity of the cranium, and observed with some attention the condition of the brain. The veins on its surface, and particularly where they entered the longitudinal sinus, were very turgid with blood, but this we considered as accidental, and arising from the suffocating manner of death. There was an inflammatory deposit, apparently of old standing, under the arachnoid coat, with thickening of the membrane itself, and adhesion to the parts beneath for about the space of an inch and a half in length, and one in breadth, on each side of the longitudinal sinus, midway between the *crista galli* and the level of the commencement of the lateral sinuses. The cortical substance of the brain under this spot was not changed in appearance, or altered in structure. We could observe no other morbid alteration in any part of the membranes or of the brain proper, either as to softening or hardening or other morbid change. Nor was there any watery deposit in the lateral ventricles, or any thickening of their lining membrane. The cerebellum, *tuber annulare*, *medulla oblongata*, and all the nerves issuing from the brain, appeared to be perfectly natural.

*Remarks.*—We have seen from the dissection that our patient laboured under not only an illusory complaint, but also under a serious organic disease, which escaped detection during life. This is one of the misfortunes to which people in his condition are liable. They feel a variety of imaginary sensations, sufficiently intense for the time, but, from their evanescence and sudden change of character, they are little regarded either by the patient himself or his attendants. Occasionally, however, these deceptive symptoms are mixed with those arising from actual disease; and it is from a combination of this kind that arises the difficulty of separating the true from the false; but whenever they begin to assume a permanency of character and situation they ought to be considered as real, and demanding attentive consideration. For example, the fixed burning pain which my patient long complained of in his back, and a sharp lancinating pain which he referred to the left ventricle of the heart, were both evidently owing to disease; the first caused by the erosion of the vertebræ of the back from the pressure of the aneurism, and the latter to inflammation of the lower portion of the left lung. Yet it is remarkable that both these pains frequently subsided and disappeared for

days and weeks, and were succeeded by others of an anomalous character. Here, therefore, the distinction between the true and false symptoms is not so easily drawn; and when the patient labours under the misapprehension of the nature of his complaint, and attributes all his feelings to some uncommon cause, as in the present instance, it is exceedingly difficult to arrive at a just diagnosis. The sibilous and sonorous respiration was evidently produced by the pressure of the aneurism on the roots of the bronchii, particularly the left, diminishing the caliber of the tube, and thereby obstructing the ingress and egress of air to and from the lungs. There was no intermission of the pulse, because the aneurism was seated beyond the giving off of the subclavian arteries, neither had he ever any unusual pulsation in the chest, or any labouring or palpitation of the heart. The latter organ was found healthy, which accounts for its natural action; and the only indication of the diseased aorta was an indescribable anxiety and perpetual uneasiness, which he was unable to refer to any particular place, which constantly tormented him, and which he as constantly referred to some invisible agency. I do not think, however, that the disease in the chest had any thing to do with the aberration of mind, although it might contribute to exasperate it.

The hallucination in reference to animal magnetism is a subject much more difficult to unfold, as its investigation embraces the connexion subsisting between mind and matter. That this hallucination was caused by an irritation, or chronic inflammation of a fixed part of the surface of the brain, appears to me exceedingly probable; because, in the first place, an inflammatory deposit under the covering membranes, with adhesion to the substance beneath, was found on dissection affecting a small space on the surface of the brain; and, secondly, because the mind was unhinged only on one particular point. On all other subjects his judgment was entire, and he could discourse as rationally as any other man; and in like manner on dissection we found all other parts of the brain perfectly healthy. There would appear to be something here like a chain of cause and effect. I shall not go so far as to maintain with the phrenologists, that the mental powers of man and other animals depend upon the size and development of certain parts of the brain, or that the peculiarities of individuals are always owing to a peculiar conformation of that organ,—merely because, from not having yet directed my attention to that subject, I am unable to form any precise opinion on the matter. But from what I have heard, and the notice taken of it by some periodicals, I think it bears in the outline a great degree of plausibility.

It is exceedingly natural to think, that the organ through which alone are manifested the thinking powers or mind of an individual, must be influenced, or entirely subverted, according as the brain is either simply irritated, or altered and destroyed in structure; because the like thing happens in other organs of the

body. Farther, it is probable that all the nerves of sense have a distinct termination in the brain, or that certain portions of it are peculiarly destined for the development of these senses. Hence the sense of hearing may be affected without injuring the sense of sight. And if this condition hold with regard to the external senses, it is extremely reasonable to think that the like circumstance obtains with the internal, and that each has a located portion of the brain through which it is peculiarly manifested; and by consequence, if any of these parts be injured or destroyed, the properties of the mind which depend on them will in like manner be injured or destroyed. The difficulty of the science would appear to consist entirely in discovering these parts, and allotting to each its proper function. To accomplish all this would require an immensity of observation continued through many years, nay ages; but notwithstanding these acknowledged difficulties, if the proposition be founded in truth, they ought to deter no one, desirous of tracing nature to her utmost recesses, from attempting to surmount them; for if this object could once be attained, there is no doubt that it would greatly contribute to simplify the treatment of diseases of the brain. The mind itself is totally unsusceptible of disease; for who can suppose that an incorporeal substance is liable to diseased action. It is an obstruction or obscurity in the organ alone that causes an imperfection or obscurity in the mind, in the same way that dim spectacles will always convey images to the optic nerve in a dim manner. If they happen to be stained, then all images will come to the mind with the same hue; and should they be opaque, none will be transmitted. An opacity of the lens of the eye is no proof that the optic nerves are incapacitated from receiving the impression of images. If the opacity be removed by nature or by an operation, the individual will see as before. In like manner, if the obstruction or incapacity of the affected part of the brain be removed, the mind will be as entire as ever. These observations would go to prove that mania, monomania, hypochondriasis, &c. are corporeal diseases, and that we should first endeavour to remove the physical incapacity of the injured organs, and afterwards confirm the cure by regimen and moral restraint.

But while we give our cordial assent to the outline of the system advocated by the phrenologists, yet we are by no means certain, that the localities they have pitched on as the organs chiefly concerned in the evolution of the mind, if we may so speak, are correct; because, in the first place, it is very probable that it may require the co-operation of two or more organs to express even the most simple idea; and, secondly, I really do think it would require an immensity of observation, and comparison of heads of every size and form, besides dissections of cases such as the present, before even the function of a single portion of the brain could be permanently established, and far more before the whole of the organs

composing the brain could be reduced into any thing like system.

The phrenologists have greatly injured themselves by their pertinacity in adhering to system; for, instead of making observations, and recording facts to serve as materials for some future building of the science, they have at once assumed that their work is so perfect as to be practically useful. Now this rapidity of perfection has never obtained with any of the sciences depending on observation;—a remark, of which the sciences of medicine afford many a melancholy example.

From the London Medical Gazette.

**SUCCESSFUL EXTIRPATION OF AN HYDATID SITUATED IN THE PELVIS, and preventing the Passage of the Urine and Fæces.**

Mrs. B., 38 years of age, of a lymphatic temperament, suffered, about eight years ago, from an unusually long and difficult labour. The accoucheur found that this difficulty arose from a tumour in the pelvis, on the left side of the vagina, and did not conceal from the patient the obstacles that might render delivery in future even more difficult. This tumour increased slowly, but without occasioning any inconvenience for four or five years. Within the last three years, however, Mad. B. began to experience some inconvenience in going to stool and passing her urine. These impediments continued to increase. At length the ischury became complete, and passing the fæces almost impossible. The husband of the patient had learned to draw off the patient's water, which he did four or five times a day. Such was the patient's condition when, in the middle of March, she presented herself at La Charité. Upon examination, a resisting tumour was discovered on the left side of the vagina, extending from the margin of the pelvis to the basis of the labium. It pushed the vagina towards the right side, and appeared to be immovable; but the parietes of the vagina readily glided over it. M. Roux thought that he perceived a fluctuation at one point; but it was so obscure that it did not at all clear up the diagnosis. It was thought necessary to attempt its extirpation; but its situation, and the vicinity of the hypogastric arteries and their principal ramifications, rendered any operation serious: the melancholy condition of the patient alone appeared to warrant the attempt. MM. Boyer and Roux thought it practicable, though dangerous, and the last named gentleman performed it on the 20th March, without exactly knowing, when he commenced it, in what manner it was to be terminated. He had formed the project of exposing the tumour by means of an incision in the form of the letter T, one portion of which would divide the left side of the vagina longitudinally, and the other the labium lenhways. The two flaps of this incision might be dissected, and the tumour laid bare.

The patient being laid upon a table, the but-

tocks upon the edge, the lower limbs were supported by two assistants. The surgeon, standing in front, introduced the index finger of the left hand deep into the vagina; he then passed the blade of a straight bistoury along the finger, and turning the cutting edge towards the left side of the cavity, he made an incision from above downwards; immediately a diaphanous liquor flowed out, of a straw colour. The finger penetrated through this incision into a spacious cavity, to the sides of which membranous fragments appeared to be adhering. Some portions of these were removed with common dressing forceps, of a pearly white colour, and of a gelatinous consistence: these were evidently the remnants of hydatids. A pair of polypus forceps was then introduced, and a large body of the same nature extracted. The extensive cavity thus resulting from the extirpation of the hydatid was filled with charpie, to which a string was attached.

The day of the operation, the patient did not experience the least pain, but the urine passed away involuntarily. Towards evening fever came on, and there was no sleep during the night.

On the 25th, although the urine still passed away involuntarily, and there had been no fæcal evacuations, the patient was in other respects better. Some of the lint was removed from the cavity, covered with pus; the rest, still adherent, was suffered to remain. The next day the bowels were open; there was no pain, and every thing was going on well. In the evening of the same day an abundant hæmorrhage ensued from the wound, in consequence of the pipe of an injecting syringe having been incautiously introduced into it; the bleeding was stopped by plugging up the wound, but not until syncope had taken place.

On the 31st, there was no longer any doubt of the patient's doing well—the suppuration was becoming less from day to day.

This operation gave occasion to M. Roux to make some remarks upon hydatids. He observed, that these cysts were frequently developed in the internal organs, but that, as they did not often become surgical diseases, they scarcely ever found a place even in the most complete surgical works; nevertheless, he had often met with cases in which the nature of the tumour had only been ascertained after the operation. He mentioned two other instances.

One day, he was called upon to give his assistance to a woman who had a tumour at the navel, which was universally thought to be a rupture. Ulceration had spontaneously taken place upon its surface, exposing a membranous sac, which was believed to be that of a hernia, but it was not reducible. The woman was suffering from some of the symptoms of strangulated hernia. M. Roux thought it necessary to make some incisions, for the purpose of relieving the supposed strangulation; but, after having done so, he discovered that the tumour was merely an hydatid.

On another occasion, he was consulted by

a female who was affected with a large tumour, of an irregular surface, in the left breast. This tumour had existed about seven years, and had increased by almost insensible degrees. There never had been any lancinating pains; the glands in the axilla were sound, but there was a sense of weight and of tension in the surrounding parts. M. Roux removed the breast, which he found to be entirely possessed by a series of hydatids.

Last year, a man was admitted into La Charité, who had, at the posterior part of the shoulder, a fluctuating tumour, which was believed to be a chronic encysted abscess. It was opened, and a transparent straw-coloured fluid escaped. Severe symptoms followed this slight operation, and the man died. On opening the body, an enormous hydatid was discovered, situated partly in the infra scapular fossa, and partly in the infra spinous fossa.

When M. Roux was surgeon to the Hospital Beaujon, nearly 18 years ago, a young girl was admitted into the physician's ward, on account of a large tumour which was felt deeply situated in the right hypochondrium, under the edges of the ribs. This tumour was evidently situated in the liver, but its nature was not clear. The girl was in excellent health, and it did not prevent her from taking exercise and pursuing her occupations. One day, in consequence of some exertion, she suddenly felt an acute pain; the tumour disappeared, but the lower part of the abdomen became tumefied, and fluctuation was very perceptible at that point. An incision was made at the lower part of the linea alba, which gave vent to a transparent straw-coloured fluid, in which a great number of hydatids were floating. The patient died soon afterwards. On opening the body, a great number of hydatids were found in the cavity of the abdomen, and in the liver there was an enormous cyst, which had been ruptured, and the effusion from which into the abdominal cavity had caused the patient's death.

From the Journal de Physiologie, &c.

**LA VUE PEUT-ELLE ETRE CONSERVEE MALGRE LA DESTRUCTION DES NERFS OPTIQUES?** Par M. MAGENDIE, Membre de l'Institut.

We have already related in this journal, experiments which prove incontestably, that the fifth pair of nerves is the principal organ of the general sensibility of the head; that it is very probably the agent of smell; that it is certainly that of taste; and, what is more extraordinary, that it maintains in dependence the senses of sight and hearing, in imparting to them through some unknown influence, their peculiar sensibility. Thus, when the fifth pair is divided on both sides, sight is destroyed, notwithstanding the iris is still sensible when exposed to the direct rays of the sun. But although analogy might lead to such a supposition, we had never ventured to suspect that the fifth pair could supply, even

temporarily, the action of the nervous apparatus of the eye, that is to say, the optic nerve. The following pathological fact will justify us in regarding the thing, if not proved, at least as possible; and at all events, will remove from the conjecture, any appearance which it might have of a paradoxical character.

We shall proceed then, to relate this interesting case, communicated by Dr. Sanson, surgeon of the Hôtel Dieu of Paris, as drawn up by M. Corbin, élève interne, who has confirmed to me, viva voce, all the details therein contained. It is greatly to be lamented, that this piece of pathological anatomy was not carefully preserved; every one might then have assured himself of a fact, thus far, I believe, unique in the history of the science.

*Hôtel Dieu de Paris, Salle Saint Bernard.*

Bardon, a writer, unmarried, aged thirty-six years, an inhabitant of Orleans, entered the hospital September 8th, 1827, and died on the 21st November.

*Amaurosis—cyst, in part osseous, situated posteriorly to the commissure of the optic nerves.*

At the period of his admission, the pupils were greatly dilated, that on the right side slightly mobile, the left altogether motionless, and vision in the left eye entirely lost; with the right, he can still distinguish objects, though with difficulty, and without a correct appreciation of their size, form, or colour.

A year and a half has elapsed, since the patient, habitually employed in writing, has been compelled to relinquish his avocation. During the last eight years he has been subject to violent headaches.

Countenance pale; lymphatic temperament. Prescription—*venesection, seton to the neck*,—slight melioration. Blisters were subsequently applied upon the forehead and temples, and the improvement progressively advanced to such a degree, that at the expiration of three weeks, the patient, from the bridge of the Hôtel Dieu, could see distinctly the passengers walking upon the *Petit Pont*.

The same measures were continued, less actively however, and the patient remained in the same condition till the 13th November. On the day just mentioned, he complained of cephalalgia, and acute pains in the eyes and ears, which appeared to converge towards the middle of the head.

On the 15th, the cephalalgia and pains continuing, fifteen leeches were applied behind the ears, which removed the headach, but neither this, nor any other measure that could be devised, had any effect in mitigating the pains.

On the 21st, at the usual hour of visit, two or three minutes after having spoken with me, he died suddenly, without a groan, or any change in his position.

*Autopsis.*—In the space between the junction of the optic nerves and the pons varolii, within the vessels which form the arterial circle, a cyst was found about the size of a

small hen's egg, with its parietes partly fibrous, and partly osseous, particularly on its anterior and superior portion, where it corresponded with the junction of the optic nerves. The cyst was filled with a yellowish matter mixed with blood, nearly one-third of which was solid and of a tuberculous appearance, while the remainder was liquid and oleaginous.

Laterally and superiorly the cyst corresponded with the optic nerves, which were flattened and almost destroyed. That which remained of these nerves, adhered on the inner side to the cyst, by means of the remains of altered cerebral substance, and terminated anteriorly, in a white point, upon the osseous portion corresponding with the commissure. Farther onwards, the nerves were seen wasted, and pursuing their course towards the orbit, but between this anterior and the posterior portion, there was no other continuity than that formed by the osseous parietes of the cyst. In the eye, the retina was observed thin, of a reddish colour, and almost transparent. Nothing remarkable was observed in any other part of the brain.

There was no trace of the pituitary gland, its situation being entirely occupied by the cyst.—Sanson, chirurgien; Eus. Corbin, interne.

There are two facts related in the preceding case, so as to leave no doubt of their reality. *The patient saw objects distinctly a few days before his death*, and there was a complete interruption of the optic nerves, in their passage from the eye to the brain. In fact, it is stated, that *there was no other continuity between that portion of the optic nerves situated anteriorly to their commissure, and that situated posteriorly, than what was formed by the osseous parietes of the cyst*. Add to this, the atrophy of the optic nerves, and the thin and transparent condition of the retina, circumstances which are only observed in chronic derangements of vision; moreover, we often see persons who have been blind for a number of years, and in whom the optic nerves have undergone no diminution of volume, or any perceptible alteration in their central medullary portion. The same thing is observed in mammiferous animals, who have accidentally become blind, or lost an eye, and in whom no appreciable alteration of the nerves can generally be detected.\* Now, admitting the truth of the circumstances related, it is not possible in the case above mentioned, to refer the existence of vision to the optic nerves, since they were evidently incapable of exercising their functions, and we must therefore have recourse to the fifth pair, in order to conceive how the impression of the

light could be communicated to the brain. I am aware, that this conjecture will appear very singular to the majority of readers, accustomed to consider the optic nerve as equally essential to vision, as the heart to the circulation; but if it lead to researches which may overthrow or confirm it, it will, in either case, not have been uselessly related, the rather, that it is only in the latter view, that I attach any importance to it. To limit oneself to conjectures, is to do nothing; while to endeavour to verify them by means of observation and experiment, is to labour advantageously for the advancement of science.

I ought to observe, however, that my experiments on the optic nerves,\* are not favourable to this supposition; for if one of these be divided anteriorly to its decussation, vision is destroyed in the eye of the injured side; but if it be divided posteriorly to this union, then the eye of the opposite side loses its action; while if the decussation itself be divided from before backwards, in the median line, the animal remains entirely blind. It follows, therefore, from these experiments, that the optic nerve is indispensable to vision; but here the lesion is sudden, while in disease, it takes place gradually and slowly. Is this difference in the *modus operandi* of the cause, sufficient to account for the extreme difference in the effect? Before pronouncing a decided opinion, let us await farther information, and suffer no opportunity of acquiring it, to pass unimproved away.

I will subjoin, to what has been already said, the note of a case, taken in my hospital, and which has been preserved only by reason of its singularity.

Hôpital de la Salpêtrière, 4th July, 1827. The body of a woman, seventy years of age, was brought to the amphitheatre; she had died of an affection unconnected with the nervous system.

She had long been blind of the right eye, the crystalline of which was opaque; vision in the left had been preserved.

The optic nerve of the blind side, was of its ordinary volume; that of the other, was much smaller, attenuated, and wasted. The atrophy was not perceptible beyond its decussation.

This alteration was so evident, that the physicians present at the dissection, when they had seen the nerve, supposed the blindness to have existed on the left side.—Guillot, élève interne.

I much regret not to have myself witnessed this circumstance, and also, that farther information was not obtained before the death of the subject; but such as it is, it will, I hope, induce physicians who have occasion to examine the bodies of individuals, partially or wholly blind, to give particular attention, not only to the condition of the optic nerves, but also to that of the fifth pair. The most guarded deduction that we can make, from what has been

\* In birds, the loss of sight is followed, in less than fifteen days, by atrophy of the optic nerve and tubercle, with the disappearance, more or less completely, of the medullary matter of the nerve, of the pearly envelope of the tubercles, and of a part of the proper substance of these eminences.

already stated, is, that we have still much to learn respecting the functions of the nervous system.\*

From the London Medical Gazette.

**CASE OF PARALYSIS OF THE LOWER EXTREMITIES, COMING ON GRADUALLY—Fatal.** [La Charité.]

A hackney-coachman, between 35 and 40 years of age, above the middle stature, and enjoying good health at the time, when in a state of perspiration, walked with naked feet on a marble floor; the perspiration instantly ceased, and was followed by cold chills, and a sense of uneasiness during the whole day. The following day he complained of sore throat, which was relieved by sudorific drinks, but lassitude, want of appetite, and some fever remained, though without headach. Afterwards the lower limbs swelled, the patient experienced in them a feeling of formication, and soon afterwards they refused to sustain the body. The toes presently became the seat of a sensation of coldness, which was propagated to the legs, thighs, buttocks, and at length to the lower part of the loins. A physician was called, who, looking upon this as a rheumatic attack, applied leeches to the insteps, and frictions, with a sedative balsam. The patient was admitted into La Charité on the 1st January, twenty days after the suppression of the perspiration. He was then in the following condition: the features expressing great suffering; the lower limbs in a state of complete paralysis; he complains of pricking pains, especially in his feet; they are perfectly helpless, and fall down like dead masses when lifted up by the hand, but they preserve their sensibility. Within the last two days only, the upper extremities have begun to take on the same disease as the lower ones; at first the fingers, then the fore-arms, lost the power of motion, but the paralysis is not complete; the patient is able to extend and bend them in a very limited degree; the arm itself is moved with rather more facility; nevertheless the muscular contractions are very feeble, for he is not able to keep it in an elevated position—it falls in the same manner as do the lower limbs: otherwise, sensibility is not destroyed in any part of the body; the pulse is frequent; the face and neck perspire; the paralysed parts are dry; the respiration is per-

formed by the diaphragm; the patient is oppressed; his voice altered; he tries from time to time to make long inspirations, for the chest can scarcely be said to dilate. There is constipation, scanty urine, but no headach. The intellectual faculties are perfect. *Four bleedings from the arm, blisters to the legs each day, a potion with gum and three ounces of castor oil, strict diet and emollient drinks*, were the means employed between the day of his admission and the 7th of January. Up to this last day the patient had not had an evacuation; the oppression not only still continued, but was increased by a great quantity of mucus which choked up the bronchi, and which he could not get rid of without great exertion, the efforts of the cough were so feeble. The pulse was 110, and weak; the perspirations continued and were very copious, especially during the night. *Jalap with sirup of poppies, barley water with honey, blister between the shoulders.* Three copious stools were procured by a purgative clyster, and an ounce of castor oil.

It is needless for us to follow up the daily reports further: the patient gradually sunk, and died on the 21st.

*Examination.*—The dura mater covering the brain was healthy, and there was but little serum under the arachnoid membrane, which presented some opaque patches on different points of the convexity of the hemispheres of the brain. The pia mater was rather vascular, uniformly red on the posterior lobe of the right side; there was a soft gelatinous exudation in the cellular tissue which unites this membrane to the cerebral face of the arachnoid, on the convexity of the anterior lobe of the left side; the convolutions of the brain were very unequal, some small and hidden, as it were, by the larger, which rose several lines above their level; the ventricles contained very little serum; the substance of the brain, cerebellum, and tuberculum annulare, presented no remarkable deviation from health, either in consistence or colour. The spine, opened the whole of its length, presented nothing particularly deserving of notice outside the dura mater; the arachnoid appeared healthy throughout its whole extent; in the cervical region, it contained one or two spoonful of turbid serum, but it was not possible to ascertain whether this had been originally thrown out in the cavity of this membrane or not. It is probable that it might have proceeded from a large opening that had been made in the internal layer of the arachnoid, in the endeavour to lay open the spine, and that it had been exhaled in the canal discovered by M. Magendie. The vessels of the pia mater were a little injected, especially towards the lumbar region. The spinal marrow, of its natural dimensions, presented no traces of compression externally, nor any alteration of colour; divided throughout its whole length, it appeared to possess its natural consistence in the cervical portion, but lower down it was evidently softened. This softening, but slightly marked at the upper part, became more so

\* In a note appended to an abstract of the preceding paper, the editor of the Lancet observes, "We do not see how either of the above cases goes to prove, that the power of vision is not dependent upon the optic nerves; in the first case, the optic nerves were not completely destroyed; and in the second, although vision was more perfect on the side where the optic nerve was affected, than on the other where it was not, still there was a very apparent cause in the right eye, perfectly independent of the nerve, viz. opacity of the lens."

towards the lower extremity; it was greater in the centre than at the circumference, but it was not more red, or vascular, than in the healthy state.

The organic alteration produced by diseases of the nervous system are not always appreciable; and even when they are so, they are sometimes so little marked as to be easily overlooked, or mistaken. In these organs, the smallest alteration may produce the most fatal effects; and if the examination be not made very minutely, these alterations are not discovered, and then we have recourse to the sounding terms of *vital lesion*, or *functional lesion*, to explain the morbid phenomena. Such was the state of the case just recorded. The spinal marrow, and its membranes, were in a healthy state; it was of its natural size and colour; when divided throughout its whole length, it presented neither infiltration nor injection of blood; it was declared to be perfectly sound—when a more attentive examination proved that its consistence was not alike in the whole of its extent.

It is evident in the above case, that the softening of the dorsal and lumbar portions of the spinal marrow was the principal cause of the morbid phenomena, and of the patient's death; and that the appearances found in the brain were neither the primary nor most important disease. The progress of the paralysis indicates the course of the affection of the spinal marrow; for some time neither the functions of digestion or respiration were troubled; there was but little if any fever; the patient felt merely lassitude and weakness in the lower limbs, but still was enabled to follow his usual avocations. These symptoms showed that the disease was then confined to the lower part of the spine. Afterwards, loss of appetite and constipation came on; at the same time, respiration became difficult, the contractions of the heart became more frequent, and, lastly, the upper extremities became affected in the same manner as the lower limbs. What could be the cause of these disturbed functions, if not the change in the spinal marrow propagating itself by the continuity from the lower to the upper part?

If we may judge by the anatomical appearances, the inflammation can never have been violent; it may be doubted whether it existed at all, since M. Recamier considers these softening as of a peculiar nature, independent of inflammation. The sensibility was preserved entire, and yet it did not appear, upon examination, that the posterior cords of the spinal marrow were less changed than the anterior. This strengthens the mode of explanation given by M. Lallemand of the experiments of M. Magendie, as to this point of physiology.

The most important phenomenon of this disease was the disturbance in respiration. The palsy of the intercostal muscles, by preventing the free dilatation of the chest, by degrees impeded the change in the blood, and brought on asphyxia; this was also hastened by the quantity of mucus accumulated in the bronchi, owing to a defect in the powers of

expulsion. Thus the patient was a good deal relieved when, by proper methods, the respiration was rendered more easy.

From the Bulletin des Sciences Medicales.

CONGESTION SANGUINE DE TOUTE LA SUBSTANCE CEREBRALE, ET SQUIRRHE DES LOBULES ANTERIEURS DU CERVEAU. Par M. le docteur V. RAMBELLI.

Count Annoni, æt. 55, of a sanguineous temperament, had from infancy been subject to epileptic attacks; he had moreover, at certain periods, palpitations of the heart, which induced the fear of the existence of serious lesion of that viscus. Having made use of the sea bath for the cure of a dartsous affection, with which he had been long troubled, he began soon after, during the month of October 1824, to complain of pain in the head, which returned particularly after meals, and was accompanied with somnolency. A blister was directed to the neck, and frictions with tartar emetic ointment upon the parts which had been the seat of the dartsous affection. This treatment proved totally ineffectual, and the disease continued to advance. On the 12th of February, 1825, he fell down in a state of syncope, which continued about half an hour, when he revived, addressed some questions to the assistants, and slept for the space of forty or fifty minutes. Dr. Dell'acqua being called, supposed the patient labouring under debility of the nervous system, for which he directed the sulphate of quinine, a good diet, and a small quantity of wine. No beneficial consequences followed this treatment, and Count Annoni, who, for an indeterminate period, had been insensible to the action of the sun, (insensible à l'insolation,) and in whom the act of sneezing had, for several months past, occasioned an acute pain at the root of the nose, soon presented the following symptoms; pulse full, elevated, and strong; digestion very regular and energetic; face ruddy; continual pains of the head, accompanied with a sensation of heaviness in the frontal region; somnolency.

March 10, immediately after eating, he suddenly lost the use of his senses, the mouth filled with foam, the extremities were convulsed, stupor, and universal sweats supervened, and this state was prolonged more than twelve hours. A blister was applied to the back of the neck, and an electuary composed of cinchona, serpentaria virginiana, arnica, and sirup of orange bark, administered internally. The drowsiness continued notwithstanding, and was almost constant; the face was bloated; the eyes red and scintillating. A pain extended from the left side of the occipital region to the forehead, when the sensation of weight was insupportable; the patient appeared lost in a kind of stupidity; generally he was unable to answer the questions put to him, and when he did so, it was

by a simple smile; the hands trembled; the influence of the will was considerably diminished; the abdominal muscles were affected with frequent cramps, and notwithstanding, there was no derangement of the digestive functions.

In the beginning of June, six leeches were applied to the temples, the internal remedies already prescribed were continued, and the patient went into the country, whence he returned, after the lapse of a month, in a state bordering upon mental alienation. On the 5th, 6th, and 9th of July, leeches were applied successively to the epigastrium, temples, and anus. The last application was followed by syncope, and in the evening there was profound stupor; loss of sensibility; respiration stertorous, short, and weak; pulse obscure, small, trembling, and unequal. A piece of caustic potash was applied on the left of the nucha, and sinapisms to the feet. On the 10th the stupor disappeared, and the patient appeared considerably better, but memory and judgment were totally destroyed; the hands moved in an automatic manner, and the feet were incessantly agitated. Reiterated bleedings, both generally and locally, pills of calomel and scammony, blisters to the legs, thighs, and head, a mixture of the succinate of ammonia, sulphate of quinine, and the infusion of arnica, were successively directed; and eventually, after alternations of improvement and relapse, the patient died on the 29th of July.

On dissection, all the vessels of the encephalon were found gorged with blood, the cortical part was more indurated than natural, and the medullary portion presented, when divided, a great number of bloody points; the lateral ventricles, particularly the left, were filled with serum; the fourth ventricle contained a tumour of a peculiar character, of the size of a hen's egg, and very firm, which was divided into four bundles, varying in size, and irregularly knotty, having a brownish aspect, and extending towards the anterior part of the cerebral hemispheres; the two anterior lobes, that of the right side especially, were in great part converted into a very hard substance, which crepitated under the scalpel. The cortical substance which covered the tumour anteriorly, and that situated beneath it, were entirely disorganized; but the posterior superior third of the lobes, had acquired the density which was observed in the other parts of the brain, and the anterior and inferior portion of the same lobes, presented a similar disposition to the depth of several lines. The small extremity of the tumour touched the coronal suture, while the great was continuous with the bundles, which were surrounded by a medullary substance, converted to a certain depth, into a soft, translucent matter, analogous to animal gelatine. In the centre of the tumour, was a cavity about the size of a nut, the parietes of which were softened, and had an orange yellow colour; it contained a small quantity of a yellowish fluid, and presented some hard membranous bridles.

The trachea had a deep red colour; the lungs, though in other respects sound, were engorged with blood; the bronchii obstructed by a bloody foam; the two auricles contained very hard coagula, resembling in colour the buffy coat of inflammation; evident traces of inflammation were observed on the internal membrane of the aorta; the abdominal viscera were sound.

From the Edinburgh Medical and Surgical Journal.

**CASE OF APOPLEXY FROM PRESSURE OF THE INTERNAL JUGULAR VEIN,**  
*applied to check Hemorrhage from a wound of that Vein.* By C. HEINEKEN, M. D., Madeira.

*To the Editors of the Edinburgh Medical and Surgical Journal.*

GENTLEMEN—As I have never met, either in practice or any popular work on surgery, (with the more recondite I confess myself but little familiar,) with a case similar to the following, I am induced to solicit its publication, not because it belongs to that, generally speaking, most useless of all classes, *the rare*, but because its result points out a very different mode of treatment from that which nine persons in ten would adopt upon meeting with it for the first time, and the adoption of which must, I conceive, inevitably prove fatal, as it did in this instance. I did not see the man while living, nor was I present at the *post mortem* examination; but I give the case nearly in the words, and at the express desire of the gentleman under whose care it occurred, and have no doubt about the accuracy of the details. I am, &c. C. HEINEKEN.

*Funchal, Madeira, 25th March, 1828.*

Joan Fernandez, a soldier in the artillery, was stabbed between the condyle of the lower jaw and the mastoid process, just below the lobe of the ear on the left side. The knife with which the wound was inflicted was about two inches and a half in length, about half an inch in breadth, with a slight curve at its point; and it had been plunged up to the handle. Profuse bleeding ensued, which one of his comrades stopped by pressing with his finger upon the wound, after a large quantity of blood had been lost; and he was removed to the Portuguese hospital. Upon examination the temporal artery was found to be wounded, and with considerable difficulty was secured by ligature. Upon the removal of pressure the hemorrhage returned almost as violently as ever, and in a constant full stream of the size of a goose quill. As the man was greatly exhausted by the large quantity of blood lost, the first object was to suppress the hemorrhage as speedily as possible; and with this intent the wound was filled with pieces of sponge, and compresses were applied over them. In two hours from the suppression of the hemorrhage he became comatose, with paralysis of the right arm and leg, and a full pulse. Twelve ounces of blood were taken from the arm,

an active cathartic given, and in the evening, as the symptoms remained the same, other twelve ounces were abstracted. On the following day the same plan of treatment was persevered in, and about every eighth hour more or less blood was withdrawn, but without giving any decided relief. On the seventh day he expired apoplectic.

Upon examination, a wound more than half an inch in length was found in the internal jugular vein immediately below the base of the skull, and, with the exception of the temporal artery before-mentioned, no other vessel, nerve, or part of consequence was injured. There was no opportunity of opening the skull.

It is, I think, very evident that the means had recourse to in this instance were the immediate cause of the man's death. That the pressure, by being carried to the extent of rendering impervious the canal of the vein immediately upon its quitting the skull, had produced apoplexy, which frequent and continued bleedings failed to relieve; and that, as it was absolutely necessary, by some means or other, to put a stop to so formidable a hemorrhage, the only chance of preventing the accident from proving fatal consisted in permanently reducing the supply of blood to the brain. This might have been attempted by tying the carotid artery on the same side, and, had evidence of pressure on the brain still continued, by putting a ligature around the other carotid also. In that case the operation alone would probably have had a fatal effect; but it would have been the only hope; and if, under any circumstances, a man can survive the obliteration of both these vessels, that appears to be one of the most favourable, in which, as in the present instance, the escape of blood from the brain had been prevented by the closure of so direct and important an exit. In speaking thus I would have it understood, that I do so after knowing the result,—that, had the case occurred to me for the first time, I should in all probability have treated it in the manner actually practised, and brought it to a similar termination; but there are not wanting those who may have both opportunity and skill to put to the test of practice what I have suggested as merely plausible in theory.

From the Medico-Chirurgical Review.

**FEVER.—DICTIONNAIRE DE MEDECINE; ARTICLES, FIEVRE—GASTRO-ENTERITE. MM. COUTANCEAU et RAYER.**

It is, we think, much to be lamented, that the noble example set by our Gallic brethren, in the publication of two successive voluminous dictionaries of medicine and its collateral sciences, should not have been followed by any similar undertaking on this side of the Channel. In a department of knowledge, as yet so essentially progressive, so mutable in its doctrines and in the manner of their application to practice, there is nothing which, in our opinion, can so effectually contribute to improvement,

as short, concentrated monographs on particular diseases, frequently given to the public, in a collected and alphabetic form, as in the work from which we select the present articles.

The usual obstacles to publication on select and favourite topics—the demand, on the part of the bookseller, for a certain quantity of matter—the apprehension, on the part of the author, that he may not be able to distend his subject to the necessary bulk, without either hiding, or disfiguring its best features, are all removed by the plan of collective labour in a dictionary, such as that now before us. Here the collaborateur furnishes a dense, perspicuous treatise, containing an epitome of every thing that has been done by others on the subject, up to the hour in which he writes, together with his own opinions, and the results of his observation and experience. The name of the writer being affixed to each article, makes him responsible to the public for his own production, and enforces, by the strongest ties we have upon mankind, their interest and fame, the necessity of making his Essay the best that he can furnish.

There is an amiable, and we conceive, an enviable peculiarity, in many of the French works of science. We often find some of their best productions under a double signature. Two friends, emulous in the pursuit of knowledge, agree to make their researches together; they assist and stimulate each other, and the result of their joint labour, is generally something superior to what either of them could have singly effected. We can boast of but few examples of this kind of literary partnership, particularly in our medical writings.

The distinguished authors of the Essays now before us, are already too well, and too favourably known to the public, to require any bibliographical notice of their works from us. M. Rayer more especially, the joint author of the first (*Fièvre*) and the sole author of the second article (*Gastro-Enterite*) at the head of this review, has given us, in his book on the Diseases of the Skin, a sufficient guarantee, as to the strength of his mind and the accuracy of his conceptions.

Although the French medical writers are generally remarkable for the precision and method with which they treat their subjects, we cannot, we fear, hold up the article *fièvre*, as a very marked example of lucid arrangement of parts. It possesses, however, many excellencies to compensate for this deficiency. The following appear to us to be the chief objects of the first monograph.

1st. Our authors consider the word *fièvre*, with its Greek and Latin synonyms, as a name used to represent one or more morbid phenomena.

2dly, They enter into “general considerations,” on the subject of fever, and under this head they pass in review the opinions, as to its nature and cause, of the most celebrated pyretologists, ancient as well as modern, chiefly with a view to decide this important question, viz. whether the fevers of authors,

but more particularly those of M. Pinel, ought to be considered as essential, idiopathic affections of the system, or merely as "physiological expressions of some local disease?"

3dly, They give the conclusions to which their own researches and observation have led them with regard to this question, and for the pathology and treatment of the inflammations causing, or constituting, (in their opinions) all fevers, they refer to the distinct heads, *Enterite, Gastro-enterite, Meningite, &c.*

In endeavouring to open any thing like a clear path, through the dense cloud of theories that has hitherto hung over the various doctrines of fever, like mist over the Valley of Mirza, we shall take our authors as our guides, reserving to ourselves, however, the privilege of making our own arrangements for the undertaking.

1. "The word *fièvre, fever, febris*, from *fervere, to boil*—*πυρετος* from *πυρ* fire, is not, say our authors, a substantive, of which the singular is not more distinct than the plural, as some have ironically observed; it is a substantive, the plural and singular of which have been used successively in different acceptations. It is an expression which has been employed by turns to represent augmentation of animal heat—increase of heat with acceleration of the contractions of the heart—the latter phenomenon without increase of heat—a general disturbance of the functions without topical lesion—a salutary effort of nature to cure disease, or to concoct the crude humours infecting the mass of animal fluids—a primary, and general modification of the economy, sometimes producing inflammations. An acceleration of the course of the blood, by means of a quickening of the contractions of the heart, with increase of calorification, and disturbance of the principal functions, &c."

"Fever, according to Galen,\* are diseases that come on without inflammation, without abscess, without local pain, without erysipelas, without a special lesion of any part. 'If,' says this great physician, 'an inflammation of the side, or of the lungs, produce fever, these diseases do not take the name of fever; they are called peripneumony, affection of the spleen, &c.' Celsus remarks, 'that two or three symptoms are not enough to characterize a morbid state. Heat, and increased frequency of pulsation in the veins are certainly the two principal characters of fever, but alone they do not constitute it, because many operations of the economy, such as fear, anger, &c. are capable of producing them.'"

M. Pinel differs but little from Galen. The word fever with him, implies increased heat, hurried circulation, disturbed functions, and the absence of local lesion. The definition given by Selle is still more vague.

"Fever is a disease variable in its course and its duration. There is cold, and heat, with the pulse sometimes more, sometimes less

quick than natural. Cullen evidently follows Galen, when he defines fever as pyrexia without any primary local disease."

Thus we see, that by each succeeding definition of the word fever, the basis of a theory as to its nature and cause, nay even as to its treatment, is at once traced out.

Such being the vague and unsettled state of the notions connected with this word, we have deemed it necessary to declare, once for all, that when we may hereafter use the word fever, we would be understood to mean that state, in which we find heat of skin, quickened pulse, and deranged secretions, without any reference whatever to the causes of these phenomena.

## 2. GENERAL CONSIDERATIONS.

There is no great difficulty in finding out to what sect our authors belong, from their readings of the great koran of physic, the works of Hippocrates. The learned Hakeem, in these, as in the book of his prophet, can find shadowed out the rudiments of all that man has ever yet discovered, as to human diseases and their cure, provided that he knows how to interpret the sacred text. In fact, there has been no view hitherto taken, either in pathology or therapeutics, of which we may not detect some traces, however obscure they may be, in the writings, authentic or apocryphal, attributed to the Father of Medicine. Thus the doctrines of the very latest and most popular living pyretologists, as to the local origin of fever, find considerable support in the recorded opinions of the Coan Sage. Take the following as an example.

"In angina, there is *fever*, pain in the head, swelling of the jaws."\*

"In wounds of the brain, *fever* and bilious vomitings are the necessary consequences."†

Hence it is argued by our authors, that, as in the times of Hippocrates, the knowledge of pathological anatomy and physiology was so very limited, and as there were so few opportunities of connecting the symptoms during life, with the state of the organs after death, he was induced to attach to *fever*, as the chief morbid phenomenon of every acute disease, all the other symptoms that he had observed, and thus to make it a generic term, to designate all diseases accompanied by morbid heat, but to which he could not assign a local origin, as in the cases of angina and injuries of the brain, just quoted. For our own parts, we think that there is abundant evidence to show that the word *Πυρετος* was used by Hippocrates, on some occasions, to designate the febrile symptoms attending local inflammations which were palpable and visible to all, on other occasions, to designate the same symptoms more or less aggravated or complicated, but unaccompanied by any topical affection then cognizable. The leading groups of these symptoms, he naturally and properly divided into

\* Edit. Chart. tom. ix. Aph. Hipp. Comment. pag. 184.

\* Hipp. de Morbis, lib. ii.

† Ibid. lib. v.

different types, distinguished by the modes and succession of their attacks; such as continued, intense fever—intermittent fever returning every day, every third day, &c. We think that our authors have established, in a most satisfactory manner, that the different epithets applied by Hippocrates to particular fevers, were only descriptive of symptoms accompanying that which, with him, constituted the principal phenomenon, namely, heat of skin, and not intended to designate distinct species of the same disease. Thus, fever, with the epithet *phricodes*, means febrile heat, preceded by shivering; with the epithet *lingodes*, morbid heat accompanied by hiccup; and so of the others.

We shall now endeavour to arrange under distinct heads, in a more concise manner than our authors have done, the different opinions that have hitherto been promulgated, as to the nature and cause of fever. We shall, of course, notice only the leading theories, those that appear to have given, during their respective reigns, a marked character and direction to the treatment of disease. It would be manifestly impossible to enumerate, in the space allotted to a review, all the minor opinions which physicians formed for themselves, from time to time, made up of parts selected from the greater systems, and combined according to their own views. The principal sects of pyretologists, whose theories as to the nature and proximate cause of fever, have swayed the science of medicine from its earliest records up to the present day, might, we think, be arranged under the following heads, viz. The Humorists—The Animists—The Solidists—The Essentialists—The Localists.

#### THE HUMORISTS.

The earliest, beyond all question, the most universally received, the most permanent in its duration, and we might, perhaps, add, the most practically useful in its application to the treatment of disease, is that theory which was born with the healing art itself, was methodized by Galen, and is acted on by many up to the present day, we mean *the humoral doctrine*. The founder of the humoral, and, as it would seem, of many other doctrines also, was Hippocrates. He came to the conclusion, that all fevers are produced by a vitiation of the fluids. The vagueness and want of precision which we find in his writings on this subject, and his total silence as to the state of the pulse, are due to his entire ignorance of the mechanism of the circulation. He saw clearly, however, that the secreted fluids became depraved, some more palpably so than others, as soon as the fever was established. Hence his notions as to the agency of the atrabilis, &c. In short, with Hippocrates, health consisted in the existence of a perfect equilibrium in the material composition of the humours; disease in the destruction of this equilibrium; the healing art, in correcting the predominant morbid qualities of these humours.

Galen attempted to improve on these common-sense and simple notions, by attributing

to each individual fluid the capability of producing a particular class of diseases; but he only succeeded in complicating them into something bordering on absurdity. The natural consequence of a theory, which supposes a separate morbid agency in each depraved fluid, was, the suggestion of distinct remedies, or purifiers, to be used as each humour became more prominently peccant, or as a particular humoral idiosyncrasy happened to prevail. Hence the almost countless multiplicity of Galenic medicines. Each fever had its distinct origin. Thus, quotidian fever was produced by the putrid puita; tertian by the yellow, and quartan by the black bile.

These doctrines, after they had been lost in the dark ages, were again brought up by the Arabian physicians, but so mistified and complicated, as, for a time, to wear the appearance of novelty. They continued, however, to regulate the curative administrations of medicine, with the most uncontrolled sway, for many centuries.

#### THE ANIMISTS.

The first important schism from Galen's creed, was that of Paracelsus and Vonhelmont, the Mahomet and the Ali of physic. The former of these enthroned a demon, which he called Archæus, in the cardiac portion of the stomach, and, under his orders, he placed myriads of inferior Archæi, in the different organs of the body. All the functions were regulated by these, armed with their respective *ferments*. All diseases, and consequently fevers, were produced by the rebellious and improper use made by the minor Archæi of their respective ferments, and by the efforts of the great Archæus to bring them into subjection and order. Vonhelmont substituted, in his theory, the rational soul for all the Archæi, great and small. According to these theories, as each disease was looked upon as a battle, fought upon a regular plan, between the Archæus and the ferments, or between the rational soul and the morbid matter, the physician had little more to do than to look on. To these extravagant notions we owe that famous work by Stahl, "*Ars Sanandi cum expectatione*." Hence the *médecine expectante* of the Essentialists of the late French school.

Although the archæus and the rational soul, (the *enormon* of Hippocrates and Plato revived) soon fell into disrepute, the notion, that fever is the result of a salutary effort of Nature to expel a ferment from the blood, was entertained by the great Sydenham himself, and is obscurely seen in the "*vis medicatrix*" of Cullen.

The medical theories of the Alchemists and their followers consisted in the belief of certain rude chemical actions going on amongst the fluids, and directed their efforts to the discovery of alexipharmacs, panaceas, and elixirs of longevity, to be substituted for the cumbersome farrago of Galenic simples.

The Mechanists, at the head of whom we may place Boerhaave, rejected with disdain the humoral doctrines, as far as the influence

of the recrementitious fluids was concerned. They retained, however, all that was good in the views and practice of Hippocrates, and pretended to direct their own upon principles purely mechanical and mathematical. The volume and form of the globules of the circulating fluids, as compared with the capacity of the vessels containing them, formed the basis of their theories as to the cause of disease.

#### THE SOLIDISTS.

Cullen may be placed at the head of these, for Hoffman was but half a Solidist. This school attributes all fevers to a spasm, or morbid contraction of the extreme capillaries. They moreover confine the term to acute diseases, unaccompanied by any primary local affection. The doubts which this theory involves, as to whether tone ought, in certain cases, to be given or taken away, naturally led to a vaccillating practice, producing little or no influence on the progress, or result of the disease. But a still more mischievous effect was produced by this doctrine. It arrested the progress of medical improvement by an artificial classification of diseases, entirely unsupported either by clinical observation, or pathological anatomy. John Hunter and his followers, when they use their favourite language, "the action of the vessels of a part," only mix up the solid parts of this theory with the immaterial vitalism of Stahl and Vonhelmont.

#### THE ESSENTIALISTS,

Or, more properly, the Ontologists of Broussais, having Pinel for their chief, finding that they could not maintain the independence of all fevers upon local inflammation, limited the number to five, which they called essential, viz. inflammatory, bilious, mucous, adynamic, and ataxic fevers. They held that the entity, or being, of which they appear to know nothing, but which they call fever, produces, by a mechanism which they do not pretend to describe, all the symptoms and organic lesions which we witness during the progress of these diseases, or find after the death of those that perish by them. This, in fact, goes to assert that fever is fever, that it will have its fling in spite of all we can do, and leads once more to the practice of the *médecine expectante* and harmless ptisans. The bitter enmity, which our authors every where manifest towards the theories of M. Pinel, renders the following homage to his candour and integrity doubly valuable:—"A faithful interpreter of facts, even when in opposition to his theoretical principles, he attributes the ataxic fever to a morbid state of the brain. This was the first step in that road, which was afterwards destined to lead to a more important reform."

#### THE LOCALISTS.

The gradual improvement and diffusion of anatomical knowledge, the more accurate and more frequent observation of post-mortem appearances, in fact, the general spread of

science, had been long preparing the basis of a much more important theory of fever than any that had been hitherto substituted for the doctrines of Hippocrates and Galen. The Localists, or those who hold that all fevers are caused by local disease, owe the materials of their theoretical edifice to the dissections made by Bertholin, Bonetus, Morgagni, Baglivi, Chirac, Prost, the Hunters, Bichat, and a host of others. The traces of inflammation or congestion, almost always found about the encephalon and abdominal viscera of those that perish from fever, very early induced different pathological anatomists to consider one or other of the varieties of these local affections as the proximate cause of fever. Chirac conceived that fever is owing to inflammation of the brain. Baglivi says, that the fevers called malignant are produced by phlegmon, or erysipelas of the intestines. M. Broussais, however, appears to have been the first who digested the facts collected up to this day, as to local irritations and inflammations, particularly those of the stomach and bowels, into a regular code of doctrinal pathology. Our authors, zealous supporters of his doctrine in all its ramifications, thus introduce this celebrated pyretologist to our notice.

"Penetrated by the sublime views of Bichat as to the sympathies, rich in numerous facts observed with a rare sagacity, M. Broussais came to overturn, from the very foundation, the antique edifice of fevers. In his works, as well as in his lectures, he has applied himself, for many years, to demonstrate, that the fevers which had been called essential, were nothing more than local diseases, inflammations, nay, even *gastro-enterites*. The following propositions (say our authors) may be looked upon as the summary expression of his doctrines.

"1mo. Fever, considered in a general and abstract manner, is never other than the result of a primitive or sympathetic irritation of the heart, through the effect of which this viscus hurries its contractions.

"2do. Every irritation, sufficiently intense to produce fever, is an inflammation.

"3tio. All the fevers of authors are connected with gastro-enteritis, simple or complicated. This they have all overlooked when unaccompanied by local pain, and even when accompanied by pain, they consider it as an accident. Authors have sometimes said, that certain fevers depended on inflammation of the digestive organs, but they have never said that these fevers, the pretended essential fevers, cannot have any other cause; never that they were produced by the same mechanism as the fever of peripneumony; never, in short, that there are no essential fevers.

"4to. It is by gastro-enteritis that small-pox begins; by gastro-enteritis and an acute ocular, nasal, guttural, or bronchial catarrh, that measles and scarlatina commence.

"5to. Intermittent and remittent fevers are periodical *gastro-enterites*, but the encephalon and other viscera are irritated sympathetically, as in continued fevers, and may also become

the principal seat of irritation, and put on inflammation in an acute or periodic manner.

"6to. The fevers termed pernicious differ from other fevers, only by the violence and danger of congestions."

M. Broussais holds that these propositions are proved by the following facts; we give his own words.

"1mo. All the causes of fevers act locally; they all irritate the gastric mucous membrane, that point of the organism on which the action of every morbid cause strikes.

"2do. In almost all fevers there are unequivocal local symptoms of irritation of the stomach and small intestines, a fact that does not allow us to overlook the existence of gastro-enteritis.

"3tio. The sympathetic symptoms, in the absence of symptoms of gastric irritation, demonstrate evidently, though indirectly, the existence of gastro-enteritis in all fevers.

"4to. A great number of organs do not participate in a morbid state in fevers, and of those that do, some are more, some less affected by it.

"5to. The adynamic and ataxic symptoms are due to irritation.

"6to. After death we always find traces of gastro-enteritis.

The above assertions constitute the doctrinal code of the Broussaists, certainly the most important medical sect of the present day.

The curative administrations in all fevers, practised and inculcated by this, as, by all other sects, are the legitimate offspring of their theory, as to the proximate cause of the disease.

We do not mean to notice here, the minor varieties of Localists of our own country, (schismatics from this parent stock,) neither those who found their innocent little heresies on particular symptoms and traces of organic lesion, not very frequently present; nor those, whose vague notions as to topical congestions, have led to destructive, and perhaps, erroneous practice.

We pass over in silence the individual examination of M. Pinel's five essential fevers, and the arguments adduced to prove, that they are but so many examples of gastro-enteritis. Indeed, our authors can see nothing but their favourite "*gastro-enterite*" in the putrid fevers of Pringle, the typhoid epidemics, described by Fracastorius, Tissot, Roederer, &c.; nay, the very intestinal worms, often found after death, are, according to M. Broussais, produced by *gastro-enterite*.

Severe and peremptory as the Animists were in their condemnation of the doctrines of their predecessors, the Humorists—the Solidists in their enmity towards the Animists; we shall find that Localists are equally, if not more determined in their condemnation of their immediate masters and rivals, the Essentialists. The following ultimate, and sweeping conclusion, to which our authors arrive, respecting the theories of the Pinel-school, we think, justify this assertion.

"We conclude from the researches which

we have made preparatory to writing this article, and from the facts which we have accumulated in its different paragraphs,—that the general descriptions of inflammatory, bilious, mucous, adynamic, and ataxic fevers, arising from an attempt to reconcile facts dissimilar, or as yet undetermined, being inexact, arbitrary, and false, ought never hereafter to be reproduced in works of pathology."

As the size and expense of the work, from which we have selected the present article for review, must render it inaccessible to many of our readers, we have taken much pains to offer a concentrated view of the different theories, in the examination of which, our authors have given proofs of great research and critical acumen. We have been more particular in our notice of the doctrines of M. Broussais, because they exercise, at present, a paramount influence on the whole medical practice of the Continent; and, because they are rapidly gaining ground in this country. Theories are important, not so much from the reality and consistency, or utter groundlessness of the facts, upon which they appear to repose; as from the decided bent they have at all times given, when adopted generally, to the measures by which we endeavour to prevent, or modify disease. We have followed our authors in their theoretical research, in order that we might arrive, with minds better prepared than they otherwise could have been, to the discussion of the practical parts of our subject, the anatomical characters, the symptoms, and treatment of these inflammations, which, by some, are thought to cause, or constitute fever; by others, to be but secondary effects of a more general cause. Be this, however, as it may, it is not the less true, that if we can remove the phlegmasia, or congestions alluded to, we shall very rarely fail in curing the general functional disorder, by which they are always accompanied.

#### GASTRITIS, GASTRO-ENTERITE, &c.

As the admiration which our authors, (particularly M. Rayer,) every where express, for the doctrines and practice of M. Broussais, appears to us to border, rather too closely, on enthusiasm, we have deemed it prudent to take in a little practical ballast from M. Chomel, on the subject of *gastritis*, before we approach the stormy ocean of *gastro-enterite*.

"Gastritis, or inflammation of the stomach," says this enlightened physician and laborious observer, "is one of those affections, which, for some years past, have particularly engaged the attention of medical men, and given rise to a great number of works, for the most part polemic. If discussions were of a nature to throw much light on the history of disease, our knowledge of the one in question would have made rapid progress. But the experience of ages has proved, that the sciences of observation march with facts; and that scholastic disputes have seldom profited them much. Instead of collecting with care, and without any other intention than that of coming at the truth, numerous examples of

this disease, the greater number of physicians have devoted themselves to theoretic discussions, have almost entirely neglected to collect facts, or have viewed them only through the dangerous prism of prejudice. What has been the result? Inflammation of the stomach, embarrassed by an almost infinite number of useless writings, obscured by inexact, or truncated observations, is at this moment, one of the phlegmasiæ, of the symptoms and anatomical characters of which, we have but an imperfect knowledge. This assertion may appear false, to some, but will not surprise those who have carefully watched the sick, and opened many bodies. In fact, if on the one hand, it is often impossible during life, to determine with certainty, what the state of the stomach will be after death; and if, on the other hand, the anatomical examination of this viscus is not generally sufficient to determine with precision, whether there were, or were not symptoms of gastric inflammation during life; are we not compelled to acknowledge, that this phlegmasia is one of those, our knowledge of which, is as yet but very imperfect. The number, and the discrepancy of the writings, of which this disease has been the subject for many years, would be almost enough to demonstrate this assertion.

"We do not, however," continues M. Chomel, "pretend to say, that gastritis is at all times, and in all its degrees, an obscure affection; we shall see on the contrary, that in many of its forms, it is marked by symptoms as defined as those of pleurisy and pneumonia."

#### GASTRO-ENTERITE.

We pass over the remainder of M. Chomel's excellent monograph on gastritis, for the purpose of entering at once upon the legitimate domain of M. Broussais.

Nothing can be more lucid than the order, nothing, in our opinion, better judged, than the method, observed by M. Rayer, in his distribution of the heads, or points of view, under which he has treated this celebrated disease, or rather this *panto-pathological* essence of all diseases. The whole article is a perfect model of what a medical monograph ought to be. There is neither the laboured verbiage of speculative theory, nor the fatiguing minuteness of narrow observation. There is enough, however, from which to stock the mind with the most useful parts of what is known on this interesting subject, and enough to stimulate it to further inquiry. In short, we conceive it to be the very best, and most practically useful essay, that we have as yet seen to emanate from the school of Broussais.

The following paragraph contains some statistical details, curiously indicative of the influence of medical theories.

"Gastro-enteritis—a name proposed by M. Broussais, to designate the simultaneous or successive inflammation of the stomach and small intestines, is, of all the phlegmasiæ the most frequent, and, at the same time, that which has been oftenest overlooked or mistaken. You will not find it indicated in any

nosological table. Gastritis itself was not long ago pretty generally looked upon as a very rare disease; for of twenty-eight thousand, two hundred, and ninety-nine sick admitted into the civil hospitals of Paris, in 1807, *six only* were designated in the definitive returns, as labouring under inflammation of the stomach; whilst six thousand, one hundred, and forty-three, were treated for continued or remittent fevers. Such a result cannot be comprehended, except by admitting that, according to Brown's principles, the name gastritis was only given to inflammations of the stomach, produced by poisons; or, perhaps it may be better to suppose with Hufeland, and many French physicians, that, in consequence of some occult changes having taken place in the constitution of the atmosphere, the bilious, mucous, and adynamic fevers have been supplanted in these latter times, by gastro-intestinal phlegmasiæ, the frequent appearance of which is now no longer disputed."

M. Rayer, after showing that the specific and separate denominations, *gastritis* and *enteritis*, have been often vaguely applied by nosologists, in their artificial arrangements, to the simultaneous, or immediately successive inflammations of the stomach and small intestines, observes—

"That as by far the best authenticated part of the history of the phlegmasiæ of the digestive organs are the changes they undergo, it must be by these changes that their inflammations can be best characterized and specified."

"I am (says our author) certain, that I shall be able to render more intelligible the variety of phenomena produced by these inflammations, by beginning the description of gastro-enteritis with the alterations that constitute it."

Previously to a separate enumeration of the various alterations occurring in gastro-enteritis, our author makes the general remark, that—

"All the forms, and all the terminations of acute and chronic inflammation, observable on the external teguments, or on the different points of the gastro-pulmonary mucous membrane, may take place in the stomach and small intestines, but with unequal frequency. These gastro-enteritic phlegmasiæ, noticed by Morgagni, Prost, &c., and very recently pointed out by M. Broussais, have furnished materials to Andral, Bichat, and Louis, for novel and important observations."

According to his proposed plan, M. Rayer commences with the organic alterations, (of which he describes twenty-one,) that have been found in the stomach and bowels of those who are supposed to have died whilst labouring under gastro-enteritis. The changes that occur in the colour of the mucous membrane are first considered.

*Accidental red tints*, which he compares with cutaneous exanthems, are said to be the most constant of all the effects of this phlegmasia. They appear either under the form of *vascular arborescence*, or of *circumscribed*

patches, varying from the most intense red to the deepest brown. Sometimes there are a number of little red spots, like the marks in purpura, occasioned by minute but separate submucous hæmorrhages.

All these changes of colour are most frequently found at the bottom, and great curvature of the stomach, and on the intestinal valvules. An universal deep violet tint of the mucous membrane, black ecchymosed spots, and sometimes effusions of blood into the cavity of the stomach, may, we are assured by Morgagni, be produced by the course of the blood in the vena portæ being obstructed. The slate-coloured gray, the greenish-gray shades, which M. Broussais has sometimes remarked, affecting the gastro-intestinal mucous membrane, he considers as products of inflammation; nay, the morbid dead-white paleness of the coats of the stomach and small intestines, noticed by Bonetus, but more particularly remarked by M. Guersent, as frequently found in children, is set down to the account of inflammation by our author; as also the softening, to the consistence almost of boiled starch, described by this writer.

Here we would take the liberty of remarking, that if every shade of colour, from the darkest red to the deadliest white, may give proof of inflammation having existed in the tissue where there are found, we would say, "*nimum ne crede colori.*" The necessity of this caution we have seen frequently exemplified by that accurate pathologist Lennec. In his post-mortem examinations, he would order a portion of a healthy artery, say of the aorta, to be detached, and filled with the gore of the subject. At his next clinical lecture he would order this preparation to be exhibited, when the lining membrane of the artery was always found stained or dyed a lively red, such as could not at the moment be either rubbed or washed off.

*Softening (ramollissement)* of the stomach and small intestines; "very rare (says our author) in the fœtus and the old; frequent in new-born infants and at the period of weaning; often observed in adults, more particularly in lying-in women. This disease attacks indifferently both sexes." By John Hunter it was attributed to the solvent action of the gastric juice; but is now pretty generally allowed to be the result of inflammation. This view seems to be confirmed by the pustules and figured inflammatory patches that generally co-exist with this singular affection, and by the success of antiphlogistic means in its treatment.

"This disease (says M. Chomel) is always accompanied by acute pain in the stomach. To this organ the patient refers all his sufferings. Can we then, hereafter, assert, that the mucous membrane of the stomach is insensible? that it expresses its sufferings only by sympathetic phenomena? Is it not, on the contrary, evident, that it obeys the common law, and indicates its lesions, like other organs, by pain, and the disorder of its functions?"

We would strongly recommend to our readers the recent work of M. Louis on this very interesting subject.

*Ulcerations*—a frequent termination of inflammation in most parts of the body, and certainly the most unequivocal testimony, that the tissues in which we find it after death had been inflamed during life. The greater frequency of ulcerations in the lower portion of the ileum is attributable to the greater frequency of pustulous inflammation in that part of the intestine, and not, as has been asserted by some writers, to the greater abundance of mucous follicles. "The duodenum, so plentifully furnished with follicles, is, of all parts of the small intestines, the least liable to ulceration."

*Gangrene* is an exceedingly rare termination of gastro-enteric inflammation, notwithstanding M. Broussais' assertion, that he had often observed it. Our limits will not allow us to do more than merely to enumerate the other alterations which pathological anatomy is said to have found in this disease. These are thinning of the parietes of the intestinal canal; thickening of ditto; diminution, or augmentation of caliber; pustules; vesicules; papules; membraniform exudations; cream-like exudations; inflammation of the submucous cellular tissue; emphysema; œdema; perforations; vegetations; scirrhus and medullary matter; tubercles, &c.

The first stage of gastro-enteritis consists in a mere sanguineous injection of the mucous membrane. The second in any of the various morbid exudations—the third in ulcerations—the fourth in thickening, or thinning of the coats, and the formation of accidental tissues. The fifth, in gangrene or destruction of the parietes themselves. As the first, second, third, and fifth stages may be developed within the first four days, they are said to belong to acute gastro-enteritis. The fourth stage, or deep and permanent alterations of texture, belongs exclusively to chronic gastro-enteritis.

*Acute Gastro-enteritis.*—All ages and sexes, from the fœtus in utero to the centenarian, are liable to this disease; and every thing that can be introduced into the stomach, from the viands of the alderman's feast to the most acrid poison, from tonic draughts and emetics to knives swallowed whole, may produce it. Starving; itself a principle remedy for this disease, when carried to excess, gives rise, according to J. Hunter, Dumas, and Magendie, to inflammation, and even ulceration of the stomach.

"But of all the causes of this malady, the inflammation of other organs is the most remarkable, and certainly the least noticed. Dupuytren, Abernethy, and Broussais, have successively pointed out the influence of large burns, inflamed wounds, &c. in exciting gastro-enteric inflammation."

*Symptoms.*—Of the four local phenomena usually assigned to inflammation, viz. pain, heat, redness, and swelling; only the first two are appreciable during life in this disease;

and they are both generally in proportion to the intensity of the inflammation, except when complicated with cerebral affections, and then they are never so sensibly felt. Pressure on the region of the epigastrium, seldom, or never fails to cause pain, when gastro-enteritis is present, but we should take care in applying it, not to mistake what may be the effect of our own exertion, or the patient's natural sensibility, for the morbid tenderness of an inflamed organ. This particular phlegmasia we may say, is never found uncombined with other important derangements, as has been satisfactorily proved by the dissection-returns drawn up at the Hôtel Dieu, by order of M. Dupuytren.

According to the school of Broussais, it is to the various combinations, or groupings of the symptoms attendant upon this disease, in its different stages and complications, that we owe the countless variety of fevers described by authors.\* We cannot help acknowledging that there is much truth in this observation. When the constitutional symptoms common to all inflammations were present, with a white tongue and viscid saliva, 'twas the mucous fever, with a yellow tongue, the bilious, with a black, the putrid fever. When there was tenderness of the abdomen, with much debility, it was the adynamic fever. When the gastro-enteritic inflammation was complicated with cerebral symptoms, it was the ataxic; with thoracic symptoms, it was the mucous catarrhal fever, or the pneumonia notha.

If it be true, and we believe it is as good as proved, that all the symptoms known to accompany European fevers at least, also accompany the disease we are treating of, in its acute form—if it be true that in nine-tenths of these fevers there are evident signs of gastro-enteritic inflammation during life, and that unequivocal traces of its having existed are found after death—if it be true, that this connexion of fever with the phlogosed state of the digestive organs was not efficiently noticed before the time of Broussais, then must it be also true, that this pathologist has conferred an incalculable benefit on the healing art, by calling the attention of those that exercise it, to this important, and hitherto unknown coincidence.

It has been proved beyond all contradiction, by the most ample statistic records of post-mortem examinations at the Hôtel Dieu—by the experiments of Orfila, by Abernethy, and many other authorities, that large inflamed wounds, burns on the trunk or extremities, and poisons applied to the surface, produce inflammation of the gastro-intestinal membrane, of the thoracic viscera, of the encephalon. The successive effects of this transition of inflammation, whether it be by sympathy, irritation, or absorption, from the surface to the centre, are an arrest, or alteration of chylickation, depraved secretions, tenderness of the epigastrium, nausea, quickened circula-

tion, heat, loss of appetite, emaciation, and all the complicated derangement of function, that extensive organic lesion never fails to bring with it. From these symptoms, as they may be variously combined in different subjects, under different circumstances, the closet-nosologist, or speculative essentialist, can compose any variety of fever, and establish an ideal species according as he finds mucus, bile, debility, or delirium prevail. But practical anatomy acknowledges only one disease, made up of certain phenomena consecutive upon a burn, a wound, or a capital operation.

*Treatment.*—In these cases, at least, there is a cause of fever anterior to gastro-enteritis. Broussais says that it is an irritation; but in his second proposition he states, that "every irritation that produces fever is an inflammation." See the effect of this theoretical hitch upon his practice. He applies leeches in every variety of fever, in all its stages, nay almost in every disease, to the region of the stomach, the thorax, the head, the extremities; pursuing the sympathetic irritation, or inflammation of *gastro-enterite* through all these different localities; and this, to the almost total exclusion of every other remedial measure, except the most rigid starvation, which he calls "diète absolue."

The above comprises the whole treatment of acute *gastro-enterite*, and consequently of every variety of continued fever. The expulsion of foreign bodies from the stomach and bowels, the recalling local inflammations that may have suddenly disappeared, or the subduing those that appear to keep up the disease by sympathy, can scarcely constitute an exception.\* In the following paragraph on prophylactic treatment, M. Rayer offers some good, though perhaps not very palatable advice to old gentlemen, and bold practitioners.

"This disease will become more rare in old men, when they shall have given up their elixirs of longevity, their stomachic tinctures and pills—their precautionary purgatives. It will become less frequent with all classes of society, when physicians, more reserved in the employment of potent medicines, shall have begun to count a little less on the tolerance of the stomach."

Emetics, purges, (amongst the rest calomel,) nay, even bark, are all formally denounced. Nothing, as we said before, is left with which to combat disease, by this exclusive theory of localism, but leeches and starving. This dangerously simplified practice is not the result of, though its utility is said to be confirmed by, experience. It is the direct consequence of the third article of the Broussais creed, as already quoted, viz. "that all fevers

\* Sauvages gives 150 varieties of fever.

\* A costive state of the bowels persisting, without any alvine discharge during five, or even ten days, is looked upon as a good symptom, and is not interfered with by M. Broussais.

are connected with, and referrible to, gastro-enteritis."

#### CONTRAST OF LOCALISM WITH HUMORISM.

How much more natural, how much more consistent with the facts, of which we are best assured in pathology and therapeutics, is the notion, that some change must take place in the circulating fluids before fever can be developed? The practice that results from this theory rejects none of the *juvantia*, overlooks none of the *lædientia*. It leaves their management where it ought to be left, to the practised judgment of the competent physician. All that we have hitherto learnt by the study of symptoms and signs; all that we have been taught by the mechanical analysis after death, of the organs that appeared to suffer during life, confirm the justness of this notion, and contribute to its development. Do we not observe the fluids become depraved as the constitution becomes more generally, and more intensely involved in fever? Do we not see all the organs resume their healthy functions, as the blood throws off by sweat, by hæmorrhage, by urine, or by stool, something, which a moment before formed a part of its own stream? But the very source of that stream is in the stomach and bowels. The principle that regulates the proportion, the quality, the relative position of its molecules, is the nervous energy. That which affords due excitement to this regulating power, is the healthy blood. No man will pretend to say, that there can be fever when all the fluids are in their healthy condition, because, on the one hand, there is due nourishment of the nervous mass, on the other, due distribution of its influence. But, as the paramount laws of matter, generally, cannot be sacrificed to individual preservation, the very organs, whose office it is to prepare and convey the materials of health to the different parts of the animal machine, must, from their very structure, take up and prepare poisons under certain circumstances, or produce by their influence, when morbidly excited, deleterious changes of combination.

Every thing then that we do in disease, ought to tend towards re-establishing the purity of the fluids when it has been disturbed. The practice of the Father of Physic was founded upon this principle. He purged upwards and downwards, to clean and prepare the digestive organs, when they appeared to suffer from foulness. He produced direct diminution of the circulating fluids by bleeding. He administered demulcents, refrigerants, corroborants. He attended to diet, and encouraged critical discharges. But above all, he distinguished the cases and the stages of disease, in which the adoption of these respective plans of treatment might be advisable. He practised an art that required the highest powers of the mind for its successful exercise. The Broussaists, on the contrary, like the Brownists, have reduced the whole science to one or two unbending maxims, which they call physiolo-

gical, to be learnt in an hour, and practised without distinction; viz: that all fevers consist in *gastro-enteritis*, and their cure in local bleeding and starving. These remedies, excellent on some occasions, we have seen carried to such indiscriminate, and destructive lengths, by M. Broussais himself, that the subjects of them perished, perfect bloodless skeletons; and, instead of showing signs of an inflammation requiring such rigour, the whole alimentary canal resembled the finest oiled paper, in paleness and transparency; exhibiting perhaps, a few arborescent patches of red, at the most declining and sharp folds of the small intestines.

There is yet another disadvantage in this doctrine, besides those already noticed. Its views do not extend beyond the limits of anatomy, and this has been lately brought to a pitch of advancement, beyond which we cannot hope to see it carried, without drawing largely on the imagination. This doctrine does not direct our research towards that other great opening that leads to chemical pathology; a source from which, we venture to predict, the next great light will illuminate our science. This is an almost uncultivated field, and in the present state of our knowledge, holds out the most brilliant hopes to the zealous and qualified labourer. We should rejoice to see such men as Prout and Orfila, enter with energy upon the track of organic chemistry.

Broussais, his followers, and his opponents, have done much good. To their controversies we owe the rapid progress that has been made in the pathological anatomy of the hollow viscera—the prudent caution now observed, even in this country, in the use of drastic purges and emetics. But from Broussais alone, we have learned the full value of that symptom, so important in fever, the sensibility of the epigastrium to external pressure. It is by applying to the etiology of this, and its subordinate functional derangements, the lights of morbid anatomy, that we are sometimes enabled to guide our practice in the commencement of fevers, so as, by prudent depletory, evacuant, and dietetic measures, to shorten their course, and mitigate, or prevent their worst symptoms.

The pathology and treatment of chronic gastro-enteritis, as understood by M. Rayer, do not come within our plan. They are not necessarily connected with fever. We, therefore, pass them over, as well as the heads *intermittent*, and *epidemic gastro-enteritis*, because they would lead too far into polemic theory, and because the reader is referred to other articles of the dictionary, such as *fièvres intermittentes*, *typhus*, &c. The very titles, however, with what we have already said, will afford a tolerably correct notion of the almost unlimited extent, to which Broussais and his disciples apply their pathological pass-word. We must, however, add, that those who neglect to make themselves acquainted with the morbid alterations, the symptoms, and the principles generally, upon which the doctrines of this school are founded, want, in our opi-

nion, a large portion of that knowledge, which a good physician of the present day, ought to possess.

We shall close this article with the concluding words of M. Rayer.

"The alterations of the stomach and intestines are so varied, and so few comparative therapeutic experiments, have been, as yet, made under well determined circumstances, that it would be difficult to ascertain, even approximately, the sum of the cures which it is permitted to hope for from the antiphlogistic plan, as compared with the more active measures, such as moxas, blisters, setons, &c. In fine, if in the present day, we see but few practitioners administering medicines for the cure of *particular symptoms*, as was, heretofore, the case—if physicians no longer blindly combat *pains in the stomach* with narcotics—*dyspepsia* with bitters, elixirs, mineral-waters, &c.—*sour regurgitations* and *belchings* with absorbents—*vomitings* by anti-emetic potions—*constipation* by calomel—*diarrhoea* by astringents, it is because, since the splendid researches of Pujol, and of Broussais, it is generally acknowledged, that the inflammatory alterations which produce these different symptoms, ought alone to form the source of curative indication."

From the Edinburgh New Philosophical Journal.

## ON THE SPONTANEOUS COMBUSTION OF THE HUMAN BODY.

On the 12th May, 1828, M. Julia Fontanelle read, in the Academy of Sciences at Paris, a memoir entitled, *Recherches Chimiques et Medicales sur les Combustions Humaines Spontanées*.

The observations which form the subject of this memoir are highly deserving of attention. In fact, besides the interest which they are capable of exciting from their very nature, they afford a new example of one of those phenomena, the existence of which has, in these later times, been questioned, solely because, while they are very singular and difficult to be accounted for, they are also of such rare occurrence, that they can only be authenticated by an aggregate mass of evidence, which evidence, although sufficient to induce conviction, may always be rejected by those who are prejudiced, or who do not give themselves the trouble of duly estimating their value.

Are there really spontaneous combustions of the human body? Such is the first question which the author examines, and he resolves it by the affirmative. Fifteen observations of spontaneous combustions, which he successively relates, enable him not only to establish the incontestible reality of the phenomenon, but also to make known the principal circumstances which accompany its manifestation. In summing up these circumstances, he remarks:

1. That persons, who have been destroyed

by spontaneous combustion, have, for the most part, been immoderately addicted to the use of spirituous liquors.

2. That this combustion is almost always general, but that it may be only partial.

3. That it is much rarer in men than in women, and that the women in which it has been manifested, have almost all been aged; one woman only was seventeen years of age, and in her the combustion was but partial.

4. That the body and viscera have always been burnt, while the feet, the hands, and the top of the head, have almost always escaped.

5. Although it is demonstrated that several loads of wood are necessary for reducing a dead body to ashes by ordinary combustion, incineration is effected in spontaneous combustions without the most combustible objects placed in the vicinity being burnt. In one case there was a very singular coincidence of two persons being consumed at the same time, in the same apartment, without the apartment or the furniture being burnt.

6. It is not demonstrated that the presence of a burning body is necessary for producing spontaneous combustion of the human body; on the contrary there is every reason to believe the reverse.

7. Water, so far from extinguishing the flame, seems to render it more active; and after the flame has disappeared, the intimate combustion continues to be effected.

8. Spontaneous combustions have appeared more frequently in winter than in summer.

9. No remedy has been found for general combustion, but only for partial.

10. Those who undergo spontaneous combustion, are the prey of a violent internal heat.

11. Spontaneous combustion develops itself suddenly, and consumes the body in a few hours.

12. The parts of the body which are not consumed by it, are attacked with sphacelus.

13. In individuals affected by spontaneous combustion, there supervenes a putrid deterioration, which presently brings on gangrene.

14. The residuum of spontaneous combustion consists of greasy ashes, and an unctuous soot, both having a fetid odour, which diffuses itself equally through the apartment, impregnating the furniture, and extending to a great distance.

The author then explains the two theories of combustion between which the learned world is at present divided; Lavoisier's, and that lately proposed by Berzelius. He then gives an account of the theories proposed for the explanation of the phenomenon in question.

Most authors, who have spoken of spontaneous combustions, have imagined they discovered an intimate relation between their manifestation and the immoderate use of spirituous liquors in the individuals attacked. They suppose that these liquors, being continually in contact with the stomach, penetrate

through the tissues, and fill them up to saturation, in such a manner that the approach of a burning body is sufficient to induce combustion in them.

M. Julia Fontanelle does not consider this explanation satisfactory. He finds his opinion, 1st, on the circumstance that there is no proof of this alleged saturation of the organs in persons addicted to the use of spirits; 2d, on the circumstance that this saturation itself would not suffice to render the body combustible,—and, to demonstrate this assertion, he gives the result of several experiments, in which he in vain tried to render ox-flesh inflammable by steeping it for several months in brandy, and even in alcohol and ether.

Another explanation has been proposed. Dr. Marc, and with him several other physicians, from the development of hydrogen gas which takes place in greater or less quantity in the intestines, have been led to imagine that a similar development may take place in other parts of the body, and that the gas might take fire on the approach of a burning body, or by an electrical action produced by the electric fluid, which might be developed in the individuals thus burnt. According to this theory, MM. Lecat, Kopp, and Marc, suppose, in subjects affected by spontaneous combustion, 1. An idio-electric state; 2. The development of hydrogen gas; 3. Its accumulation in the cellular tissue.

This latter explanation would appear to be confirmed by a very curious observation of M. Bailly's. That physician, on opening, in the presence of twenty pupils, a dead body, over the whole of which there was an emphysema, which was greater in the lower extremities than any where else, remarked, that, whenever a longitudinal incision was made, a gas escaped, which burned with a blue flame. The puncture of the abdomen yielded a stream of it more than six inches high. What was very remarkable, was, that the gases contained in the intestines, so far from increasing the flame, extinguished it.

M. Julia Fontanelle, for reasons similar to those which induced him to reject the first hypothesis, is of opinion that the presence of hydrogen gas cannot be admitted as the cause of spontaneous combustion. He finds this opinion more particularly upon experiments in which he in vain tried to render very thin slices of flesh combustible, by keeping them for three days immersed in pure hydrogen gas, in percarburetted hydrogen gas, and in oxygen gas.

Lastly, he considers the opinion equally untenable, that spontaneous combustion of the human body is owing to a combination of animal matter with the oxygen of the air, whatever may be the alterations which this matter may undergo: 1. Because a sufficient temperature is not developed; 2. Because, admitting this combustion as real, the residuum would be a charcoal, which could only be incinerated at a red heat, while, on the contrary, there is nothing but ashes; 3. Because one of the products of spontaneous combustion of

the human body is an unctuous substance, which the combustion of animal substances never yields; 4. Because it scarcely yields any ammoniacal products, while such are always produced by animal combustion.

After thus rejecting all the hypotheses hitherto proposed, M. Julia Fontanelle concludes that this phenomenon is the result of an internal decomposition, and is altogether independent of the influence of external agents. We give his own words:

"We consider," says he, "what are called spontaneous combustions of the human body, not as true combustions, but as intimate and spontaneous reactions, which depend upon new products originating from a degeneration of the muscles, tendons, viscera, &c. These products, on uniting, present the same phenomena as combustion, without losing any of the influence of external agents, whether by admitting the effect of the opposite electricities of Berzelius, or by adducing in example the inflammation of hydrogen, by its contact with chlorine, arsenic, or pulverized antimony, projected into this latter gas," &c.

It may be objected, however, that whatever may be the cause which induces this combustion, the caloric disengaged ought to be considerable, and consequently should ignite all the objects in the neighbourhood. We reply to this, that all combustible substances do not by any means disengage an equal quantity of caloric by combustion. Davy has shown, that a metallic gauze, having 160 holes in the square inch, and made of wire one-sixtieth of an inch in diameter, is penetrated at the ordinary temperature by the flame of hydrogen gas, while it is impermeable to that of alcohol, unless the gauze be very much heated. According to the same chemist, gauze of this kind, raised to a red heat, allows the flame of hydrogen gas to pass through it, without being permeable to percarburetted hydrogen gas. It is probable from this, that the products arising from the degeneration of the body may be very combustible, without, however, disengaging as much caloric as the other combustible bodies known, and without leaving a residuum as the two latter gases; and, in fine, we are of opinion, that, in some subjects, and chiefly in women, there exists a particular diathesis, which, conjoined with the asthenia occasioned by age, a life of little activity, and the abuse of spirituous liquors, may give rise to a spontaneous combustion. But we are far from considering as the material cause of this combustion, either alcohol, or hydrogen, or a superabundance of fat. If alcohol plays a prominent part in this combustion, it is by contributing to its production; that is to say, it produces, along with the other causes mentioned, the degeneration of which we have spoken, which gives rise to new products of a highly combustible nature, the reaction of which determines the combustion of the body.

It is to be regretted that the observations hitherto published are not more complete. We propose to ourselves to collect all that

may tend to throw light upon a subject so important in anthropology and medical jurisprudence.

From the *Lancet*.

CLINICAL LECTURE ON THE EXTIRPATION OF THE NECK OF THE UTERUS. By M. LISFRANC, Paris.

*To the Editor of the Lancet.*

SIR,—The following are notes which I took at a clinical lecture given by M. Lisfranc, previously to amputating the neck of the uterus of a young woman, in whom schirrus had commenced at that part. I am aware that you have already published, in your 12th vol., an account of this operation by Mr. Turley of Birmingham; but as there are some points mentioned in this lecture concerning the anatomy of the parts, and the symptoms following the operation, which he has not noticed, as well as a fuller description of the after treatment employed, you will, perhaps, not think it unworthy a place in your Journal; more especially, as this operation has not yet met with the attention which I think it deserves in this country.

I remain, Sir,  
Yours obediently,  
O.

M. Lisfranc began by giving a short history of this operation. The first attempt was the removal of the whole uterus through the vagina; this was successfully performed, in one case, by a surgeon in Switzerland. Langenbeck removed the uterus, in fifteen cases, by an incision above the pubes, as for the high stone operation; but all his patients died. M. Dupuytren removed the neck of the uterus by paring it *in situ* with a knife, curved laterally, having first introduced a speculum to guard the vagina and fix the uterus: this mode M. Lisfranc reprobated: the blood prevents you seeing whether you have removed all the disease or not; and the vagina may be wounded from the uterus receding before the knife. Many of his cases terminated fatally.

M. Lisfranc then proceeded to describe his own operation. This is to be commenced by introducing the speculum, (described by Mr. Turley, but for which Weiss's might be substituted with advantage,) the blades of which are then to be gradually separated; this stretches the upper part of the vagina, and brings the mouth of the uterus within the tube; the inner surface of which being polished, reflects the rays of a candle held at the orifice, and enables one thoroughly to examine the state of the part. The neck of the uterus should now be seized transversely with a pair of forceps, each blade of which terminates in two pointed curves, care being taken to apply them at an equal distance from the fore and back part, otherwise the uterus will descend obliquely, and the incision will be oblique, instead of perpendicular to the axis of the organ; in applying the forceps we should seize the

part rather suddenly, otherwise the instrument may slip and wound the vagina; we now remove the speculum, and gradually draw the uterus as low as we can; this point differs in different women, in some we can make the neck project some lines beyond the vagina, in others it will not quite reach the orifice; the latter case rendering the operation rather more difficult. An assistant should now separate the labia moderately, and another pair of forceps are to be applied perpendicularly to the former, with the blades above and below; for if we have only one pair, when the cut has past the level at which they are applied, the uterus is drawn obliquely by the part yet undivided; and instead of the incision being in one plane, a flap is formed, of some lines in length, on the fore part; the second pair obviates this inconvenience. The assistant now raises the neck of the uterus a little, to allow room for the knife; this is a curved blunt-pointed bistoury, the blade of which is sheathed with linen from the handle to within an inch and a half of the point; with it the neck is to be cut directly across from behind, at the requisite height; the forceps are then removed, and the uterus is allowed to retire.

The bleeding is not generally great; in only one of 22 cases, in which I have operated, have means been used to check it, and in this they were probably unnecessary, as they were employed by a young student in whose charge the patient had been left.

About an hour after the operation, vomiting generally occurs, returning every quarter of an hour for four, six, or even ten hours; this is, at first, alarming, the patient becoming pale and exhausted; the vomiting too generally causes a return of bleeding; but I am loth to use means to arrest it, as it discharges the uterus and prevents the chance of metritis. These symptoms subside gradually, and by the following day the patient appears quite well, the lancinating pains of the cause having disappeared, &c. In only one case metritis appeared, but readily yielded to a large application of leeches.

On the third or fourth day, the surgeon should introduce his finger into the vagina, and remove any clots which may have lodged there, as their putrefaction would irritate the wound. I think this better than using injections, which should not be employed until the seventh or eighth day, as the parts are as yet tender from the tractor, and there is danger lest the pipe be incautiously thrust against the wound. About the eighth day we may examine the part with the speculum; and we shall generally find it covered with healthy granulations and half cicatrized; we may now begin to use an injection of the solution of chloro-oxide of sodium, weak at first, gradually increasing the strength; the wound heals rapidly, but should a small ring remain sore, round the orifice of the uterus, we may employ the nitrate of silver in solution. In five or six weeks the patient is well.

In some patients, during the healing, some alarming symptoms occurred, viz. lancinating

pains in the uterus; these, however, generally proved to be preludes to the appearance of the menses, and disappeared with them.

The objections which have been urged against this operation are, 1st. That the peritoneum descends so low, and the insection of the vagina on the uterus is so narrow, that there is danger of wounding the peritoneum in removing the diseased part. 2d. That in many cases the neck is so softened as not to afford sufficient hold for the forceps: To the first I reply, that before I performed this operation, I examined the parts in upwards of 50 subjects at la Pitié, and in all I found that the insection of the vagina instead of being linear, occupied a space of three or four lines all round, and that the distance from the commencement of the insection to the fold of the peritoneum was seven lines in front, and six at the back, thus obviating all danger of wounding the peritoneum, even were we to amputate at the insection of the mucous membrane. To the second objection, I can only say, that where the neck of the uterus is so softened, the body will be always diseased, and that nothing but the complete removal of the organ can succeed.

The patient, a young woman, aged 24, was now brought in and operated on; the chief part of the pain was caused in drawing down the uterus; the incision also gave more pain than in either of the former cases I saw, (those described by Mr. Turley,) for then neither patient was aware when the incision was made, and thought, when the operation was concluded, that the preliminaries only had been made.

Bleeding continued for about four hours, about 14 oz. of blood were lost, which caused fainting. In the evening slight pain in the belly, for which she was bled to 4 oz., was rather sick once, but did not vomit. Passed a bad night; probably kept awake by a woman in labour in the same ward. On the following day she had a slight attack of gout in the middle finger of the right hand; she had had one attack previously.

The case continued to do well, and the wound healed kindly.

From the London Medical and Physical Journal.

### RHEUMATISMUS FEBRILIS.

*Paper on a singular Description of Disease which appeared in the Island of St. Christopher, in the latter end of the year 1827 and beginning of 1828.* By JOHN SQUAER, Esq. Assistant Surgeon 93d Regiment.

The disease which forms the subject of the present paper had broken out for several weeks in the islands in our immediate neighbourhood to leeward, which are all foreign and "free ports," with the exception of the small island of Tortola, (which is a dependency of St. Kitt's,) and have frequent intercourse with us, by means of small English trading vessels;

consequently it is not to be wondered at, the facility with which disease of an epidemic nature finds its way here.

It is not meant that the present disease should be considered as having been brought to this island by means of communication: proof enough will be afforded to upset this idea in speaking of the comparative prevalence of this disease amongst the troops of the garrison and inhabitants, even although it is characterized in the newspapers of the island by the title of "*an epidemic of the most painful nature, which the oldest inhabitant did not remember to have seen or heard of before.*"

This disease is classed as a species of rheumatism, in the quarterly return of sick of this garrison, and is styled by me rheumatismus febrilis; commonly known by the name of rheumatic fever in this and the neighbouring islands.

In the latter end of December and beginning of January of the present year, many people began to complain of very violent headache, severe pain in the temples, shooting towards the forehead; frequently it was situated in the back of the head, stretching towards the neck and shoulders, which was one of its most painful positions, as the least motion created great agony, and it was difficult to find any easy posture for the head. There frequently was a painful sensation as if the head were drawn down towards one side or another; pain, or, at all events, a disagreeable sense of stiffness, was felt in the eyes, especially when moving them from side to side, or raising them upwards: the patients expressed it, by saying the socket felt as if it were too small for the eyeball; frequently the eyes felt painful to the touch; the adnata was slightly tinged with red vessels.

Shooting pains were at the same time felt in the back, loins, and thighs, particularly immediately over the knees, which soon became fixed and uncommonly severe: the same thing took place in the arms, forearms, wrists, fingers, knees, ankles, and feet, causing lameness; the calves of the legs were similarly affected.

A roseolar eruption came out early in the disease, which covered the wrists and extended up the forearm: it spread over the backs of the hands; the ankles and feet were in the same state; it was sometimes elevated in large wheals, and, when it affected the neck, it was extremely painful: the hands and feet were considerably swelled.

In delicate females, the roseola came out on the face in patches, and on different parts of the body, and remained for several weeks after the other symptoms had disappeared.

It need hardly be added, that motion of any kind greatly aggravated the symptoms, and the gentle pressure of the hand could scarcely be suffered.\*

\* The stiffened form, occasioned by the pains in the head connected with the shoulder, and the dread of motion, obtained for it the fantastic name of "the dandy."

Fever came on simultaneously with these various affections, or very soon was observed in conjunction with them, marked by a sense of heaviness in the head and great listlessness, nausea and loss of appetite, and, in delicate people, the irritability of stomach was sometimes distressing. Severe rigours, and alternating flushes of heat; face flushed; quick full pulse; and hot dry skin; with, in a few cases, delirium, were also observed.

Pain of stomach, sensible to gentle pressure, was present in one or two instances.

The violence of the symptoms and fever lasted from four to five days; but it was never under seven or eight days that all the pains were gone. In most cases, the pains were felt for a much longer time; and in severe attacks, pain and tenderness to the touch remained in the eyes, hands, calves of the legs, ankles, and feet, for several weeks afterwards.

These symptoms varied in number and degree of violence, according to circumstances, and were much influenced by mode of living and constitution, sex, and age.

The soldiers composing the garrison of Brimstone Hill were less liable to this disease than the inhabitants; and their attacks were not of so long continuance, nor generally so severe. Nearly all the officers had it, and it was severe in one instance only. The very general run it took amongst the inhabitants had the effect of its being supposed to be epidemic; in many instances, not leaving a family till every one had been attacked.

The young and robust had smart attacks, and fever of shorter duration, and they did not so often labour under its effects; and were even exempt from one or two symptoms that afflicted people of an opposite description.

Delicate females and aged persons had more protracted attacks, and they suffered more from irritability of stomach; and the roseolar eruption in them was most remarkable; and the feet continued swelled and tender, producing lameness for some time: the fingers were also swelled and painful.

On account of the lingering nature of the disease, many were induced to suppose that, during the space of eight or ten weeks, they had fresh attacks, and were even impressed with the idea that they must have a third attack before they could get well: this was owing to exposure to the cold damp weather, which at first caused it, and consequently easily re-excited the pains they had not entirely got quit of.

This disease, in all the instances I have witnessed, was considered of a simple, and though of a violent nature, yet there was nothing dangerous in it. It has been said to have terminated fatally in one or two instances in this island: in some of the others; it has caused death in several instances.

This circumstance I am inclined to attribute to some untoward combination of disease, or might be the result of accident, as was the case in one instance. A coloured man, of the town of Old Road, having had symptoms of the disease, thought himself sufficiently well

even to go to his work, imprudently bathed in the river, which aggravated the disease to such a degree as to cause his death: previous to which, the irritability of stomach was very great, vomiting quantities of black-looking matter repeatedly.

Inflammation of the stomach, I am inclined to think, is the unfortunate combination which, in fatal cases, commonly is the cause of death. In a very few instances, I have observed it in the commencement of the attack, and it was necessary to direct particular attention to this symptom, or combination; for, as there is a possibility of this combination appearing in greater or less degree, so as perhaps to be little heeded, and be allowed to proceed too far, without any precaution being taken to remove it, it is at once accounted for how it may become the cause of death, and confirms the truth of this opinion.

Instances of relapses were few amongst the troops, and none of the lingering symptoms attached to them that have been enumerated in the description of the disease.

Children seemed for some time to be exempt from this disease, but latterly they have also suffered. It was indicated by peevishness, and soreness on being touched; great irritability of stomach; in those who were able to walk, an imperfect manner of using the limbs was observed, causing them frequently to fall. Feverishness was present in all.

There were a few peculiarities noticed in this disease, which entitled it to be considered as a novel and unknown kind of morbid affection: 1st, the extreme violence of the pains in the commencement, and the peculiar sensations they created; 2d, perspiration was not easily excited; 3d, thirst was not much complained of, even in the violence of the fever and in delicate females; 4th, the roseolar eruption above mentioned, and the swelling and tenderness of the hands and feet, was not often observed in the cases in the garrison; 5th, delirium was chiefly confined to delicate females, and aged persons of weak, nervous constitution.

The weather, previous to the appearance of the disease under consideration, and during its continuance, was of a nature unprecedented in severity in the West Indies, at least for very many years.

In the latter end of November, and nearly up to the present period, the weather became extremely boisterous, being nothing but a continuance of heavy rains and high winds; the evenings cold—very cold for this country, so much so that we were obliged to shut our doors and windows on sitting down to dinner; and we found it requisite to cover ourselves with a blanket at night. No Creole constitution could hold out against such weather: they are generally of such a frame that they are not at all capable of undergoing the fatigues and exposure of Europeans who have been a few years in this country. Besides, their mode of living and their habits are also very different from the regiments serving in the country. It was even surprising we escaped as we did;

and, although the dress of our men was not altered, which at the time was merely linen trowsers, and which could not guard them very well from the cold damp air of the night, yet we did not suffer in any thing like the way that the natives did. The greater part of our men are young, of strong constitutions, and well fed: it was not to be supposed that they could easily be affected by weather that to them must have been only agreeable.

In treating this anomalous disease, the objects had in view were to lower increased action in the system, and to restore the deranged functions of the vessels of the skin, which might be almost considered the cause of the disease. In accomplishing these purposes, few means were required. The pain of head, and great degree of excitement in young men of stout habits, sometimes required blood-letting, but, generally speaking, it was seldom employed: there was a much better remedy found, which nearly answered both purposes, and that was cathartics. Aloes, colocynth with a combination of calomel, in the following proportions and manner of exhibition, was the usual plan adopted:—*R. Extract. colocynthidis comp., aloes socotrinæ, gummi resinæ, āā gr. iij.; hydrargyri submuriatis gr. xxiv. fiat massa in pilulis duodecim dividend sumantur tres h. s.* On the following morning, the pills were assisted by doses of infusion of senna, to which was added a small quantity of neutral salts, or supertartrate of potass. This plan generally required to be repeated once or twice.

When the action of the bowels was thus increased and kept up, the febrile action and violence of the symptoms underwent an almost immediate diminution; particularly the headache, which was the most distressing symptom. Cold water was at the same time applied to the head, by means of folds of linen.

To determine to the skin was another mode of treatment employed, to remove altogether the pains of different parts of the body: this was effected by using the warm bath, and giving small doses of antimonial or James's powder, with a few grains of calomel, three or four times a day; and keeping the body warm, from the commencement of the treatment.

The diet was light and plain. Wine, when it might be advantageously employed, was given.

When pain of stomach was present, which very rarely was the case, and, when it was increased by pressure, its removal was always of the first moment; in doing which, the counter-irritation occasioned by the application of vesicants was a very powerful remedy. Gentle purgative injections at the same time were essentially useful.

With regard to the employment of the sulphate of quinine, I am not able to bear testimony of any power it is supposed to possess in diminishing the violence of the symptoms, or in preventing returns of this disease; and seeing no reason to believe that there existed any morbid consent between the sensorium and deranged impressions of distant parts, I never

employed this medicine with the view of defeating its return.—I must confess I used it (the sulphate of quinine) in one or two instances only, before the nature of the disease was exactly declared. It came on in subjects accustomed to frequent attacks of ague, the symptoms of which were chiefly complained of at the beginning. It afterwards turned out that violent pains, such as those that characterize rheumatismus febrilis, were conjoined; therefore cannot positively decide in favor of the sulphate of quinine having any effect in shortening the disease, or in preventing its recurrence when it had apparently gone off.

The disease was very apt to return, or, from having disappeared, was liable to be again excited, if the patients were unguardedly exposed to its causes, which have been stated to be an extraordinary degree of cold and damp in the atmosphere, and the prevalence of high winds, with heavy rain. Under these circumstances, the best security that could be had against its aggravation or recurrence was to defend the body by warm clothing, and confinement to the house, or even to the bed-room.

During the last six weeks we have had rather better weather, but it is still far from being settled, and of late the disease has not appeared in any thing like the frequency it did some time ago.

*St. Christopher's, April 11th, 1828.*

From the London Medical Repository and Review.

#### ON THE LIVER, AS AN AUXILIARY PULMONARY ORGAN, with some *Practical Remarks on the Effects induced by its Derangement.*

Whether with respect to human labours the "quid utile" can be always beneficially answered, it is difficult to say: but with regard to the works of the Creator, the response must always be in the affirmative. Every thing which He has created has its use; and although, in reference to many parts of the human system, we are as yet unable to point out their uses, yet that these parts have uses, no intelligent mind can doubt. In proportion as anatomy, physiology, and pathology are pursued, uses are found in addition to those which were previously supposed, and even imagined uses are discovered not to be real. There is often an unfortunate haste in ascribing wisdom to the Creator, where He has not shown any, at least in the way in which the wisdom is considered as being shown. This fact shall be kept in view in the following remarks: and a due modesty will be preserved in speaking of the uses of the important viscus, the liver, now under our consideration.

There is no need to enter into any anatomical detail respecting the liver generally; and any description that is to be given will be limited to those peculiarities that seem to favour the view pointed out above, that the liver is an auxiliary pulmonary organ.

The use commonly ascribed to the liver is

that of aiding digestion, by favouring the separation of the nutritious and excrementitious parts of the food, and by communicating a peculiar stimulus to the feculent part, thereby aiding its expulsion. That this use is not sufficient, is an opinion founded upon several circumstances. The first to be mentioned is the large size of the liver in the fœtus, and the quantity of meconium contained in the intestines. The size of the liver being so great in an individual where no process of digestion goes on, must be for some other purpose: and this, it seems justifiable to suppose, is the separation of a quantity of impure matter from the blood, which, if carried into the circulation, would be injurious. Again, in jaundiced people digestion goes on: and Dr. Fordyce, having tied the ductus communis choledochus, found that the formation of chyle took place as before.

The peculiar situation of the liver with respect to the circulation of the blood, seems to favour the view that it is an auxiliary pulmonary organ. The blood, rendered impure by the different changes going on in the system, is about to be returned to the lungs. Loaded with impurities, it is brought to the liver on its way, by veins, and from this impure venous blood, the fluid called *bile* is secreted. Why should the liver, were it not for some purpose of this nature, differ from all other viscera, in having, as its fluid on which to act, *venous*, and not *arterial*, blood? And why should the liver be placed, as it were, just at the termination of the venous circulation? And why should the blood of the intestines, which, it seems reasonable to suppose, must be the most impure part, be that which, in particular, is made to pass through the liver? All these questions seem difficult to answer, unless we allow that the liver is an auxiliary pulmonary organ, acting as such, by *separating from the blood, principles, which, if carried into the system, would be highly injurious.*

In further illustration of this view, some facts, showing certain similarities between the vessels of the lungs and of the liver, may be mentioned.

The pulmonary artery, like all arteries, has the power of contraction; and Magendie states, that the branches of the vena portæ are the only ones which, by the disposition of their external membrane, are able to contract when the blood which passes through them is diminished in quantity. The pulmonary veins, too, as contrasted with the venæ cavæ hepaticæ, have no valves, and do not anastomose after they have acquired a certain thickness. In both these respects the two sets of vessels agree. Barclay says, "In all the arteries, the blood flows from the trunk to the branches; in the veins, it flows from the branches to the trunk. The vena portæ is the only exception. This singular vein is ramified at both extremities. By the branches of one extremity it collects the blood from the stomach, spleen, pancreas, and intestines; and by the branches of the opposite extremity distributes it through the liver."

From the various anomalous circumstances with respect to the pulmonary artery and the vena portæ, it seems difficult to say whether the pulmonary artery resembles a vein more than the vena portæ resembles an artery. This difficulty seems to have presented itself to ancient anatomists, for "prior to the time of Harvey, all the blood-vessels connected with the right or pulmonic ventricle were considered as veins, and hence, in those days, the pulmonic artery was called the *vena arteriosa*, or *arteria venosa*. In the same way all the vessels connected with the left or systemic ventricle were considered as arteries; and hence the pulmonary or systemic veins were regularly termed *arteriæ venosæ*, or *venæ arteriosæ*."—Barclay on the Arteries.

Some morbid manifestations, which seem to favour the view that the liver is an auxiliary pulmonary organ, may now be stated, being premised by a brief statement of the chemical composition of the bile, and also of the change which the blood undergoes in the lungs.

The *proximate* elements of bile seem to be water, resin, picromel, albumen, soda and its salts; perhaps a little phosphate of lime, and some oxide of iron. These, reduced to their *ultimate* elements, will give a great quantity of *carbon*, besides *hydrogen* and *nitrogen*. Thus, it will be seen, that by the liver a considerable quantity of carbon is separated from the blood. Again, with respect to the change of blood in the lungs, every one knows that it consists, in a great measure, in the evolution of *carbonic acid*. Here then we perceive a similarity in the function of the liver and of the lungs: in both the separation of carbon is the consequence of their action. This short statement may be concluded by a passage from Magendie: "On account of the considerable extent of the mucous surface, with which the drinks or other liquids are in contact, and of the rapidity of their absorption by the mesenteric veins, a considerable quantity of liquid, foreign to the economy, traverses the abdominal venous system in a given time, and changes the composition of the blood. If this liquid arrived at the lungs in this condition, and proceeded thence to all the organs, very serious inconveniences might arise."

Now as to the morbid manifestations. Every one who practises in a great city must have met with cases similar to the following:—

J. A., aged 26, troubled with a swelling of his right side, complains of a weight there, and of pain upon pressure; his bowels constipated, requiring, generally, medicine to keep them open; has frequently, almost constantly, an oppression of spirits, with a sense of tightness across his chest, making him every now and then take a full breath for relief; great drowsiness; horrible dreams at night; and is so little refreshed by sleep, that he finds great difficulty in rising in the morning; and when he does rise, is very little refreshed: the tongue in the morning is covered by a thick fur. He experiences a general languor, and is better when any powerful motive calls him into bodily and mental activity, and also when in the country.

He has a peculiar pulsation at the pit of the stomach, detected by the stethoscope. He looks well; so that his friends imagine he is not so unwell as he pretends to be: eats moderately, and drinks freely.

Such cases, I imagine, are very common. They come across my path, and are often treated with success by using means which have the effect of rendering the circulation through the liver perfect, and thereby restoring the secretion of bile, which, being in proper quantity, stimulates the intestines, and enables them to act afterwards of themselves.

These symptoms seem to arise from the bile not being separated in proper proportion from the blood, which, thus impure, passes through the lungs, and from its state, oppresses that organ, and is carried to the head, inducing that heaviness, that unwillingness to think or act, that sluggishness, those unpleasant dreams. The bile being deficient, the intestines want their appropriate stimulus, and hence cannot act. The peculiar pulsation indicates an impediment to the flow of blood through the liver, which aids the effect of the impure blood in producing the symptoms already described.

The authority for the correctness of these views is founded upon a series of observations, but more particularly upon some humoral pathological facts which have come under my own notice, and some stated in a work, by Dr. Foote, on cholera Indica. To these the reader's attention is particularly requested.

In this disease there is no secretion of bile, at least of bile properly formed; it thus differs from the common cholera in this country. The patient complains of weight in the epigastrium and hypochondria; complains of a tension about the abdomen; exhibits the greatest anxiety; is unwilling to be disturbed. The disease is generally preceded by vomiting and purging; but in those cases, where these symptoms do not precede, the collapse is instant, and the individual dies in a soporific state. There is a peculiar pulsation at the pit of the stomach. On dissection, the liver is found gorged with blood, the veins filled with blood of a blackish colour, and the intestines filled with fetid air. The gall-bladder is frequently found full of thick bile, resembling pitch. The bladder is constantly empty; the lungs exceedingly collapsed; the veins of the dura mater are turgid, and, according to Finlayson, the brain seems covered with general ecchymosis. The blood, too, (which is a curious circumstance,) when a vein is opened, during life, flows very tardily; sometimes, indeed generally, it dribbles away, drop by drop, and is blackish.

Dr. Foote, from all these facts, concludes that there is an *excess of carbon and nitrogen in the blood*, the secretion of the bile and of the urine, and that from the skin, being stopped; and that the excess of these deleterious ingredients, acting either *per se*, or producing some deleterious ingredient by a union together, induces the symptoms described.

Dr. Foote also states a very interesting fact, namely, that the tension experienced in cholera Indica, and the non-coagulation of the

blood, are symptoms occurring from the bite of the snake called *Cobra di Capello*; that the peculiar fetid air found in the intestines, is met with in individuals bitten by poisonous fish; and that the above state of the blood occurs in asphyxia, in those killed by the inhalation of carbonic acid, by lightning, by malignant fever, and by the plague.

These are very curious facts, and they testify, with the foregoing, the effect produced when the liver does not perform its function. Indeed, the symptoms of cholera Indica bear a very near relation, the difference being in degree, to those which have been described in the case of J. A.; and from long observation, the results of which may, perhaps, hereafter be more fully detailed, it is concluded that the symptoms of the case of J. A. were produced by the liver not performing its function as an auxiliary pulmonary organ, of separating from the blood those principles, carbon and nitrogen, which remaining, are so deleterious. And this view is strengthened by the fact, that fresh country air is often very successful in relieving the torpidity of body and the listlessness of mind occurring in such morbid states. This subject I at present leave. I appear to some, no doubt, to tread very closely upon the humoral pathology; but this I regard not—truth is my aim, and to do good is my object; names alone I hold to be trivial things; they should never, to a philosophic mind, be an object of terror.

Some affections of the lungs, induced by a diseased state, may be noticed at some future opportunity. M. D.

London, June 15th, 1828.

From the Medico-Chirurgical Review.

#### THE INFLUENCE OF ANATOMICAL VARIETIES ON SURGICAL OPERATIONS. By M. ROBERT.

Anatomical deviations have been studied physiologically, and from this study have been deduced the laws of their formation, and the development of monstrosities. The study of these varieties of structure, under a surgical point of view, would be of great benefit to the surgical word, and to society at large. Suppose, for example, a surgeon is called on to operate for an aneurism of the brachial artery at the bend of the arm, and is ignorant that this vessel may bifurcate as high up as the axilla. Instead of searching carefully for the vessel which is diseased, he ties that one which lies in the usual place. Meanwhile the tumour enlarges, and at length bursts. Attention to the study of anatomical varieties prevents such oversights as these. It is of great importance that the surgeon, when operating, should be acquainted with all the anomalies of structure which the part may possibly present. With this knowledge in his mind, he will not be alarmed or embarrassed in his operation, should the anomaly exist. M. Robert has evinced great industry and research in this extended essay, and the

order which he follows in his inquiries, consists in a successive examination of the great regions of the trunk and the extremities, subdividing his researches into varieties of structure in the bones, the ligaments, the muscles, the vessels, the nerves—and lastly the viscera themselves. We shall follow our author, and endeavour to present our readers with a succinct account of his researches.

#### I. THE HEAD-BONES, AND FACE.

The cranium presents great varieties in structure and shape. We often meet with depressions and protuberances, which might be confounded with sanguineous tumours or fractures of bone, after contusions. The defect of ossification, in consequence of which the fontanelles sometimes remain unclosed, is a common variety—but it is not so well known that the brain sometimes protrudes through these apertures, and forms an *encephalocele*, which becomes strangulated as the bones thicken; and requires the trephine at the sides of the unnatural opening. In very old people, the skull becomes so much attenuated, in some places, as to render operations hazardous, if this circumstance be not borne in mind. The frontal bone has remained divided by a prolongation of the sagittal suture, and has been taken for a fracture, of which a remarkable instance is recorded by Quesnay.

A very curious case of development of the vessels of the diploe is given by Frank.

A peasant, aged 24 years, in a scuffle with a soldier, received a sabre wound on the anterior and right side of the frontal bone, an inch and a half from the sagittal suture, where it is obliterated at this place in the adult. The sabre had removed a portion of the external table of the skull. The man neglected himself for some days, and when Frank arrived there were such symptoms as induced him to apply the trephine in the neighbourhood of the wound. As soon as the saw had arrived at the diploe, a torrent of blood issued forth—and, in short, the patient died of the hæmorrhage the same day. On dissection, it was found that there were seven vascular communications between the dura mater and the diploe, through so many foramina in the internal table of the skull, at this place. Hence the fatal hæmorrhage. More recently, Magendie attributes to this kind of hæmorrhage, the majority of those cases reported by writers, where *extravasated* blood has issued forth after the application of the trephine.

A great many varieties occur in the anatomy of the face. The supra-orbital foramen may be placed quite behind the orbit, making it extremely difficult to divide the frontal branch of the fifth pair, in the *tic douloureux*. The *os unguis* is sometimes wanting, and its place supplied by the nasal process of the superior maxillary bone. In such a case considerable difficulty would be experienced in perforating the bone for fistula lachrymalis. In some individuals a middle bone is found between the two portions of the inferior maxillary; it may either be single or divided by the

symphysis into two, and, from its mobility, has been mistaken for a fracture of the lower jaw and treated accordingly. There may be two *ranine* arteries, so that in glossitis our author prefers making deep incisions on the dorsum of the tongue, to opening the *ranine* veins. When, as sometimes happens, the *membrana pupillaris* remains after birth, it is necessary either simply to puncture it, make a free division, or cut away a point of the circumference of the iris, and form an artificial pupil. The inferior orifice of the lachrymal canal is frequently guarded by a valve, obstructing the passage of the stilet. There are sometimes two *stenonian* ducts which should be remembered in the operation for salivary fistula. Not unfrequently the spinous process of the seventh cervical vertebra, is separate from the rest of the bone and moveable beneath the skin. After an injury upon the part, this might readily be mistaken for a fracture. The *scalenus medius* of Soemmering, when it exists, separates into two bundles the nerves of the axillary plexus. This would cause much embarrassment in the operation for tying the subclavian. Sometimes the muscle is directly interposed between the artery and nerves, which would completely protect the latter; and in a child, which we had an opportunity of examining last summer, there was no *scalenus anticus* muscle at all, so that the subclavian vein lay in contact with the subclavian artery. The *omo-hyoideus* may arise not from the scapula, but from the middle of the clavicle, which would also embarrass the operator on the subclavian.

#### II. VESSELS OF THE NECK.

Allan Burns mentions an instance, and Harrison, we believe, gives another, where the *right* subclavian arose from the *descending* arch of the aorta, and passed across the neck in front of the trachea, as high as the inferior border of the thyroid gland. In such a patient, were tracheotomy performed, the artery would most inevitably be wounded. It must have been some irregularity in the distribution of the great vessels which happened to a celebrated professor of surgery at Berlin, who having performed the operation of bronchotomy on the daughter of a brother-professor, the patient died under his hands.\* The subclavian may take the same origin but pass behind, instead of before the trachea, between it and the *œsophagus*. In such a case, care would be required in performing *œsophagotomy*. A third variety in the course of the right subclavian, is where it takes its rise from the arch of the aorta, beyond the origin of the left subclavian, and on the left side of the body of the second dorsal vertebra. It then crosses in front of the spine, and behind the thoracic duct and *œsophagus*, lying much exposed to wounds from foreign bodies sticking in that tube. In 1815, Mr. Kirby† was summoned to

\* Walter. Mémoires de l'Académie de Berlin.

† Dublin Hosp. Reports, v. iii. p. 324.

a woman, who had swallowed a piece of bone, and who, in spite of tracheotomy, died of suffocation and hæmorrhage, apparently from the interior of the œsophagus. On dissection, it was found that a spicula of bone had pierced the posterior wall of the œsophagus, and transfixed the right subclavian, which took this unusual course. When there is this variety, the nervus vagus on the right side, occasionally sends off no recurrent branch, its place being supplied by several branches arising from the inside of the vagal trunk. In such a case, if the carotid were tied these branches would probably be cut, and the voice perceptibly affected. Immediately after its exit from between the scaleni, the subclavian, in some instances, becomes fairly enveloped in the brachial plexus. Perhaps it was owing to this anomaly that the nerve was tied instead of the subclavian, in the case related by Sir Astley Cooper. Zagorsky has observed, that the innominata on the right side was entirely wanting, but it existed on the left.\*

It has been well remarked by Hodgson, that aneurism of the innominata, or even of the arch of the aorta, will frequently rise so high in the neck as to be mistaken for aneurism of the carotid, or even of the subclavian. A case of the latter kind is mentioned by Allan Burns, where it was proposed to tie the subclavian artery. Fortunately, however, this operation was not performed.

The right carotid may arise from the arch of the aorta, instead of the innominata. It then mounts up a couple of inches, or more, in front of the sternum, and crosses the trachea, lying exposed, of course, to injury in tracheotomy and wounds of the neck. Both carotids, on the other hand, may arise from the innominata, and then the left takes the course in front of the trachea. In such a case it may admit of doubt whether a ligature of the innominata would not be attended with very serious results in consequence of the stoppage of the flow of blood at the same instant through one subclavian and both carotids. Portal has related a case in which extirpation of an enlarged tonsil proved fatal, in consequence, it would seem, of a wound of the internal carotid, which lay extremely near it. Barclay, also, mentions a similar case.

Allan Burns observed in a child, that the left vertebral artery took its rise from the arch of the aorta, whilst the right arose from the subclavian, and passed up behind the carotid, along with the sympathetic nerve, as high as the third cervical vertebra, where it entered the osseous canal in the transverse processes. In such a case, there would be some danger of including this artery in the ligature if it were necessary to tie the carotid, and besides, an aneurism of the artery in this situation would

be readily confounded with one of the carotid itself. However, if a surgeon follows the excellent rule of never drawing the knot upon a vessel till he is satisfied that pressure of it between his finger and thumb stops the flow of blood into the sac, this blunder can never happen. Some varieties occur in the distribution of the thyroid arteries which we shall briefly notice. The superior may arise from the common carotid low in the neck, pass up alongside of that vessel, and then when it has arrived at or near the angle of the jaw turn downwards to the thyroid gland. In this variety the vessel is large, and would be in the way both of the external incisions and the ligature in the operation on the carotid trunk. The inferior thyroid also, having taken its usual origin, may pass directly in front of the common carotid below. Burns saw both inferior thyroids arise by a common trunk from the subclavian or aorta, which would add to the danger of œsophagotomy. In tracheotomy, it should be remembered, that there is very frequently indeed a large artery, called by Harrison the thyroideus medius, which arises from the common carotid, and passes in front of the trachea. Burns mentions a case where this artery arose from the innominata, and passed upon the œsophagus as high as the left lobe of the thyroid gland. The patient was a young lad, and œsophagotomy having been performed, the bleeding from the vessel was so considerable as to require a ligature.

In the operations on the neck, the veins are often sadly in the way of the surgeon's knife. There may be two or even three external jugulars on one or both sides. Another variety has been noticed by Wilde, where a vein as large as the jugular, was formed by the union of two branches in front of the trachea, and proceeding upwards from thence dipped down by the side of the thyroid gland to join the internal jugular. Of course in laryngotomy and tracheotomy, such a distribution might be productive of considerable embarrassment. A case is related by Virgili in the Memoirs of the Academy of Surgery, where, having opened the trachea between the rings, such hæmorrhage took place, that the blood getting into the trachea excited a most violent paroxysm of coughing, forcing out the canula whenever it was introduced. In this dilemma, the man being almost suffocated, Virgili cut longitudinally into the trachea down to its sixth ring, and held the patient's head out of bed, with his face towards the ground. This operation was perfectly successful. M. Roux, in a similar case, introduced a silver catheter into the trachea, and sucked out the blood.

The subclavian artery, external to the scalenus, where it is usually tied in operations, is described as lying in a triangular space, the base of which is formed by the subclavian vein, and the sides by the scaleni muscles, and axillary plexus. The vein, however, is sometimes placed much higher than it *should* be. A man had a large aneurism of the subclavian, and the operation for tying this vessel was per-

\* The innominata has also been seen rising so high in the neck as to be endangered by the razor of the suicide, or the knife of the tracheotomist.

formed in the usual situation. The tumour was a good deal in the way, but the operator at last arrived at what he considered was the artery, and he was confirmed in this opinion, by finding that, on raising the vessel, the pulsation in the tumour ceased. The ligature was applied—bad symptoms supervened—and the patient died. On dissection, it was discovered that one of the brachial nerves and a part of the subclavian vein had been tied, the needle having passed *through* the latter vessel, which lay higher in the neck than usual. The cessation of pulsation in the sac, on lifting up the nerve, was explained, by its having dragged up the artery with it so as to produce considerable compression. The subclavian vein has likewise been seen passing *with* the artery, *between* the scaleni; in this variety it might either be tied for the artery, or both included in the one ligature. Morgagni found two subclavian veins, uniting where they joined the internal jugular, which would also be an awkward anomaly.

### III. VARIETIES IN THE ANATOMY OF THE THORAX.

These are few. The last rib may be deficient, with or without deficiency of the corresponding vertebra. An intercostal space will then be wanting, which should be recollected in the operation for empyema. Two ribs may be joined before or behind, or they may be entirely joined together, and then the double rib is commonly inserted into the sternum by two cartilages. Many varieties occur in the location of the muscles, but these we shall pass over, as they are of little practical importance. There may be two intercostal arteries, one taking the course of the *mammaria interna*, the other lying in the middle of the intercostal space, and exposed in the operation for empyema. In a phthisical patient opened by Morgagni, death was occasioned by rupture of the *vena azygos*, which was as large as the superior cava.

This concludes the first part of M. Robert's Memoir, and we freely award him our tribute of approbation for the patient industry in collecting facts which it evinces. Such men are the pioneers of science, and their task, though uninviting and laborious, is of infinite importance. The subject of surgical anatomy is scarcely paid sufficient attention to in this country, for if it were, we should have fewer opportunities of seeing reports of errors and unfortunate operations blazoned forth before the gaze of a credulous and greedy public. We have been at considerable pains to detail the varieties which occur in the vessels of the neck, as the anatomy of that truly *surgical* region is complicated at the best, and the blunders which have been made in it, even lately, are unluckily notorious. In a succeeding number of the *Journal des Progrès*, we are promised a continuation of the Memoir, and we shall take care to lay an abstract of it, when it comes, before our readers.

From the Medico-Chirurgical Review.

### HOSPITAL REPORT OF M. CHOMEL, from La Charité, between the 1st of February and the 1st of September, 1827. Reported by M. DE LAGARDE.

1. **PERIPNEUMONY.**—The total number of cases [of all kinds] treated in the above period are 281, of whom 39 died. The proportion of acute diseases was greater than usual, as compared with the chronic. To show how uncertain is the ratio of mortality in the same disease at different periods, we may remark, that out of eighteen cases of peripneumony, occurring in the spring quarter of the above period, six died—while in an equal number of peripneumonies occurring in the summer, there was not a single death. The treatment adopted by M. Chomel (who is an excellent practitioner) was active depletion, adapted to the violence of the symptoms and strength of the patient. But although this treatment was put in force, even from the earliest dawn of the inflammation, the later could not always be checked; but pursued its course to a fatal termination. Large doses of antimony were tried in some cases, with success—in others, without good effects.

2. **INTERMITTENTS.**—Twenty-four patients labouring under intermittent fever were admitted during the half-year. The types were various, and some were complicated with slight inflammations, not requiring any active treatment. A few days were always allowed to pass before the febrifuge was administered. In eleven cases the cure was spontaneous, without any medicine, and merely from change of scene and proper diet. When the ague resisted this process, the quinine was given in doses of not less than six grains in the 24 hours, with complete success.

3. **ANEURISM.**—One case of aneurism of the arch of the aorta, with pulsating tumour below the left clavicle, presented itself. The patient was a washerwoman, aged 38 years. The plan of Valsalva was tried, but without success, and the patient left the hospital, tired out with the starvation and depletion which she underwent there, without any apparent benefit.

4. **GASTRIC COMPLAINTS.**—Several patients labouring under derangements of the stomach [*embarras gastrique*] were treated during the semestre. Many were cured by low diet, repose, and acidulated drink. In some cases it was necessary to prescribe emetics. In a certain proportion of cases, these were extremely serviceable, and removed the symptoms very quickly—in others, the emetics were injurious—"a fact which proves that stomach affections are not all of the same nature, and to be cured by a routine treatment."

5. **ACUTE RHEUMATISM.**—Most of these cases were managed by gentle antiphlogistics and low diet, requiring from two to four weeks for cure. In one case, which was very severe, and where almost all the joints were swelled and inflamed, the large doses of tartar-emetic were employed (six grains a day) without much, indeed with scarcely any good effect.

The **TOLERANCE** (a term applied to the period when the antimony ceases to cause sickness) was established on the second day; but little or no progress was made, the rheumatic inflammation rather shifting its seat than quitting its hold. After a month's sojourn in the hospital, the patient went away, by no means cured. It does not appear that our Gallic neighbours venture much on bark, arsenic, mercury, or other heroic remedy in this distressing disease.

6. **AMENORRHOEA**.—The general practice in France is to apply fifteen or twenty leeches to the pudendum. M. Chomel prefers applying three or four daily, for five or six days, at the expected menstrual periods, in order to imitate more nearly the process of nature. Previously to the expected epoch, he orders cupping-glasses (dry) to the upper and inner part of the thighs, and warm vapour baths to the lower part of the body.

We shall now proceed to analyse some of the cases detailed in this report.

7. **PNEUMONIA CARDITIS**.—A plumber, aged 40 years, previously strong and healthy, with the exception of some catarrhal affections, felt a shiver, on the evening of the 6th February, 1827, followed by pain in his right side. He went to work the next day, but on the 3d he was obliged to take to his bed. On the 10th Feb. he entered the hospital. Below the right nipple there was acute pain, increased by deep inspiration, with inability to lie on that side. The cough was frequent, the expectoration free and mucous, the sound, on percussion, rather dull, even on the left side. In the right side of the chest the "râle sonore" and the "râle crepitant" were heard in several points. In the posterior portions of lung, in this side, the respiration was pretty natural. In the *left* side the respiration was every where heard. He had been bled the preceding evening, and the blood was highly inflamed. Fifteen leeches to the side affected. Ptisans. 11th. Little alteration. Venesection to  $\frac{3}{4}$  xij. was ordered, but not carried into effect. 12th. The symptoms are exasperated, and the sonorous wheeze is heard in the left side of the chest. Bled to twelve ounces. 13th. No diminution of the symptoms—blood not much inflamed. To take six grains of tartar-emetic in the next 24 hours, in an emulsion. 14th. The patient is more quiet—no alteration in the state of the chest, as indicated by auscultation—four stools from the antimony, but no sickness. Tongue natural—no tenderness at the epigastrium. The antimony to be repeated. 15th. The countenance more sunken. Has had two stools. The antimony to be increased to twelve grains. 16th. Very obscure in the region of the heart, on percussion—respiration very quick—cough and expectoration the same—no stools, no vomiting—pulse 108, and irregular—great debility. Tartar-emetic to be increased to 24 grains. 17th. Had abundant perspiration last night—scarcely any respiration to be heard in the front of the left side. Antimony increased to 36 grains.

We need not pursue the case any farther.

The patient died on the 20th of the same month.

**Dissection.** There was no diseased appearance in the head. The left lung was everywhere adherent, and the superior three-fourths of that lung were hepatized—the inferior portions not quite so, but approaching to that state. The right lung presented the different degrees of hepatization. There was some ulceration in the mucous membrane of the larynx, about the glottis. There was pericarditis, and considerable effusion into the cavity of the pericardium, of almost a purulent fluid. The whole of the heart was covered with false membranes from a line to two lines in thickness. There were several small ulcerations, and other marks of inflammation in the mucous membrane of the œsophagus, stomach, and intestines.

**Reflections.** M. Chomel evidently anticipates the remark that depletion was too sparingly employed at the commencement of this disease. He excuses himself by observing that the two general bleedings produced no mitigation of the symptoms. But what are two bleedings of twelve ounces each? Double the quantity would have been a great deal too little. Besides, the day that was allowed to elapse between the first and second bleeding, was a fatal neglect. *Bis dat qui dat cito*. But M. Chomel, or his reporter, tells us that, in the same ward, a few days previously, a man had been attacked with pneumonia, and was very vigorously depleted from the beginning, yet still the inflammation went on to a fatal issue. Hence he concludes that there was something specific or malignant in the peripneumonies of the spring of 1827. There might be so; but we question whether either of these cases presented any just grounds for not carrying depletion to a much greater extent, without attending to any thing but the disorganizing inflammation that was evidently advancing daily on a vital organ.

Whether the large quantities of tartarized antimony which were swallowed during the illness, without producing either vomiting or purging, had any thing to do with the ulceration and inflammation observed in the *primæ viæ*, we will not pretend to say, since much greater devastations are seen where no medicine of this kind had been administered. But, considering the effects which this substance produces on the external surface, we should not wonder if a corresponding degree of irritation might not be produced by such large exhibitions internally. We shall make room for one more case.

8. **MOLLESCENCE OF THE COATS OF THE STOMACH**.—A married woman, aged 22 years, who had had one child, became troubled with a considerable menorrhagia in the beginning of the year 1827, at which period she was also exposed to several moral emotions of a distressing nature. Nevertheless she became pregnant, and experienced almost constant malaise—anorexia—thirst—tenderness at the epigastrium after eating—and, finally, vomiting of yellow and bitter matters. It was three days

after the commencement of these more serious symptoms that she entered La Charité, viz. on the 24th May, 1827. The expression of the countenance was natural, as was the state of the skin and tongue—the pulse was scarcely quickened—thirst very moderate—epigastrium very tender on pressure, but the abdomen soft and indolent. Each day she had ten or twelve vomitings of bilious matters, with some streaks of blood—stools regular. Leeches were applied to the epigastrium, and fomentations, lavements, diluents, &c. were employed, but without success. The vomitings continued—the tenderness of the epigastrium increased—the tongue was sometimes red, or shining—sometimes natural. Opium, for a time, diminished the sickness, but ultimately failed. On the 24th June, the sickness suddenly ceased—and the epigastric pain vanished entirely. But debility and emaciation advanced, and she expired on the 9th July, no vomiting having occurred for a fortnight before dissolution.

*Dissection.*—On opening the abdomen, the stomach was found torn from the cardiac orifice to about the middle of its anterior surface; but without any extravasation into the abdomen. A great portion of the mucous membrane of this organ was completely destroyed, and some parts of the muscular and peritoneal coverings were so soft and thin as to be ruptured almost by handling them. There were only a few red patches in the mucous membrane of the intestines. The uterus contained a fœtus of three months.

*Remarks.*—This was an extremely well-marked case of gastritis, (of the mucous membrane,) as far as pathology was concerned. But it is not a little remarkable that, while such a dreadful disorganization was going forward in a vital viscus, there should be so little febrile disturbance in the system. The pulse and skin scarcely evinced any deviation from a state of health, and the tongue was often natural. The cessation of the vomiting too, for a fortnight before death, was an occurrence not to be expected, according to the ideas which are formed from elementary instruction, and systematic descriptions of diseases. It is from clinical experience, and from faithful clinical reports, that the mind becomes stored with the knowledge of those almost infinite varieties presented in diseases, the want of which knowledge renders the practitioner liable to perpetual error in prognosis and diagnosis. The apparently dry details of a case of this kind are quite wearisome, if not disgusting to the young, and especially to the routine practitioner. But we can tell them, that a careful perusal of such cases is one of the best modes of disciplining the mind for receiving accurate impressions at the bed-side of sickness. There is a very prevalent idea among professional men, that *practice alone* makes the good and successful practitioner. We deny it—and this denial is grounded on more than 30 years of careful observation, not only of disease, but of men. In all that course of time, we never knew a good and successful practitioner who

did not read and study, as well as observe. It is usual for the lazy man of experience to quote John Hunter, as an example of great eminence, without reading. Not having known John Hunter, we cannot speak as to his *practical* talents; but the foregoing opinion is the result of what we have seen among our own acquaintances, which are not very few. It is fashionable to deride books and study; but, for our own parts, we have no hesitation in affirming, that nine-tenths of our *practical* knowledge would never have been acquired, had it not been for that discipline which results from studying the practical observations of others. This sentiment from gray hairs may probably have some weight with those who think that every thing is to be gained by the *sight* of diseases, and little or nothing from *reflection* excited by reading. Not a day passes—not a day has passed for twenty years, that we have not seen the most outrageous errors committed by men who pride themselves on never consulting any thing but their own *experience*. Such men were born in darkness—live in darkness—and will die in darkness.

From the Lancet.

#### ON THE FUNCTIONS OF THE DIFFERENT PARTS OF THE ORGAN OF HEARING. By Dr. CH. L. ESSER.\*

This paper is the extract of a work to which the prize was adjudicated in 1825, by the Faculty of Medicine of the University of Bonn.

*The cartilage of the external ear* appears to contribute very little to render the sounds more distinct, but it serves to increase their force; not only by reflecting a part of the vibrations into the meatus auditorius, especially those which fall into the concha, but also by means of the vibrations which the undulations produce, and which are transmitted to the membrane of the tympanum. It is not correct, therefore, to suppose, (as M. Itard does,) that the external ear is of no service to man in hearing.

*The bones of the head* do not contribute less than the external ear to the propagation of sounds, which does not take place solely through the medium of nerves, as some authors have supposed, (Treviranus, Swan, &c.) for, in such a case, a watch applied on the cheek, ought to communicate sounds as clear through the medium of the facial nerve, as if it were applied on the zygomatic arch; but this is not the case. The occipital bone is more adapted to the propagation of sounds, than the bones at the anterior part of the head; this is explained by its connexions with the labyrinth, and by the vicinity of the mastoid cells. The use of these cells is not to impede the echo in the interior of the ear, as M. Treviranus supposes; this function belonging to the Eustachian tube.

In several animals, the bones which sur-

\* Kastner's Archiv. für die ges. Naturlehre; tom. xii. 1er. cah. 1827, p. 52.

round the labyrinth, and those of the head in general, present arrangements very favourable for the propagation of sounds, in this way compensating for the absence of the external ear.

*The meatus auditorius externus* is evidently the part which contributes most to concentrate and transmit the sounds to the *membrana tympani*.

*The membrana tympani* is put into vibration by the undulations which reach it; a point doubted by M. Itard, although all other authors are agreed on it; this is, however, not the only use of this membrane; for the vibrations of sound can arrive at the internal ear without the assistance of it, and even with greater force; another function of the *membrana tympani*, is to protect the internal ear from external agents. The experiments of the author, and the facts which he relates, leave no doubt on this subject. The hypothesis of Antenrieth and Kerner, according to which the membrane of the tympanum is considered as an assemblage of cords differently stretched, in proportion as the membrane is round or oblong, is shown to be without foundation. The distinction of the different sounds does not rest on a mechanical arrangement of the ear; but on a psychological cause.

*The Eustachian tube* is the principal auxiliary of the membrane of the tympanum, and performs four different functions; first, it allows the air contained in the cavity of the tympanum to be placed in equilibrio with the external air. If this equilibrium be disturbed, derangements in the functions of hearing take place, such as the tinkling and stopping up of the ear. If the quantity of air contained in the cavity be increased by deep expirations, great pressure is made on the *membrana tympani*, and on the other parts of the cavity, especially the *fenestra rotunda*; this pressure is caused by the stopping up of the ear, which diminishes in proportion as the equilibrium of the air is re-established by means of the Eustachian tube. If the air in the cavity of the tympanum be rarefied, and the Eustachian tube shut from the effect of spasm, the external air presses on the *membrana tympani*, penetrates through its pores, and the tinkling is produced through this passage. The two phenomena disappear as soon as the equilibrium of the air is re-established in the cavity of the tympanum, which is effected by pushing the air towards the Eustachian tube, the mouth and nose being firmly closed; or by introducing the little finger very deeply into the *meatus auditorius externus*, and by gradually drawing it back, and pressing from below to above against the wall of this canal. Thus a vacuum is formed, the membrane of the tympanum is inclined towards the *meatus auditorius externus*, and the Eustachian tube gives passage to air from the back of the mouth. It is evident that this explanation cannot apply to all the phenomena which occur in the organ of hearing. Cerebral congestions, or derangements of nervous action, are the causes of more chronic sensations.

2d. The second function of the Eustachian tube is to allow the air contained in the cavity to be put in vibration; which could not take place if it were closed. In deafness from the obliteration of the Eustachian tube, the perforation of the tympanum is a means of cure, by re-establishing the communication of the tympanum with the external air. The opinion that the walls of the Eustachian tube are constantly in contact with each other is incorrect.

3d. The Eustachian tube prevents any confusion in the vibrations of air contained in the tympanum; and lastly, it serves to conduct the mucus secreted in the tympanum, and by its parietes, into the posterior nares.

*The bones of the ear*, by the assistance of their muscles, draw the *membrana tympani* in different directions; but it would be difficult to explain how, and for what reason, this effect takes place; their influence on hearing is not very clear; they serve to transmit the vibrations of the *membrana tympani* to the *fenestra ovalis*, although this is not their only use. Some physiologists, Treviranus, and others, deny the conducting property of these bones, because they found in hares a red and gelatinous mass, surrounding the ossicula; and which, according to them, is peculiarly fitted for the transmission of sounds; but this red mass is nothing else than effused blood, which is found only in those hares killed in the chase, whilst no trace of it is observed in those which are decapitated. The sac, in which this mass was contained, according to these authors, was, perhaps, the tendon of the stapedius. The bones of the ear, in some animals, are hollow, and thus appear more fit for the transmission of sound.

*The labyrinth*, although well understood in its anatomical relations, is, and will, probably, always remain the most obscure part of the organ of hearing, as to its real use. The experiments which the author has made on the existence of the fluid of Cotugnius, are not decisive, and it remains doubtful if this liquid exists during life, or whether it is merely produced after death.

*The vestibule* or *membranous sacs* which exist in some animals, and the *semicircular canals*, appear to contribute most to the sense of hearing; but it is difficult to say in what manner. By looking at comparative anatomy, we find that the principal utility of *semicircular canals* is to strengthen the sounds. They are very much developed in those animals which have no external ear, or which is badly formed, (for instance, birds, mole, man,) those *semicircular canals* which are well developed, are generally accompanied with a small cochlea, and *vice versâ*. Man is the only exception from the equilibrium between these two parts. The cochlea appears to be of less importance than the *semicircular canals*; for it soon disappears in the animal kingdom, and birds only possess a rudiment of it; it presents more varieties than the *semicircular canals*; its principal use appears to be that of presenting a greater surface for the vibrations of sounds, and of con-

centrating them, and in this way giving them power. The opinion of several authors, that the cochlea serves to distinguish the quantity and quality of sounds, does not agree with the results which comparative anatomy furnishes.

The distinction between different sounds is a function purely psychological. If the development of the cochlea indicates the development of the faculty of distinguishing the different species of sounds, the following is the

order in which it is found in animals and man: 1st. The porcupine has  $3\frac{1}{2}$  spiral turns: 2d. the dog and fox, 3 turns: 3d. man, cow, hog, and cat,  $2\frac{1}{2}$  turns: 4th. the horse and dolphin,  $2\frac{1}{4}$ ; and the rabbit, 2 turns: birds occupy the last place in this series.

The part which the *auditory nerve* takes in hearing is undoubtedly of the greatest importance; but the philosophy of its functions will always remain a mystery.

## Medical and Philosophical Intelligence.

*On the Circulation of the Blood, and the causes of Absorption.* By FERD. LAU, London.—There is no object in physiology that deserves more attention than the phenomenon of the motion of the blood in the animal machine. Physiologists have made the most zealous inquiries on this important subject, and many useful results have been obtained. But it must be confessed, that we do not yet possess a satisfactory explanation, as to all the powers that make the blood circulate. We know that the left ventricle of the heart pushes the blood through the arterial and capillary system; but we cannot conceive, that the same power is alone sufficient to propel it up again to the right side of the heart; still less do we understand how the blood of the *vena portæ* is a second time brought into a capillary system. Our researches must, therefore, be directed to detect some other additional power, besides that of the heart. One of the most important discoveries which has been made, relative to the circulation of the blood, is that of absorption by the veins, which is now established beyond all doubt. It is a general opinion that this absorption is effected by the diastole of the right heart; but this is contradictory to the laws of muscular action, which we only know to be contractive. But there is another process going on, which, in my opinion, must have an influence on the absorption of the blood in the veins, and which, as far as I know, has hitherto altogether escaped the notice of physiologists. Le Gallois, Rudolphi, and others, have observed, both in man and animals, that the glottis alternately shuts and opens; but Professor Mende, of Göttingen, had a favourable opportunity of accurately observing this action. He was called to the assistance of a man who, with the intention of suicide, had divided the larynx in such a manner that the glottis lay quite bare to view. He was so struck with the motion he observed, that he showed the curious fact to one of his colleagues, who was equally surprised to see what he never could have expected from the appearance of the parts in a dead body.

Professor Mende published a little treatise\* on this subject, in which he says that the

glottis is closed, and opened, alternately, by two bodies resembling the lips, in the act of shutting of which there seems to be some degree of power; and to make the closure still more complete, the epiglottis lays down on the glottis; likewise, as if by some muscular action, the closure takes place between each inspiration, and expiration, and lasts longest after the expiration.

It appears, then, that during the closure of the glottis, the communication between the air-canals of the lungs, and the external atmosphere, is entirely suspended; or that the closure is air-tight, which is also particularly favoured by the labial structure of the parts. Now it is my object to show, what the effect of this closure of the glottis must be on the circulation of the blood. We know that a part of the air is absorbed by the blood in the lungs; and are we, therefore, not entitled to conclude, that a vacuum is formed in the bronchi during the closure, and must not such a vacuum evidently have a powerful effect on the fluid blood, which thus, by atmospheric pressure, is compelled towards the vacuum? The pressure of the atmosphere chiefly acts upon the abdomen; the moveable contents of which, and the blood of the large veins, are pressed into the pectoral cavity, until the glottis is opened again, when the equilibrium is restored. This reasoning, I think, is founded on such indisputable principles, that there will be no difficulty in conceiving it. However, I consider the subject of such importance, that I strongly recommend it to the investigation of able men. It is now more than two years since I took this view of the circulation of the blood; and it appears to me, that it throws a light on many obscure points in physiology, pathology, and the action of medicines. —*Lancet*.

*On the Effects of Cupping Glasses on the Development of Vaccine Pustules.\**—The members of the Académie Royale de Médecine have been, for some time, endeavouring to prove the effects of exhausted glasses on virus and poison inserted into the skin, but they do not appear to have yet arrived at any satisfactory conclusion. Did Dr. Barry's theory of

\* Mende, von der Bewegung der Stimmritze.

\* Journ. Gén. de Méd. Mars, 1828.

absorption lead to so useful a point in practice, as to enable us to arrest the progress of poison inserted into the skin, it would be worthy of some attention, however contrary to the laws of physic it might appear. Dr. Barry, conceiving that the absorbent fluids are driven up towards the heart by the weight of the atmosphere on the surface of the body, was naturally led to suppose that, by depriving any part of that surface of atmospheric pressure, no absorption could go on in that part, whilst so deprived. Facts, so far as they have yet been collected, tend to support this opinion, although they prove no more than that the fluid in the absorbent vessels is propelled by the weight of the superincumbent atmosphere, than the flow of blood out of a punctured vein proves that the atmosphere *attracts* the fluid, against its own gravity, out of the punctured vessel. The committee appointed by the Académie to examine Dr. Barry's experiments, found that no absorption went on under a vacuum; that, in fact, if poison be inserted into the skin, and a cupping glass, with the air exhausted from it, be applied over the part, the absorption of that poison into the system is prevented. M. Itard, wishing to satisfy himself of this fact, repeated the experiment, not by inserting poison, as Dr. Barry did, but by inoculating the part with the vaccine virus. He vaccinated a child on the shoulders by several punctures, over some of which he applied glasses, whilst the rest were left uncovered. The punctures which were covered by the glasses formed no pustules, whereas all those which had not been so covered, gave rise to vaccine pustules, possessing the usual characters. M. Bousquet was not satisfied with the result of M. Itard's experiment, so that he determined to repeat it on a large scale, at the bureau of public vaccination of the Académie. M. Bousquet obtained a very different result from that which M. Itard had obtained. In the experiments of the former the glasses appeared to have scarcely any sensible effect in preventing the puncture from forming a pustule. It appears to be the general opinion among the members of the Académie, that a vacuum produced over the puncture may prevent the action of poison on the system, which generally takes place very suddenly, and in a degree proportioned to the dose; but that it is different with regard to a *virus*, which has the property of reproduction, and whose action is slow.

This subject appears to us easily explained. The application of an exhausted glass to a part will suspend the function of absorption in that part while it remains on. It acts in two ways in doing this; first, by the pressure of its edge on the absorbing vessels running from the part, thereby obstructing the motion of their contents; second, by *expanding these vessels beyond their natural caliber*, thereby suspending their function for the time. The absorbents are naturally intended to sustain the weight of the atmosphere, and when this weight is removed, they necessarily expand, and assume a caliber which is not natural to

them. The arteries and veins do the same. It cannot be expected that the absorbents, while in this state, can bring their sides together in order to press forward their contents. Let it be tried whether a portion of the intestinal canal will be able to contract so in a vacuum. As the vacuum of a cupping glass suspends the tonicity or contractility of the blood-vessels where it is applied, nothing can be more probable than that it suspends that of the absorbent vessels, whose coats are much more delicate than those of the arteries and veins. The fact appears to be, that the pressure of the atmosphere is natural to all the vessels, as it is to all other terrestrial objects, and that it is one of the causes which determine their physical form; it is also one of the causes which enables them to perform their functions, but it does not follow from this that they act like syphons, more than it does that the intestinal tube acts so.

Now, it is not only possible, but also probable, that all the poison, or even virus, may be extracted in some instances, if an exhausted glass is applied *immediately* after its introduction—that is, if the glass is applied before the poison or virus has entered the extremities of the absorbents. It may, by this means, be washed out by the blood. But should it once enter the extremities of the absorbents, nothing but a retrograde action of these vessels could discharge it again; for, as the edge of the glass extends all round to some distance beyond the wound, and as there is no pressure between the glass and the wound to force the poison back, it must necessarily remain at rest there, unless it can run back under the law of gravity. The only chance of abstracting the poison when it has once entered the extremities of the absorbents would be, by using a very small glass, which would barely extend beyond the edges of the wound. It is generally supposed, that, because an exhausted cupping glass extracts blood from a puncture, it must also extract the contents of the absorbents, or any thing else which may be in the wound. But this is by no means the case. The blood is constantly forced by the action of the heart towards that part, as well as towards others; it therefore flows out at the puncture. But as there is nothing to force the contents of the absorbents in a retrograde direction, that portion of the fluid situated between the edge of the glass and the wound will remain at rest, but the fluid situated between the glass and the heart may, and probably does, flow on in its natural course. It is true that the absorbents running into the wound from the distal side of the glass may pour out their contents, but, it is not probable that any of the poison enters the divided extremities of these.

Now, it would be advantageous to apply immediately an exhausted glass over the bite of a rabid animal, in order to suspend the absorption of the virus until instruments could be procured, and the consent of the patient and his friends obtained; to have the part excised. The absorption of any other virus or poison might be suspended in the same way for a pe-

riod, to allow time to decide upon the best plan of treatment to be pursued. But should excision be considered necessary, all the parts which have been covered by the glass should be removed, before the patient can be considered safe.—*Lond. Med. and Surg. Journ.*

*On the Nature of Continued Fever, and the changes which the Blood undergoes in the progress of that Disease.*—Dr. Reid Clanny, of Sunderland, has published a lecture which he delivered not long ago at the Sunderland Infirmary, on the composition of the blood in typhus fever, and on the light which his analytical researches throw on the nature or proximate cause of fever generally. The statements of facts contained therein is much too novel, and, if correct, important likewise, not to deserve all possible publicity. But they are brought forward by the author in a manner far too bare and authoritative, and are in themselves a great deal too extraordinary to allow of us placing much confidence in the results till they are supported by the details of his own experiments, and confirmed by the experience of others.

According to Dr. Clanny the watery part of the blood increases in proportion during the progress of continued fever, while the proportion of each of the solid parts diminishes; and when the crisis has taken place the opposite change commences, so that ere long the blood returns to its former condition. Dividing the period of an ordinary case of mild typhus, or (as we should advise him rather to call it) synochus fever, into three stages of six days each, the first being the stage of increase, the second that of formation, the third that of declension,—he says he has found from the average of many experiments the following to be the proportions of the chief principles in a thousand parts of blood at the close of each stage.

	In Health.	1st Stage.	2d Stage.	3d Stage.
Water, -	678	729	772	732
Colouring Princ.	160	136	122	130
Albumen, -	121	98	72	101
Fibrin, -	28	52	22	26
Salts, -	13	12	9	11

From this table it appears that all the animal principles, as well as the salts of the blood, decrease materially in quantity as the fever advances, and increase again as it recedes; and the author farther alleges that the same changes do not occur in other febrile disorders.

Another change which he says he has detected is a diminution in the quantity of carbonic acid contained in the blood. In health, blood contains, according to his experiments, a sixteenth of its volume of that gas. In the advanced stage of unfavourable cases of typhus it does not contain any; and in the intermediate periods the proportion is found to decrease gradually, but he does not mention in what ratio.

Dr. Clanny infers from these premises that contagious fever is in essence nothing else

than a stoppage of the process of sanguification; and he supports this view by some ingenious arguments drawn from the features of the fever itself, and from the phenomena of analogous diseases. We are not quite prepared to pronounce his premises false; but our own observations certainly lead us to suspect as much. With regard to the diminution of the carbonic acid in the blood during the progress of fever, we must recal the author's attention to the late explicit denial by Dr. John Davy of the statements of Mr. Brande, Dr. Scudamore, and others, as to the presence of *any* free carbonic acid in ordinary circumstances. Dr. Davy could not find it. We have repeated his experiments with the same result, and believe we have also discovered where Brande, Scudamore, and Dr. Clanny found *their* carbonic acid. But even although Dr. Clanny's analysis of the blood in fever should prove correct, that is merely a condition necessary to the existence,—and not evidence of the validity,—of his theory of the nature of fever; for in imputing the febrile symptoms to the absence of sanguification, he commits the very error with which he has charged such theorists as Clutterbuck and Broussais; namely, he mistakes the effect for the cause. If inflammation prevails during life in the head or abdominal viscera, and leaves traces of its ravages in the dead body, that is no proof of local inflammation being the cause of the general fever. In like manner, if a starving of the blood occurs during fever, all the functions of assimilation being suspended, that fact certainly is no proof of the starved blood being the cause and not the effect of the disease.—*Ed. Med. & Surg. Jour.*

*Puncture of the Pericardium.*—Dessault performed this operation about twenty years ago; the patient died, and upon dissection, it was ascertained that he had mistaken the nature of the disease. Since his time, Laennec and other authors have recommended the operation. At the present day, the precision of our means of diagnosis, enables us to recognise the cases in which an operation might be indicated, but I am not aware that it has been attempted by any surgeon since Dessault. The following case in which it was performed, is detailed in the *London and Paris Observer* for March 11th, 1827: I have not found it recorded in any of the English medical journals. The subject of the case, — Skinner, a girl æt. 14, residing in White street, Cartergate, had been attacked with rheumatism the preceding January. The surgeon in attendance, Mr. Jowett, of the parish of St. Mary, ascertained by means of the stethoscope, that the pericardium and inner membrane of the heart were inflamed. The remedies employed on this occasion relieved the disease, and the patient appeared to be convalescent. The stethoscope, however, together with other indications, evinced the existence of dropsy of the pericardium. On the 13th January, the state of the patient

changed greatly for the worse, and the day following it appeared impossible that she could survive the night, unless something were done for her relief. The operation having been proposed and assented to, was performed that afternoon in presence of Dr. Manson, who had been called in consultation. It was at first proposed to extract the fluid by means of a suitable apparatus, but an accidental circumstance occasioned it to be extravasated into the cavity of the pleura, where it was soon absorbed. In the first twelve hours after the operation, there was a sensible amendment, and although the patient was in a state of great debility and exhaustion, strong hopes were entertained of her ultimate recovery.—*M. de Fermon. Bull. des Sciences Médicales.*

*Employment of Stramonium in different Nervous Affections.*—Professor Wendt supposes that the action of stramonium, though in other respects sufficiently analogous to that of belladonna, is distinguished from it, in operating less upon the circulatory system, and in producing singular effects upon the organs of generation. Taken in a large dose, by an individual in good health, it excites the venereal appetite; while, given to a person labouring under this species of excitation, it soothes it by a homeopathic action. Cases of sur-excitation therefore, of the genital organs, are those in which it is particularly indicated; it is also useful, according to Professor Wendt, in all the phlegmasiæ accompanied with many nervous symptoms, in affections of the internal genital system of the female, in nymphomania, in epilepsy, occasioned by onanism or any irritation whatever of the generative organs, in myelitis, œsophagitis, and carditis. In all cases, the inflammatory diathesis, if present, should be removed, before resorting to the administration of the plant in question. Professor Wendt prefers it in the form of tincture, prepared as follows:—Two parts of the bruised seeds; malaga wine, eight parts; alcohol, one part; digest at a gentle heat, express, and filtre. The dose for an adult, is from five to twelve drops, every two hours.—*Rust's Magazine.*

*Instance of Obliteration of the Aorta opposite the Fourth Dorsal Vertebra.* By PROFESSOR MECKEL.—A peasant, aged 35 years, previously in good health, robust, and well-made, was, all at once, on the 18th January, seized with a sense of great debility, while carrying a sack of grain to market. He was carried to the hospital immediately. The symptoms of syncope and vertigo were dissipated in a few hours; to which succeeded gastric irritability; pain in the chest, total loss of appetite, bilious vomiting, the pulse remaining little altered. By the sixth day, the patient appeared to be completely cured—got up—and was walking about—but suddenly fell down dead.

*Dissection.*—On opening the thorax, the pericardium was observed to be filled with black blood, occasioned by rupture of the

right auricle, which was softened in its structure. The aorta ascendens was found to be too much dilated for injection from that point—and, therefore, ligatures were thrown on the left subclavian and carotid arteries, while the tube was fixed in the arteria innominata. The injection was considered to be unsuccessful, and as the subject had been designed for a demonstration, it was thrown aside. On opening the abdomen, afterwards, the vessels were seen injected, as were those of the lower extremities down to the feet. The examination being prosecuted, they found the aorta, immediately below the arterial ligament, reduced to the size of a crow-quill, while a beautiful net-work of vessels was seen between the trunks, going off from the arch of the aorta, and the intercostals of the aorta descendens. The said intercostals were very much enlarged, and had produced grooves in the ribs. From this circumstance it was inferred, that the obliteration of the aorta was an affection of long standing, and could not possibly have dated from the late attack of syncope, six days previously. The man must, therefore, have not only survived the cause of the obliteration, whatever it was, but lived in good health for many years afterwards. On inquiry, all that could be learnt, was, that this man had been very often ill in his youth; but afterwards had grown up strong and muscular.—*Journal Complémentaire.*

*M. Dupuytren's Treatment of Phagedenic and Corroding Herpes.*—There is not a physician who has not had an opportunity of observing and treating phagedenic or corroding herpes, and to experience a disagreeable proof of the inefficacy of the anti-herpatic, anti-scrofulous, anti-venereal remedies, and others which have been tried by turns against this cruel disease, according to its different appearances, and its supposed nature. We know, that in spite of all the remedies, the phagedenic herpes eats and destroys no less the nose, the lips, the cheeks, the eyelids, the ears, the temples; parts which it more especially and frequently attacks. Fire itself seems to irritate, as well as arsenical paste; these agents have besides the inconvenience of destroying the parts on which they are applied, and to add to their deformity. These motives have, for a long time, induced M. Dupuytren to seek other remedies against phagedenic herpes, and it seems certain, that they may be cured without deformity, by the use of the following powder:—

℞ Hydrarg. Submur. præcip. partes	199
Oxidi. Arsenici. Albi. vel }	partem - 1
Acidi. Arseniosi.	

200

This remedy, which acts rather as a specific than as a caustic, may be variously employed. If the surface of the herpes is ulcerated, moist and cleaned, it is powdered with a little puff, charged with the above described powder, so as to cover it with a thick layer, of about the

twentieth part of an inch. If this surface is covered with a scab, it must be thrown off by means of a poultice, and then it is powdered as has been just described. In fine, if the herpes is actually covered with an imperfect cicatrice, it must be destroyed; twenty-four hours after, the surface is powdered, when it must necessarily have ceased bleeding.—*Medical Guide to Paris.*

*M. Jadelot's Treatment of Croup.*—M. Jadelot considers the croup as a kind of angina of the air passage, presenting more violent symptoms, and having true paroxysms separated by well marked intermittents of special character. He admits different degrees in the disease according to its intensity, but without changing opinion as to its nature. Bleeding by leeches, and emetics, are the agents the most employed in the treatment of croup. The emetic alone has often sufficed to stop the disease, especially when it takes place in weak, pale, and bloated subjects; but in the opposite cases he insists on the application of leeches, and allows the blood to flow long enough for the infant to become pale, and the pulse to lose its strength. If the bleeding be too soon stopped, there is a danger of not arresting the progress of the evil, and a result, which is at least troublesome, is, that of being obliged to apply more leeches.

After the bleeding, M. Jadelot causes vomiting, several times in succession, at intervals of two or three hours, and the practice is attended by the greatest success, for the children find themselves relieved each time that they have vomited.

When the croup has arrived at the second period without having been opposed, and the presence of a false membrane is suspected, M. Jadelot directs leeches to be applied, but from the moment that they fall off he hastens to produce vomiting, and it is in this case that he employs by spoonfuls, every ten minutes or quarter of an hour, the mixture called *anti-croupal*\*, until he has obtained vomiting. He insists equally upon derivatives used upon the skin or in the intestinal canal; he advises also to provoke sneezing.

When the disease is very rapid, it has been a question whether we ought to commence by bleeding or emetic. M. Jadelot's opinion is, that we should bleed first if the infant be robust, and if it present signs of congestion towards the superior parts; on the contrary, he would commence by vomiting, when the subject is pale and exhausted, and there is little heat and fever.

*Symptomatic Diabetes Mellitus.*—It is an important fact in the pathology of this mysterious disease, that when the system is gradually sinking under phthisis, one of its common-

est terminations, or when life is cut short more abruptly by some other supervening disorder, the morbid secretion sometimes returns to its healthy state,—a proof that the elaboration of saccharine matter in the kidney is not owing to an organic change in structure, but simply to derangement of function. A new and singular fact of the same nature has been detailed by Dr. Bennewitz in Osann's Clinical Report for 1823-4-5; namely, the occurrence of diabetes mellitus in conjunction with pregnancy. The case is in many respects interesting. A stout young woman who previously had three children, and always carried her child to the full time without any material disturbance of her health, became pregnant for the fourth time. During the whole of that pregnancy she was tormented with insatiable thirst, and profuse discharge of urine; but as she had no other complaint of sufficient moment to attract her attention, she did not apply for advice; and the nature of the urine was never ascertained, as the quantity of liquid she drank was naturally thought to be a satisfactory explanation of the increased quantity of liquid discharged. The thirst and diuresis ceased suddenly soon after she was delivered, and she recovered perfectly. At the age of twenty-two she became pregnant for the fifth time; and hardly had the pregnancy begun when the thirst and diuresis reappeared even in a more tormenting degree than before; no other symptom of ill health, however, accompanied them, so that it was not till the seventh month that she applied for medical advice, and even then thirst was her chief complaint. The desire for drink, caused by a burning and itching sensation in the throat, was such that she drank daily five or six Berlin quarts; but her hunger was not preternatural. Her digestion at the same time was vigorous; and although she said she had been much stronger at the commencement of her pregnancy, she was still a stout looking woman. The urine considerably exceeded in quantity the liquid drank, amounting in fact to eighteen medicinal pounds; it was watery and muddy; had a faint smell like stale beer; and had a taste resembling that of beer, but much sweeter. The tongue was clean and dark-red; the mouth constantly dry; the gums shining, red, and retracted, so that the teeth were loose; the voice weak and hoarse; the bowels regular; her sleep disturbed by calls to drink; the skin rough, harsh, dry, never perspirable; the pulse full, hard, and frequent; the temperature of the body irregular;—and menstruation had continued during the whole period of her pregnancy. Latterly she had also pains in the loins, shooting towards the pubis, and particularly troublesome when she walked. No symptom whatever could be detected of a local affection of the kidneys. On account of the state of the pulse twelve ounces of blood were taken from a vein; but no change whatever was caused in the symptoms. The blood drawn formed an abundant dark-red crassamentum without sizyness, and a clear serum of a peculiar faintly-sweetish smell and taste. A strict animal diet and warm clothing were then

\* Anti-Croupal mixture—℞ Infus. Polygalæ ʒiv.; syr. ipecacuanah ʒj.; oxymel. scillæ ʒij.; antim. tart. gr. jss. misce.

enjoined, together with the occasional use of magnesia and hyoscyamus to keep the bowels moderately open; but although she in consequence seemed to feel more comfortable, the state of the urinary secretion remained unaltered. About this time it was analyzed by *Hermbsstaedt*, and found to contain *two ounces of saccharine matter per pound (civilpfund.)* At length the labour pains commenced prematurely, (the precise time not mentioned,) and she was delivered of a female infant weighing twelve pounds, and which died in the passages. Next day she was attacked with great weakness, tearing pain in the lower belly, so acute that she could not bear the pressure of the bed-clothes, delirium, flushing of the countenance; but as the lochia continued to flow naturally, she was not subjected to any particular treatment. Next day, however, the same symptoms continuing, leeches were applied to the abdomen, and a laxative administered. The operation of the latter was followed by profuse perspiration, the first she had had since her pregnancy began. The inflammatory symptoms then rapidly disappeared; at the same time the thirst, diuresis, and saccharine taste of the urine became less and less, and she was soon restored to perfect health. The urine was carefully analyzed again by *Hermbsstaedt*, (at what distance of time after delivery is not stated,) and he could not detect in it any trace of sugar. Six months after being dismissed cured, she became pregnant a sixth time. The relater did not see her during her pregnancy; but he was subsequently informed by her that she had the same thirst, heat in the throat, and diuresis, though in a much less degree than formerly; and that in addition she had a profuse *fluor albus*, which no treatment could check during her pregnancy, but which ceased of its own accord soon after delivery.—*Ossann's 12ter Jahresbericht des Poliklinischen Institutes zu Berlin*, p. 23.)—*Ed. Med. and Sur. Journal.*

*Acetate of Morphine.*—A letter by Dr. Marroli is inserted in the *Annali Universali di Medicina*, detailing a number of cases in which this preparation was successfully employed; in one instance, a stout and vigorous man had been attacked, on three succeeding years, at the same period, by a spasmodic pain about the middle of the humerus, unaccompanied by redness or tumefaction. The first attack yielded to a plaster, the composition of which was unknown, and the second to leeches, cupping, tartar emetic ointment, blister, &c. In the third, half a grain of the acetate mixed with sugar, and divided into two doses, was directed to be taken during the 24 hours. After the second dose, the pain disappeared entirely: A case is also detailed of a woman, who had been tormented for several days with lacerating pains in the left arm, extending to the corresponding mamma, and the sternum. She was unable to move her arm; there was continued fever, thirst, &c. V. S. thrice repeated, purgatives, frictions, with volatile liniment, and finally the acetate of morphine

was directed. The pains were greatly mitigated after the first dose, and almost entirely disappeared after the second. Exostosis of the sternum, revealed an inveterate syphilitic affection, the usual remedy was employed, the exostosis subsided, and the patient recovered the use of her arm as before.

We cannot afford room to all the cases of Dr. Marroli, which indeed do not possess a very high degree of interest, except, inasmuch as they tend to diffuse a knowledge of a remedy which has not hitherto attained the eminence it deserves. Several examples are given of its efficacy in spasmodic affections of the uterus, attended with fever, vomiting, &c. &c. We can only subjoin the following detail of a different disease.

A man, æt. 50, of a good constitution, was attacked with severe pain of the left side of the neck and cheek, accompanied with strong pulsations of the carotids, flushed countenance, anxiety, palpitations of the heart, pulse hard, vibrating, &c. V. S. was seven times repeated during six days, leeches repeatedly applied, purgatives, digitalis, oxide of bismuth, diaphoretic beverages, &c. were employed, and the patient appeared convalescent, when there suddenly supervened a rigour which lasted several hours, followed by anxiety, intense thirst, high febrile action, profuse perspirations, and violent pain in the neck. Six grains of the sulphate of quinine, made into eight pills, were directed to be taken, one every two hours; the disease yielded to this remedy, but re-appeared some time since, and besides the symptoms above enumerated, the patient complained of a pricking sensation at the spot where the leeches had been applied the preceding year. One grain of the acetate of morphine was divided into four parts, one to be taken every twelve hours; there was a sensible diminution of pain after the first dose, and it disappeared entirely after the second.

*Cancer of the Penis.*—At a meeting of the Institut Royal, M. Delpech announced that he had recently removed the penis in a case of cancer involving the whole of this organ. The disease penetrated beneath the symphysis of the pubis, and could not, therefore, be entirely removed by amputation, while on the other hand, it would have been very difficult to pass a ligature around the vessels under the arch. The extirpation was effected by M. Delpech, by making in the first instance an incision, which divided the scrotum into two parts, extending to the root of the penis; the deep seated parts were thus brought into view, and the operation completed. The division of the scrotum was afterwards maintained complete by means of sutures, which united the skin to the deep seated parts, leaving after cicatrization a profound channel, which afforded a ready passage to the urine, the want of which is the cause of so much inconvenience when the penis is amputated near its root.

In a note to the above, M. Gendrin observes, that the separate cicatrization of the

two portions of the divided scrotum has its advantages, and is an improvement due to M. Delpech, but the necessity of this division, in order to extirpate the roots of a cancer of the penis extending beneath the symphysis, was demonstrated by M. Dupuytren some years since, and has been twice performed by that celebrated surgeon.—*Jour. Général de Médecine, &c.*

*Metritis cured by the Hydriodate of Potash.*

—A woman, æt. 42, complained, two days after a difficult accouchement, of severe pain in the uterus, extraordinary sensibility of the abdomen, and frequent desire of urination. The uterus was still considerably distended, the vagina hot and dry, the lochial discharge very small in quantity, fever not very high; the secretion of milk had already begun. The bowels were evacuated by an injection, and M. Guerard prescribed three grains of the hydriodate of potash in six ounces of emulsion, to be taken during the 24 hours. The following morning, the uterus had contracted, the pains disappeared, the lochia were re-established, and the patient appeared so well that the medicine was discontinued. The iodine had no influence upon the lacteal secretion, nor upon the health of the child.

*Case 2d.*—Madame F. after much suffering, was delivered by the forceps in her first accouchement. The second day, inflammation of the uterus supervened, it had only slightly contracted, and was very tender to the touch; there was no lochial discharge; the other functions were scarcely disturbed. Dr. G. prescribed the hydriodate of potash as in the preceding case, and with similar success, for after the lapse of twenty-four hours the patient was convalescent.—*Horn's Archives.*

*Paralysis from Cubebs.*—Mr. Broughton has related a curious case of this kind, in the person of a young gentleman, who had been taking the above mentioned medicine for about a fortnight, in the dose of two drachms thrice daily, for a gonorrhœa, and being otherwise in good health, and living quietly. A distortion of the mouth to one side, whenever he attempted to speak or smile, supervened—and the pulse became irregular, with some other symptoms of constitutional derangement. He was bled to half a pint, without any advantage. Then he was freely purged, with benefit, but at the end of three weeks the distortion had not entirely disappeared. Was this not an affection of the portio dura, and the result of cold caught in the side of the head, rather than the effect of cubebs?—*Med. Gaz.*

*Employment of Pyrothionide\* in Angina*

\* "This is the name given by Dr. Ranque, chief physician to the Hôtel Dieu, at Orleans, to a product obtained by the combustion of hempen, linen, or cotton cloth. As it is coming into notice for chilblains, chronic inflam-

and other Inflammations.—Dr. Ranque has made a happy application of this substance to the treatment of Angina, whether simple, or complicated with scarlatina or measles. In simple angina, the resolution of the inflammation has been rapid; it having been sufficient to employ, ten or twelve times a day, a gargle consisting of two grains of pyrothionide to the ounce of barley water, and a little honey. In violent angina, accompanied with scarlatina, he did not venture in the first instance, to confine himself to this gargle alone, but employed concurrently, leeches to the neck and epigastrium, and then the disease was limited to the cutaneous eruption. But subsequently, emboldened by success, he employed only the gargle and a rigid diet, and in a few days, traces of the angina were scarcely perceptible. Reflecting upon its salutary effects in these different cases, he was induced to make trial of it in epidemic angina maligna, and pretends that he has found it equally successful in the treatment of this terrible disease. According to him, pyrothionide in union with barley water and honey, is one of the best resolvents of inflammation, and one of the best solvents of the effused coagulable lymph. He relates three cases of ophthalmia treated successfully by this means, and cites some instances of its employment in syphilitic gonorrhœa, simple or complicated, and in leucorrhœa. The mode of administration consists in dissolving 24 grains of concrete pyrothionide (it is obtained in this form by evaporating the aqueous solution at a gentle heat; it has a resinous aroma, is not acid, is agreeable to the palate, and leaves a sensation of cold on the surface to which it has been applied, after producing a slight excitation,) in six ounces of water, and using it as an injection, three or four times daily, while at the same time, it is employed in form of fomentations. We do not believe that epidemic angina maligna has been so easily cured, as is pretended by Dr. Ranque, although we are willing to attribute some remedial agency to the pyrothionide. What is this substance? evidently simple pyroligneous

mations of the eyes, we give the directions for its preparation. Take a handful of hempen, linen, or cotton cloth, either old or new. Place it in a shallow basin, and set fire to it in the open air. As the combustion proceeds, prevent the basin from heating too much. When it has ceased, reject the carbonaceous residue, and there will be found a semi-aqueous, semi-oily product, of a reddish and brownish tint, and possessing a penetrating but not disagreeable odour. Pour upon this substance a glass of cold water, and spread the water by means of a bottle brush, over those parts of the basin where any of the oil may be formed, with a view to its complete solution. In this manner, a liquid is obtained of a tint more or less deep, according to the quantity of the substance, produced by the combustion and dissolved in water."—*North American Med. and Sur. Jour. from the Nouv. Biblioth. Med.*

acid, notwithstanding all its inventor may say to the contrary; and as the utility of this acid has been recognised in many of these circumstances, it is not to be wondered at, that the use of the pyrothonide has been sometimes followed by salutary consequences.—*Archives Générales de Médecine.*

*Frontal Neuralgia, cured by the Extract of Belladonna.* By Dr. LECLERCQ.—A gardener, æt. 27, possessing a strong constitution and sanguineous temperament, had been habitually obliged to submit to venesection several times a year, for the relief of violent pains in the head, to which he had been subject. Dec. 14, 1827, after having worked for a long time in the cold, he was attacked with acute pain in the right frontal region, and the eye of the same side. The pain, which returned about five o'clock every morning, and did not cease till about nine or ten, commenced with a pricking sensation, followed by shooting pains; the eye reddened, became suffused with tears, and was unable to support the light, the eyelids were contracted, and the patient suffered very severely. The pulse was unaffected, and all the functions were performed regularly. About 8 or 9 o'clock, the pain gradually abated, and soon disappeared entirely; leaving a slight heaviness of head, and a kind of stupor. The plethoric condition of the patient, appeared to demand V. S. which was unattended with any advantage. The periodical character of the disease led to the employment of the sulphate of quinine, which was given at first to the extent of fifteen grains, and soon increased to twenty, without any other effect than slightly to retard the accession of the paroxysm. Recollecting to have read in the *Journal Général de Médecine*, a case of sub-orbital neuralgia, cured by M. Lisfranc, by means of frictions around the orbit, with an aqueous solution of belladonna, Dr. L. determined to have recourse to the same remedy. A cloth moistened in a solution, consisting of one drachm of the extract of belladonna, in an ounce of lettuce water, was repeatedly applied to the seat of pain. The first day, no alleviation was perceptible; the second, a sensation of numbness throughout the whole frontal region, &c. was substituted for the pain; and the third day, the cure was complete. The belladonna was continued two days after the cessation of the pain. During eight or nine days, the patient complained that he could only distinguish objects confusedly with the right eye. More than three months had elapsed when the preceding account was drawn up, and there had yet been no return of the disease. This case is interesting; 1st, because it is an additional proof of the efficacy of belladonna in the treatment of facial neuralgias; 2d, because it tends to prove the special action of belladonna upon the fifth pair of nerves, an opinion entertained by some physicians; 3d, because it confirms the statement of MM. Chomel, Cloquet and Bricheteau, who have asserted that they have seen the employment of belladonna followed by tempora-

ry blindness; in the case related vision was affected. From the cases cited by these physicians, it might perhaps be inferred, that belladonna should not be employed for a long period, nor without great prudence, in diseases of the eyes and neuralgias of the third pair, but these apprehensions vanish, when we recollect the success with which M. Demours has employed, for two or three years in succession, the extract of this plant, in order to dilate the pupil, and thus facilitate vision, in cases of blindness arising from opacity of the centre of the cornea or of the crystalline.—*Archives Générales de Médecine.*

*Innocuous Nature of Putrid Exhalations.*—A committee have been engaged in France in examining the circumstances relative to the knacker's operations. His business consists in killing old worn-out horses, and turning every part of their body to account. The most singular results which the committee have obtained relate to the innocuous nature of the exhalations arising from the putrefying matter; every body examined agreed that they were offensive and disgusting, but no one that they were unwholesome; on the contrary, they appeared to conduce to health. All the men, women, and children concerned in the works of this kind had unvarying health, and were remarkably well in appearance, and strong in body. The workmen commonly attained an old age, and were generally free from the usual infirmities which accompany it. Sixty, seventy, and even eighty, were common ages. Persons who live close to the places, or go there daily, share these advantages with the workmen. During the time that an epidemic fever was in full force at two neighbouring places, not one of the workmen in the establishment at Montfaucon was affected by it. It did not appear that it was only the men who were habituated to the works that were thus favoured: for when, from press of business, new workmen were taken on, they did not suffer in health from the exhalations.

In confirmation of the above observations similar cases are quoted: above 200 exhumations are made yearly at Paris, about three or four months after death; not a single case of injury to the workmen has been observed. M. Labarraque has observed, that the catgut makers, who live in a continually putrid atmosphere, arising from macerating intestines, enjoy remarkable health. Similar circumstances were remarked at the exhumations of the *Cimetière des Innocens*.

Whatever disease the horse may have died of, or been killed for, the workmen have no fear, adopt no precautions, and run no risk. Sometimes, when strangers are present, they pretend to be careful, but, upon close inquiry, laugh at such notions. They handle diseased as well as healthy parts always with impunity. They frequently cut themselves, but the wounds heal with the greatest facility, and their best remedy is to put a slice of the flesh about the wound.

On making inquiry of those to whom the horse-skins were sent, and who, besides, having to handle them when very putrescent, were more exposed to effects from diseases in the skin, they learnt that these men, also, from experience, had no fear, and never suffered injury. Horse-skins never occasioned injury to those who worked them, but in this they differed from the skins of oxen, cows, and especially sheep, which sometimes did occasion injury, though not so often as usually supposed. —*Recueil Industriel*, v. 55.

*Analysis of a specimen of Cutaneous Perspiration.* By J. BOSTOCK, M. D. F. R. S.—Dr. Bright sent to Dr. Bostock, for analysis, about four ounces of fluid, being the cutaneous perspiration of a patient of his at Guy's Hospital. By various computations (for an account of which we must refer to the original paper,) the following was obtained as the result:—

Water	981.7
Animal matter	4.6
Muriate of soda	12.56
Soda	1.14
Phosphates and sulphates	a trace

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1000.00

The animal matter was found partly soluble, and partly insoluble, in alcohol. The alcohol being evaporated, afforded a residuum manifesting a certain resemblance to urea, being apparently intermediate in character between this substance and osmazome. The part which was insoluble in the alcohol, resembled most nearly "the substance which forms the principal ingredient of the serosity of the blood." There was a very minute and scarcely appreciable portion of albumen, but no jelly.

It appears that the patient, from whom the perspired fluid was obtained so largely, was a robust sailor, aged 64; he had formerly suffered from gravel, and had slept in damp sheets six days before his admission. Shivering, eructation, vomiting, pain in the belly, and constipation, followed. These symptoms were relieved, after a short time; when he complained of occasional griping pains, and his stools became deficient in bile; his urine pale, and much increased in quantity. After two days more he had pain round the umbilicus and over the pubes, particularly on pressure, or voiding his urine, the quantity of which now amounted to ten pints in twelve hours. Some dysenteric symptoms next showed themselves, for which he took ipecacuanha and hydrar. c. creta. His mouth soon became affected, and the state of his bowels improved; but the quantity of urine continued very large. He was ordered to go into the warm bath twice in the week, and this was followed by perspiration so copious, that it was observed "running completely through the bedding, and forming streams upon the floor." He gained strength notwithstanding, and the urine diminished in quantity, and the patient appears to have got well. —*Lon. Med. Ch. Transactions*.

*Analysis of the Cephalo-spinal Fluid.* By

M. LASSAIGNE.—In one of the numbers of this journal, published last year, we gave the results obtained from an analysis of the fluid, contained in the spinal canal of the horse. It was shown that this fluid, which was rather denser than water, contained, independently of the alkaline salts which are found in ordinary serum, very minute quantities of albumen and osmazome.

Desirous of ascertaining the composition of this fluid in the human species, M. Magendie sent me, for analysis, a quantity which he had extracted from the body of an old woman, who had been ill several years, and ultimately died of insanity.

Its specific gravity at the temperature of  $+10^{\circ}6$  was 1.0082, and 100 parts furnished water 98.564, osmazome, 0.474, albumen, 0.088, soda, animal matter and phosphate of soda, 0.036, chlorurets of sodium and potassium, 0.801, phosphate of lime, 0.017.

At the request of M. Magendie I compared this fluid with that found in the cerebral ventricles of a man, who, for the last two years, had been affected with general paralysis, and died insane at the Maison Royale de Charenton. Its specific gravity at  $+8^{\circ}5$ , was 1.0086, and contained in 100 parts, water, 98.738, osmazome, 0.444, chloruret of sodium and potassium, 0.713, soda, animal matter, and phosphate of soda, 0.058, albumen, 0.047. —*Journal de Chimie Médicale, &c.*

*Chemical Examination of the contents of a sebaceous tumour, forming an atheromatous cyst.* By Professor NEES d'ESSENBECK, jr.—The tumour was situated on the great pectoral muscle of a male subject, and equalled in size a large nut; its parietes were composed of three lamina, the most internal, had a horny or epidemic character; the middle, about half a line in thickness, resembled the mucous membranes; condensed cellular tissue formed the external envelope.

The contents of the cyst weighed, when dried, 95 grains, and were composed of pure stearine, 23 grs.; osmozome, 12 grs.; animal mucus, 11 grs.; dry albumen, 23 grs.; phosphate of lime, 19 grs.; carbonate of lime, 2 grs.; carbonate of magnesia,  $1\frac{1}{2}$  grs.; traces of elain and of acetate of soda, and loss  $3\frac{1}{2}$  grs.

The contents of the cyst may, therefore, be regarded as a kind of emulsion formed by the union of the fat and albumen with the earthy salts. —*Bull. des Sciences Médicales*.

*Termination of the Umbilical Vein in the right auricle, and a single umbilical artery arising from the abdominal aorta, in a new-born infant.* By Professor MENDE.—The child, in whom these remarkable deviations from the natural state were observed, died shortly after birth, without any apparent cause.—There was nothing unusual on the exterior of the body; but, on injecting the vessels, it was found that the umbilical vein did not divide into two branches to traverse the liver, but was continued as a single trunk along the convex surface of the right lobe of this organ,

without being attached to it, as far as the right auricle of the heart, when it opened itself a little above, and in front of the termination of the inferior vena cava. The base of the heart was inclined a little more than natural to the right side, and towards the sternum. There was only one umbilical artery, which arose from the abdominal aorta at its bifurcation; it passed on the left side of the bladder, and continued its course to the umbilicus.

Professor Mende draws from this case some physiological conclusions, relative to the uses which the liver serves in the fœtus. He considers that this organ does not receive all the blood which comes from the placenta; the comparison which has been instituted between the human fœtus and amphibious animals, especially the seal, being incorrect; and that the blood coming from the placenta is not modified by the liver, and does not serve for the secretion of bile, the gall-bladder being, in the case described above, full, and the fœtus well-nourished.

*On the Vagitus Uterinus.* [Revue Médicale Février 1828.]—Although many German writers on obstetrics and medical jurisprudence believe firmly that the fœtus may be sometimes heard to cry in the uterus, and the fact is so generally admitted in the German schools, that several medical gentlemen who have visited this country have stated to us that they have themselves heard it, and all laugh at the idea of its being doubted,—the accuracy of the fact nevertheless continues to be questioned both in Britain and in France, more particularly, however, by the French accoucheurs. M. Lesauvage of Caen, well known to toxicologists, as having some years ago settled the question regarding the alleged poisonous properties of pounded glass, has declared his dissent from the general disbelief of his countrymen in the *vagitus uterinus*, and transmits as his reason the following incident to the Parisian Society of Medicine. A bitch fell sick when far advanced in pregnancy. On approaching her, there was heard distinctly, and even at the distance of ten paces, the cries of her pups, whose movements could be also seen through the abdominal parietes. She did not bring forth her young till two days after, “so that the *vagitus* in this case necessarily supposes the spontaneous development of a gas in the amniotic fluid of each fœtus.”—*Edin. Med. and Sur. Jour.*

*Method of discovering Potassa by the Blow-pipe Flame.*—M. Harkort, of Freyberg, says, that, in consequence of an observation made by Kirwan, namely, that oxide of nickel with potash, gave a blue glass before the blow-pipe, whilst soda with the same oxide produced a brown glass, he was led to examine whether the distinction might not be made to afford a useful test. On making the experiment with potash, he obtained an excellent result; the blue produced is not likely to be confounded with that produced by cobalt, because it inclines to a milky appearance. So sensible is

this test, that the presence of potash was readily discovered in the periclinité, (a new variety of felspar, distinguished by professor Breithaupt,) although existing there in a very small quantity. The experiment relative to soda was not so successful, the glass acquiring only a weak brown colour.—*Jahr. der Chem.* 1827.

*Effect of Electricity on pointed leaves, &c. and on Vegetation.*—For the double purpose of ascertaining the power of spines and sharp-pointed lanceolated leaves in modifying the electric relation of the atmosphere and the earth, and in affecting the progress of vegetation by their electric influence, M. Astier insulated a sextuple spine of the *Gleditzia triacanthos* at the top of his house, and brought a wire down from it to an insulated flower-pot, in which were planted five grains of maize; a similar sowing was made in an uninsulated pot, for the purpose of comparison. The experiment continued from the 6th to the 20th of June, including two stormy days. The electrometer gave considerable signs of electricity in the flower-pot, and by using the condenser sparks were produced. The electrified grains were found to pass more rapidly through the first periods of vegetation. When Bengal rose-trees were submitted to the same experiment, the flowers of the electrified plant appeared more rapidly and more abundant than in the other case.—*Ann. Linn. de Paris.*

*Preservation of Leeches.*—A new vessel of deal large enough to contain sufficient water for five hundred leeches, is to be furnished with a stop-cock to draw off the water. It is to be half filled with the mud from the lake or pond whence the leeches have been taken, and two or three roots of the Florence Iris, (*Calamus Aromaticus*) are to be set in the mud. The leeches like this plant. The usual precautions as to temperature, frequent change of water, &c. are to be taken; the water is to be changed slowly, and the fresh water added by means of a funnel descending to the bottom of the vessel. This method has been found preferable to all others tried at the hospital of Bamberg.—*Bull. Univ.* cxiii. 369.

*Change of Crystalline State in a solid Body.*—“It was in the sulphate of magnesia that I first remarked the change, in form, of a solid body, or, more accurately, the change in the position of its atoms, without the assumption of the liquid state. If this salt, or the sulphate of zinc, be slowly heated in alcohol, and gradually raised to ebullition, the crystals will lose their transparency by degrees, and, when broken, they will be found to be formed of a great number of new crystals, entirely different in their form to those of the salt employed.”—Mitscherlich, *Annales de Chimie*, xxxvii. 206.

This is a case of internal motion, to be added to those already known of basalt, arsenious acid, barley-sugar, sulphur, &c. &c.—*Quarterly Journal, &c.*

*Singular action of Arsenic Acid on Sugar.*—When a solution of pure arsenic acid is mixed with sugar, and left for a few hours, a rose colour is produced, which soon becomes a fine purple, and then remains, with little further change, for many days. Sugar of milk, mannite, raisin sugar, sugar of starch, produce similar effects; but sugar of liquorice, diabetic sugar, and such bodies as starch, gum, &c. produce no effect of the kind. Nor do the soluble arseniates or arsenious acid produce these effects with the substances named above.—*Bull. Univ.* A. ix. 281.

*The proportion of Morphine contained in Opium.*—The actual proportion of morphine contained in opium is not generally known to the profession; and chemists have been very silent upon this subject. But M. Pelletier let out the secret, a few months ago, at the Académie Royale de Médecine. He says, that five hundred grammes\* of opium will yield from thirty-six to forty grammes of morphia.

*The proportion of Emetine contained in Ipecacuanha.*—M. Pelletier informed the Académie, also, of the usual proportion of emetine which ipecacuanha contains. According to him, one livre† of ipecacuanha will yield from twenty-five to thirty grains‡ of emetine.

*Vaccination in France.*—During the year 1825, there were born in France 587,948 children, of whom 378,500 were vaccinated; 26,571 had the small-pox; of these 2,245 were either disfigured or debilitated by the disease, and 3,369 died of it. The sum of 31,305 francs was expended in the vaccination of about two thirds of the children born in this year.

In the kingdom of Naples, the number of births, for 1824, was 231,936; vaccinations 67,974.

The Russians have recently introduced this salutary practice into California. The chief surgeon, Novitzky, has vaccinated a great number of children, and, also, Governor De Monterey, and his family. In 1823, vaccination was introduced into the Aleutian isles.—*Bull. des Sciences Médicales.*

*Method of obtaining the Figure of a Plant.*—A piece of paper is to be rubbed over with powdered dragon's blood, in the manner practised by engravers, and then the small branch or leaf, of which the design is required, is to be laid upon it: by means of slight friction, it soon takes up a small quantity of the powder, and being then laid upon moistened paper, an impression is to be taken in the manner practised for lithography without a machine. This process may be usefully employed for preserving certain physiognomical and characteristic

features, which cannot be retained by drying the plant.—*Bull. Univ.*

*Compressibility of Water.*—Oersted finds, in conformity with the previous experiments of Canton, that water is more compressible at the freezing point than at a higher temperature. At 32 degrees Fahrenheit the compressibility of water is about a tenth greater than 34½ degrees Fahrenheit. At higher temperatures it is still less, but not in so high a proportion.

## New Publications.

*Deafness; its Causes, Prevention, and Cure.* By John Stevenson, Esq. Member of the Royal College of Surgeons, Lecturer on the Structure, Economy, and Diseases of the Eye and Ear; and Surgeon-Oculist and Aurist Extraordinary to his Royal Highness the Duke of Clarence. 8vo. pp. 262. Henry Colburn, London, 1828.

*A Rational Exposition of the Physical Signs of the Diseases of the Lungs and Pleura; Illustrating their Pathology, and facilitating their Diagnosis.* By Charles J. B. Williams, M.D. 8vo. pp. 191. Underwood, London, 1828.

Dr. Williams has treated his subject in a very able manner. The work is well deserving of perusal.

*Traité des Maladies des enfans nouveaux nés et à la Mamelle, fondé sur de nouvelles observations cliniques et d'anatomie pathologique, faite à l'hôpital des enfans trouvés de Paris, dans le service de M. Baron, par C. Billard, ancien interne de cet hôpital, docteur en Médecine de la Faculté de Paris, &c. un fort vol. 8vo.*

*Atlas d'Anatomie pathologique pour servir à l'histoire des maladies des enfans, par C. Billard, gr. in 4to. de 10 planches, avec le texte explicatif.*

*Analyse détaillée de l'histoire de la santé, des influences qui la modifient, et des conséquences positives d'hygiène qui en découlent; par P. N. Gerdy, professeur d'anatomie, de physiologie, agrégé à la Faculté de Médecine de Paris, chirurgien du Bureau central des hôpitaux, &c.*

*Réflexions sur la vaccine et la variole, ayant pour but d'obtenir, par la vaccination, l'extinction complète de la petite-verole; par J. A. Brisset, D.M.P. Paris, un vol. 8vo.*

*Synopsis of Nosology.* By W. Cullen, M.D. to which is added, the classification of Diseases by Dr. John Mason Good. 18mo. pp. 151.

*Medical Guide to Paris, &c. Translated from the French of M. Ratier, by J. R. Alceek. 18mo. pp. 141.*

*On the Origin and Progress of various changes of Structure in Man and some inferior animals.* By Dr. J. Baron. 4to. with plates.

\* A gramme is about 15 grains troy.

† A livre is equal to 15 ounces and 6 drachms troy, and a grain, French, is equal to 4-5 of a grain troy.

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**PATHOLOGICAL AND SURGICAL OBSERVATIONS RELATING TO INJURIES OF THE BRAIN.** By B. C. BRODIE, F.R.S. and Surgeon to St. George's Hospital.

PART I.

*Read 26th Feb., 11th and 25th March, 1828.*

SECT. 1.

It is my intention in the following pages to lay before the Society some observations relating to injuries of the brain, and the treatment which these injuries require. If any apology be necessary for this undertaking, I may remark, that I have been led to it by the great importance of the subject, and also by this consideration, that although much information may lie, as it were, scattered throughout the mass of surgical literature, no practical writer, as far as I know, has attempted to make such a collection and arrangement of facts as will enable the surgical student to take a distinct and connected view of all the parts of this curious and interesting inquiry. The present paper contains only a part of the observations which I have to offer, namely, those which relate to the first or immediate consequences of the injury. Should these be favourably received, I shall venture, on a future occasion, to communicate to the Society a second series of observations relating to those more remote consequences which are connected with inflammation of the brain and its membranes, or which arise after inflammation has subsided.

**SECT. 2.—Immediate Effects of Injuries of the Head as indicated by Dissection.**

In treating of injuries of the brain, of course I mean to include the consideration, not only of those by which the brain is affected in a direct, but also of those by which it is affected in an indirect, manner. Wounds and contusions of the external parts of the head demand our attention, inasmuch as they may be, and not unfrequently are, followed by disease of the more important parts contained within. Among the effects produced we are to distinguish those which are the immediate result of the injury, and those which are to be attribut-

ed to inflammation and its consequences. In the former we are still further to distinguish the actual derangement or destruction of the natural organization, such as it is disclosed by dissection, and the symptoms produced during the life of the patient by the disturbance of the functions of the injured organ; attempting at the same time to view these two orders of facts in connexion with each other, as the method by which, on this as well as on other occasions, we may be the best enabled to found the practical art of surgery on the basis of a scientific pathology.

The appearances which are observable on dissection in a person, who dies soon after an injury of the head, are very various, and may be variously complicated; but they admit of being classed under the following heads:

1. There may be simple contusion of the scalp with extravasation of blood between it and the tendon of the occipito-frontalis muscle, or between the latter and the pericranium, or between the pericranium and the bone; concerning which it is scarcely necessary to repeat the observation of Mr. Pott as to the close resemblance of the impression which is given to the fingers by the margin of the mass of extravasated blood, and that of depressed bone.

2. The scalp may be lacerated so as to expose the surface of the pericranium, or the pericranium itself may be torn off with it so as to expose the surface of the bone. Of these injuries, however slight may be the apparent difference between them, the latter is, as I shall show hereafter, likely to produce much more serious consequences than the former.

3. If a blow be inflicted on the head of the dead subject, the small vessels which connect the dura mater to the inside of the bone, at the part where the blow is inflicted, become ruptured; and in consequence the dura mater is separated from the bone to a greater or less extent. This, which happens in the dead body, may happen in the living body also, and is not an unfrequent consequence of an injury of the head. The separation of the dura mater is sometimes very extensive. A boy twelve years of age, fell from a height of fifty feet, and struck his forehead against the ground. He was admitted into St. George's Hospital in a state of stupor, in which he lay

for three days, when he died. On dissection, besides a large extravasation of blood on the inferior surface of the brain, the dura mater was found to have lost its adhesion to the bone everywhere, except in the basis of the cranium, and the external surface of that membrane had a brown and sloughy appearance.

4. The cranium may be fractured in all varieties of ways, from the most simple fissure to the most complicated fracture accompanied with depression and extending in a number of directions. A fracture in most instances takes place in the upper part of the cranium. Fractures of the basis are always the consequence of very severe contusion, and recoveries from these accidents are comparatively rare, not because a fracture of the basis is in itself more dangerous than a fracture elsewhere, but because it is almost invariably complicated with extensive injury of other and more important parts.

A fracture generally occurs in that part of the cranium on which the blow has been inflicted. But we find that in cases of fracture of other bones, the fracture is often situated at some distance from the part which is immediately exposed to the shock of the injury, as when the fibula is broken a little above the outer ankle in consequence of the foot having been twisted outwards, or the ribs are broken in the side in consequence of a blow on the sternum; and some French writers have supposed that fractures of the cranium occur in the same manner, being produced by what they have denominated the *contre-coup*.

It has been observed to me, however, by Mr. Earle, that he has not known a fracture of this kind to take place except where the blow seems to have operated in such a manner as to impel the occiput forcibly against the atlas, the line of fracture passing through the former bone, where it rests on the latter. My own experience corresponds very nearly with that of Mr. Earle. The only well marked cases of fracture of the cranium, in which the fracture could be attributed to the effects of the *contre-coup*, which have fallen under my own observation, were similar to those which he has mentioned. I do not, however, mean to assert, that such fractures absolutely never occur independent of the re-action of the atlas. Among the cases recorded in the Prize Memoirs of the French Academy of Surgery there are some which show that the thing does happen,\* and Mr. Bell has offered

an ingenious and scientific explanation of the mode in which it happens. It is, however, worthy of remark, that the only two cases which Mr. Bell has adduced in illustration of what he has advanced, are those in which the fracture extended across the occiput, in one case passing through, and in the other case passing close to, the foramen magnum of that bone.

In all cases of fracture of the cranium, with depression of bone, it is of importance to observe that the division of the inner does not correspond to that of the outer table of the skull, the former being always broken to a greater extent than the latter. In consequence of this the actual depression is greater than it would appear to be from the mere inspection of the external fracture.

I have seen a case in which there was a fracture with distinct depression of the inner table, while there was a simple fissure which was scarcely perceptible, and that without the smallest depression, of the outer table. But more remarkable instances of the kind are recorded by authors. M. Saucerote,\* in the Prize Memoirs of the French Academy, quotes a case from Tulpus, in which there were extensive fissures of the inner table of the skull, although the outer table remained uninjured; and another from Parey, in which, while the outer table was entire, the inner table was broken into splinters, some of which were actually driven into the substance of the brain. Dr. Hennen also in his Treatise on Military Surgery,† gives an account of a case similar to the last, in which the inner table was splintered, and at one part driven more than half an inch into the membranes of the brain, although there was not even a fissure of the outer table. The greater elasticity of the outer table of the skull, and the greater brittleness of the inner table seem to afford the only reasonable solution of these phenomena.

5. In young children we sometimes find the cranium depressed or indented after a blow on the head, and in the course of a few days restored to its natural level without the aid of a surgeon. I suppose that in these cases the earthy part of the bone has given way, while the animal part has remained entire, so that there has not been a complete fracture or actual solution of continuity, and that the pulsations of the brain constantly operating against the inner surface of the bone have been the means of elevating the depression. I have had no opportunity of verifying or contradicting this opinion by dissection, but it corresponds to what we know to happen in cases of injury done to other bones during the period of childhood.

6. The disjunction of the sutures is much more rare than fractures of the cranium. It is evident that this cannot happen except in

\* For example, M. Saucerote quotes from *Joan. Bonhius de Renunt. Vuknerum* the following history. A man died after having received a blow above the right eyebrow. On dissection it was ascertained that there was no fracture in the part which had been struck, but in the right orbit there was a fracture half an inch in length, extending towards the *Sella Turcica* of the sphenoid bone. But with respect to many other cases which are mentioned in the same memoir, it may be said that there is no sufficient evidence that the fracture which was attributed to the counter-

stroke, did not really arise from a second blow on another part of the head.

\* Vol. IV. 8vo. edition, 1819, p. 322.

† P. 323, second edition.

those who are not much advanced in life, and in whom the sutures are not completely consolidated. Such a case is always to be regarded as one of peculiar danger, not so much because the disjunction of the sutures is in itself likely to lead to bad consequences, but because the force necessary to produce it is so great, that it is also likely to produce extensive and serious injury of other parts.

7. Extravasations of blood within the cranium, in consequence of a blow on the head, occur in various situations: 1st, between the bone and dura mater, and here the extravasation may arise from a rupture of the small blood-vessels by which the dura mater is connected to the bone, or from a laceration of the trunk or branches of the middle meningeal artery. There is however never any considerable hemorrhage from the former source. At least, all the experience which I have had on the subject tends to confirm the opinion advanced long ago by Mr. Abernethy, that blood is never poured out in such quantity as to produce a dangerous pressure on the brain, except where the middle meningeal artery has been lacerated, and from this vessel the hemorrhage is sometimes very copious. I do not recollect to have seen it lacerated, except in combination with fracture running across the bony canal in which it is lodged; cases are however recorded by authors, in which the artery has been opened into, and bleeding has taken place from it, independently of fracture.\* 2dly, there may be extravasations of blood within the dura mater, and here the blood is generally collected between the dura mater and the tunica arachnoides. Sometimes, however, but rarely, the blood occupies the ventricles; at other times we find it extravasated in the substance of the brain, or in the cells of the cellular texture by which the tunica arachnoides and pia mater are connected with each other. Large extravasations are sometimes found on the upper surface of the brain, but more frequently on its basis. In the latter situation, the hemorrhage is usually the consequence of a rupture of the substance of the brain. As a blow on the abdomen may lacerate the substance of the liver or spleen, and occasion hemorrhage into the peritoneal cavity, so may a blow on the head cause a rupture of the tender substance of the cerebrum or cerebellum, and hemorrhage into the cavity of the dura mater. These cases generally afford examples of the *contre-coup*. The rupture of the brain rarely takes place at the exact spot at which the blow is inflicted; and the great irregularities which exist on the inner surface of the basis of the cranium, sufficiently explain wherefore the inferior is more liable to be ruptured than the superior surface of the brain.

Wounds of the sinuses sometimes bleed profusely where there is a free opening in the bone made by accident or operation, through

which the blood can readily escape. But a very slight pressure is adequate to the suppression of this as well as of other venous hemorrhage; and I have never known an instance in which there was such a collection of blood as was capable of interfering with the functions of the brain, between the dura mater and the bone, or between the dura mater and the brain, in consequence of a wounded sinus. There is often a considerable effusion of blood from the ear, especially in cases of fracture of the basis of the cranium. This may, as far as I know, sometimes arise from other sources; but it seems probable that it must, in most instances, arise from the laceration of the lateral sinus, where it extends downwards behind the petrous process of the temporal bone and the external meatus; and in one instance I ascertained it to have been so by the examination of the body after death. In another case which fell under my observation, there was hemorrhage from both the ear and the nostrils. The patient, a boy, died shortly after the accident; and it was found on dissection that there was a fracture of the base of the cranium, with laceration of the cavernous sinus, and that the hemorrhage had taken place from this sinus.

7. There may be all descriptions of wounds of the brain and its membranes, punctured, incised, and lacerated, with or without loss of substance; and with these, the effects of contusion which have been already enumerated, may be variously combined.

### SECT. 3.—*Concussion of the Brain.*

It is evident that many of those consequences of an injury of the head which are disclosed to us by dissection, are not likely to be marked by any peculiar symptoms in the living person, at least not previous to the access of inflammation. Wounds and lacerations of the brain, and compression of the brain, whether it arise from extravasated blood or a depression of bone, may impair or destroy the functions of that organ; but neither simple fissures of the cranium, nor disjunction of the sutures, nor separation of the pericranium or dura mater, are in themselves adequate to produce such effects in the first instance, although they may lay the foundation of serious disease afterwards.

But it has been long since established by the investigations of surgeons, that another cause, besides those which are rendered manifest by dissection, may be concerned in producing the symptoms which immediately follow a contusion of the head. A man receives a blow on the head; he becomes insensible, and continues so for a few minutes or for several hours. He dies, in consequence of this or some other injury; and on examination after death, the brain and its coverings appear to be perfect in all their parts; so that the most accurate anatomist can discover nothing different from the natural appearance of these organs. Opportunities of verifying this observation occur more or less to all those who have had much experience in their profession.

\* Two such cases are quoted by Mr. Abernethy, one from Mr. Hill, and the other from Mr. Latta.

In such cases, the patient is said to have been stunned, or to have suffered from concussion of the brain: and it is to one of these three causes, namely, concussion, compression, and wounds of the brain, that the symptoms which immediately follow an injury of the head, and which are antecedent to those produced by inflammation, are to be referred.

Opportunities of inspecting the brain, where the patient has laboured under symptoms of concussion, may arise, 1st, where the concussion has so disturbed the functions of that organ as to have been in itself a cause of death (which is, on the whole, a rare occurrence.) 2dly, where the concussion of the brain has been complicated with other and still more serious mischief. We learn from such examinations, that the symptoms which are ascribed to concussion do not depend on any such derangement of the organization as admits of being disclosed to us by dissection. The brain appears to retain its natural structure unimpaired. We are not however justified in the conclusion that there is therefore in reality no organic injury. It is difficult to conceive in what other manner concussion of the brain can operate so as to produce the effects which it is known to produce; and if we consider that the ultimate structure of the brain is on so minute a scale that our senses are incapable of detecting it, it is evident that there may be changes and alterations of structure, which our senses are incapable of detecting also. The speedy subsiding of the symptoms of concussion does not contradict this opinion. A deep incised wound in other parts of the body may, under certain circumstances, be completely and firmly united in the space of twenty-four hours; and it is easy to suppose that the effects of a much slighter injury may be repaired in a still shorter space of time.

The disturbance of the functions of the brain, which is the consequence of concussion may exist in various degrees and may be of various duration.

In many instances there is at first complete insensibility to external impressions. The patient lies as if in a state of apoplexy, from which however he recovers in the course of a few minutes. In some instances the recovery is complete; the patient rises and walks away as if nothing unusual had occurred. In other cases this state of total insensibility is followed by one in which the sensibility is impaired, but not destroyed. The patient is not affected by ordinary impressions, but if spoken to in a loud tone of voice, he will shift his position, and answer in a peevish manner. Sometimes he is in a state of imperfect delirium, talking in an incoherent and rambling manner, as if intoxicated. The pupils contract on exposure to light, and are sometimes more contracted than under ordinary circumstances. There is no paralysis. The respiration in the great majority of cases is performed easily and naturally; in a few instances only it is laboured, and approaching to being stertorous. These symptoms may wholly subside in the

course of a few hours or they may continue for three or four days. In the latter case it frequently occurs that the patient regains his sensibility for a time, and then relapses into his former condition. Where inflammation of the brain follows the injury done by concussion, it may be that the primary effects of the concussion are entirely relieved, so that there is a considerable interval of sense before the inflammation shows itself. But it may be also that there is no such interval, and the symptoms of concussion, in this last case, are gradually and imperceptibly converted into those of inflammation.

Concussion of the brain in almost every instance occasions headach; sometimes a slight headach, which is speedily relieved; at other times an intense headach, which may remain for some days, a solitary symptom, after all other symptoms are vanished. Sickness and vomiting for the most part are early symptoms, and seldom continue after the patient has recovered from the first shock of the accident. Of course there is no recollection afterwards of what occurred during the period of complete insensibility. The memory however is sometimes affected to a still greater extent; and the impressions made on the mind by the events immediately antecedent to the injury become obliterated. A groom in the employment of the Persian ambassador, in the summer of 1819, was engaged in cleaning one of the ambassador's horses, when he received a kick from the animal on the head. He did not fall, nor was he actually insensible or stunned; but he entirely forgot in what employment he had been engaged at the time of receiving the blow. Being unable to account for the time which had elapsed, he concluded that he had been asleep: said so to his fellow-servants, observing at the same time that "he must set to work to clean the horse, which he ought to have done before instead of going to sleep." A boy going down into the hold of a ship fell from a considerable height, and struck his head. He lay insensible, as it appeared from the observation of his shipmates, about half an hour, when he came upon deck without any assistance. Nevertheless on the following day all the circumstances of the accident had passed from his memory. Some time afterwards when he was received into St. George's Hospital, I found that he knew nothing of the accident except from the report of others. He had not only entirely forgotten the accident itself, but he did not even remember his having gone down into the hold of the vessel before the accident, nor his having come upon deck afterwards: and he never regained his recollection on these points. Desault mentions the case of a man, who, after a blow on the head, at first had no recollection except of recent events: but afterwards a change took place, in consequence of which his memory failed him as to recent events, while he could remember those which had occurred in childhood.

A number of circumstances which it is unnecessary to enumerate, as every physiologist

is well acquainted with them, tend to show that the influence of the brain is by no means necessary to the action of the heart: which may, under certain circumstances, continue uninterrupted, even after the entire removal of the head. Nevertheless, in cases of concussion of the brain, we generally find the circulation more or less affected; the pulse intermitting, irregular, feeble, perhaps scarcely perceptible, and the patient in a state approaching to that of syncope; and such may be his condition for a few minutes, or for the first four or five hours after the infliction of the injury. The connexion and sympathy which exist between the different parts of the nervous system, afford a reasonable explanation of this apparent anomaly, which, however remarkable it may be, is not more remarkable than the syncope which not unfrequently follows the first introduction of a bougie into the urethra, or that which is the consequence of many other trifling injuries of parts remote from the centre of the circulation, and exercising no direct influence over the functions of the heart.

In those cases in which concussion proves fatal it appears to be this disturbance of the heart's action which is the immediate cause of death. In general when the patient has lain for some time in the state which has been described, a re-action of the circulating system takes place, and the pulse beats with greater strength in proportion as the failure of it was greater in the first instance. But where the shock has been unusually severe there is no such reaction. The pulse becomes more and more feeble, more irregular and intermittent; the extremities grow cold, and at last the action of the heart being altogether suspended, the patient expires. In some cases, even after re-action has begun to take place, it seems as if the constitution was unequal to the effort: there is another failure of the circulation, the result of which is the same as if the patient had never rallied from the beginning.

#### SECT. 4.—*Compression of the Brain.*

If the dimensions of the cavity of the cranium be suddenly diminished, as in a case of fracture with depression of bone, or if the actual quantity of the contents of the cranium be increased, as in a case of ruptured vessel and extravasation of blood, the functions of the brain become impaired. This is a matter of experience and observation, about which there is no dispute. There may be, however, some difference of opinion as to the physiological explanation of the phenomena which arise in such cases. It has been usually held that the substance of the brain is actually compressed; but Mr. Bell observes very truly that we have no more right to believe that the substance of the brain admits of being compressed, than that water is compressible; and he infers, that what is called compression of the brain, operates not on the substance of the brain itself, but simply on its blood-vessels; lessening their diameter, and thus preventing that due supply of scarlet arterial blood which is necessary

to a due performance of the vital functions. It is evident indeed that the effect which compression of the brain produces on its vessels must be to a greater or less extent such as Mr. Bell has described it to be. It may, however, be urged on the other hand, first, that in some cases symptoms similar to those which arise from compression, take place where there is a preternatural determination of blood to the head; where the vessels instead of being empty are actually overloaded; and that in these cases the symptoms are relieved by drawing blood from the jugular vein, or from the veins of the arm; as if the pressure occasioned by too much blood in the vessels was productive of nearly the same effects on the brain, with that arising from blood in a state of extravasation: secondly, that, although we admit the substance of the brain to be incapable of being compressed into a smaller compass, yet that the effect of all pressure on it must be, and is, to alter the position and relative situation of the delicate fibres of which its minute structure is composed, and that we need seek no further explanation of the symptoms which are met with in these cases.

In whatever way compression of the brain operates so as to disturb the functions of that organ, it is difficult to explain wherefore the symptoms to which it gives rise are sometimes slight, and at other times urgent, although occurring under circumstances apparently similar. A depression of bone, which in one instance produces comparatively little effect, in another case occasions a manifest destruction of sensibility: and the same observation may be made respecting internal extravasations of blood. Every practical surgeon must have observed that there are differences in the symptoms produced, which are not to be accounted for by any difference in the quantity of pressure, nor in the particular part of the brain which is affected by it. At the same time it is undoubtedly true, that, for the most part, the patient suffers more from an extensive than from a slight depression; more from a large than from a small extravasation. There is reason to believe that pressure is on the whole more dangerous when it affects the lower part of the brain, than when it affects the upper part; and it has appeared to me that more urgent symptoms are produced by a given quantity of blood, when it is effused into the cells between the tunica arachnoides and pia mater, than when it is collected in one mass so as to produce a less general pressure.

Having made these preliminary observations, I shall proceed to consider the particular symptoms which arise from pressure on the brain.

1. *Pain in the head.*—The blow which occasions a fracture and depression of the cranium, or an extravasation of blood within the cranium, is likely to produce concussion of the brain also, and as pain in the head is a symptom of the latter injury, it may be a question, in many instances, to which of these two causes it is to be attributed. That intense

pain in the head may, however, be wholly dependent on pressure on the brain is proved by a case in which a patient under my care laboured under this symptom, and no other, except indeed that the pupil of one eye was preternaturally dilated. There was a fracture with depression of a very small portion of one parietal bone, and immediately on the depression being elevated, the pain in the head was completely relieved.

2. *Insensibility*:—which is sometimes incomplete, corresponding to what is observed in cases of concussion of the brain; the patient lying for the most part unconscious of what passes around him, but capable of being roused by stronger impressions on his senses; while at other times the loss of sense is perfect, so that the skin may be pinched, the flame of a candle may be held close to the eye, and the loudest voice may be uttered in the ear, without any evident effect being produced on the sensorium. Where the cause of these symptoms is a fracture and depression of bone, they show themselves immediately after the infliction of the injury; but where they depend on an extravasation of blood, as, in many instances, the extravasation may take place slowly, so an interval of time, an hour for example, may elapse before the patient becomes insensible. Not unfrequently there is insensibility, from concussion of the brain in the first instance; then the patient recovers, and afterwards, as the blood is gradually effused within the cranium, he relapses into his former state of insensibility. These observations were made first by Le Dran, and afterwards by Mr. Pott, and it is needless to remark how great is their importance, as connected with the diagnosis of these different kinds of injury. But even when pressure on the brain is actually established, the insensibility to which it gives rise is liable to some degree of variation. At one time it may be perfect; then the patient may show some signs of consciousness, and then relapse into a state of perfect stupor. It may be observed, that there is especially an increase of sensibility after blood-letting, and that as the effect, which the loss of blood has produced on the circulation, subsides, so the sensibility becomes again diminished.

If these observations be correct, it is evident that there is not any such difference in the character of the insensibility produced by concussion, and that produced by compression of the brain, as will enable us at once, in all cases, to distinguish these two kinds of injury from each other. Those who are led to take a different view of the subject, may indeed urge, that in some cases there is considerable pressure on the brain, without any symptoms at all; and that when, in a case of fracture, and depression of the cranium, or extravasation of blood within the cranium, the patient lies with a partial loss of sense, this is to be attributed not to the actual pressure, but to the concussion of the brain, which the violence inflicted must necessarily have occasioned in a greater or less degree. I might however refer to several cases, to which this explanation

cannot be well applied; but a single example will be sufficient. A woman received a blow on the head; after which she was able to walk home, complaining that her head was hurt, and that she had received her death blow. In an hour after the accident, she gradually became insensible. About fourteen hours afterwards she was brought to St. George's Hospital, labouring under symptoms precisely corresponding to those which have been described by Mr. Abernethy, as arising from concussion. These symptoms continued, and even rather abated than increased, until the third day, when an aggravation of them took place, and she expired. On examining the body, eight ounces of blood were found effused underneath the dura mater. The circumstance of there having been no loss of sense in the first instance, and the interval of an hour which elapsed between the period of the accident and that of the occurrence of the symptoms, sufficiently demonstrate that they were the consequence of pressure produced by the hemorrhage, and not of the concussion.

It sometimes happens, that there is a destruction of sensibility in one part of the system, while the general sensibility is impaired only in a slight degree. An old man was admitted into St. George's Hospital, who had been run over by a cart. There was a fracture with depression of one parietal bone. He was sensible, but slow in giving answers, and peevish, and it was observed that he was totally blind. Mr. Gunning removed a portion of the parietal bone with the trephine, and elevated the depression; but the operation produced no change in the symptoms. About thirty-six hours after the accident, the pulse became frequent, and he was delirious. He remained entirely deprived of the faculty of vision; believing that he saw imaginary objects, but totally unconscious of the existence of those which were actually before his eyes. At the expiration of the fifth day he died. On examining the body, the membranes of the brain were found to be inflamed, and smeared with pus and lymph. In the basis of the cranium, there was a transverse fracture extending across the sphenoidal bone, and the fractured edges were displaced in such a manner as to press on the optic nerves immediately behind the orbits, and to explain, in the most satisfactory way, the total loss of sight. Such cases as that which follows, are not very uncommon. A gentleman was thrown from his horse, and received a blow on the head. He lay with well-marked symptoms of compression of the brain, which however began to subside in a few days. In a short time, his general sensibility was completely restored, but there was a numbness, or loss of sensation, of one hand for more than a year afterwards.

3. *Paralysis*:—Here, as on other occasions, the same cause which prevents the brain receiving impressions from the nerves, prevents it also transmitting its influence through the nerves to the muscles. Where the destruction of sensibility is complete, the voluntary muscles are completely paralyzed. In whatever

position the patient may be placed, in that he remains motionless. The bladder, incapable of contraction, becomes preternaturally distended with urine; and the relaxation of the sphincter ani allows the involuntary discharge of feces from the rectum. Afterwards the muscles of respiration become affected also; the patient breathes with stertor, as in a most profound sleep; and the diaphragm contracts at longer and longer intervals, until respiration altogether ceases. It is this paralysis of the muscles of respiration, which in ordinary cases of pressure on the brain is the immediate cause of death. Where there is an imperfect loss of sense, there are often no marks of paralysis whatever. At other times there is paralysis of one side of the body, while the muscles of the other side, continue to obey the will as usual; and sometimes the paralysis is permanent. Dr. Hennen\* gives an account of a patient who recovered with life from the effects of a fracture and depression of the left parietal, and left side of the frontal bone; but fourteen years afterwards, he was still paralytic in the opposite arm and leg.

Hemiplegia is however a much more rare occurrence where pressure on the brain is the consequence of accidental violence, than it is in cases of apoplexy from a spontaneous rupture of a blood-vessel. The difference may reasonably be attributed to the different situation of the pressure. In cases of apoplexy, the extravasation is for the most part situated either in one of the ventricles, or in the substance of the brain; but after a blow on the head, the cause of pressure more commonly operates on the surface. Occasionally the paralysis is confined to one set of muscles, or even to a single muscle. There may be, for example, loss of motion in one hand, or a *ptosis*, or dropping down of one upper eyelid. In cases of hemiplegia after an injury of the head, the paralysis is on the side opposite to that on which the pressure exists: at least I have never met with an exception to this general rule. The observation, however, does not apply to more partial paralytic affections. A young gentleman fell from a coach-box, and struck the left side of his head against the wheel of the carriage: he was not stunned, but there was an ecchymosis of the left cheek and temple, a copious discharge of blood from the left ear, and the muscles of the left side of the face were rendered paralytic. When he laughed, the mouth was distorted to the right side; and he was unable to close the left eyelids. The loss of power over the muscles was not attended with any loss of sensation, and was not permanent, the recovery of the patient being complete in about three months. It seems reasonable to conclude that in this case the cause of the paralysis was pressure produced by the extravasation of blood on the portio dura of the nerve of the seventh pair, by which the muscles of the face are supplied, and not on the brain itself. In like manner I

have known *auphosis* of the *left* upper eyelid connected with pressure on the inferior surface of the *left* hemisphere of the cerebrum, the pressure being so situated as to affect the nerve of the third pair immediately behind the left cavernous sinus.

4. *Convulsive actions of the muscles*.—Where there is paralysis of one side of the body after an injury of the head, we sometimes observe convulsive twitches of those of the other side. But it appears to me to admit of a question whether this symptom ought to be regarded as the consequence of simple pressure on the brain. We find it occur in cases of punctured and wounded brain, where there is no pressure; and it so happens, where it has fallen under my observation in cases of depression of bone or extravasated blood, and where the exact nature of the injury was afterwards ascertained, that the pressure has been always found to be complicated with wound or laceration of the substance of the brain.

The convulsive twitches to which I here allude are slight and only partial, and are to be distinguished from those violent fits of general convulsions on which I shall have to offer some observations hereafter.

5. *Affections of the pupils*.—The state of the pupils varies very much in cases of pressure on the brain even under circumstances apparently similar. I have seen the pupils dilate with the absence, and contract with the presence of light, although the patient lay in a state of complete insensibility, and did not seem to be at all conscious of the impressions made on the retina. But this is a rare occurrence, and for the most part where the other symptoms of pressure are present, the pupils are insensible and motionless; being generally dilated, but sometimes contracted. It is not uncommon for the pupils to remain for a time in a state of dilatation, then to become suddenly contracted, and after remaining so for a longer or shorter time, to become again dilated, these changes taking place independently of light and darkness. I have observed especially, where the pupils have been dilated, that they have frequently become contracted immediately after the abstraction of blood; the dilatation returning as soon as the immediate effect of the blood-letting on the circulation has ceased. Dr. Hennen mentions a case in which blood was extravasated among the membranes of the brain, and in which the pupils were observed sometimes to become dilated with an increase, and to contract with a diminution of light. In a patient in St. George's Hospital, in whom there was an extravasation of blood on the upper part of the right hemisphere of the cerebrum, and no cause of pressure elsewhere, both pupils were insensible and motionless; but the right pupil was in a state of dilatation, and the left in a state of contraction. In another patient, in whom there was fracture and depression of the left parietal bone, the left pupil was permanently dilated, the right pupil being in a natural state. In a third case, in which there was a fracture and depression of the frontal

\* Military Surgery, p. 304.

bone above the right superciliary ridge, there was a dilatation of the pupil of the left eye; and again, in a fourth case, where there was a fracture and depression in the same situation as in the case last mentioned, and no cause of pressure elsewhere, both pupils were dilated and equally insensible, but immediately regained their sensibility and power of contraction on the depression being elevated.

As there may be general insensibility without the pupils being insensible to light, so there may be insensibility of one of the pupils without general insensibility, and even without loss of vision. A gentleman fell from his horse, received a severe contusion of the head, and was taken home, labouring under manifest symptoms of pressure on the brain. When, after the lapse of several days, these symptoms became somewhat abated, it was observed that the pupil of the right eye was dilated, and incapable of contraction; but his power of vision was unaffected. This symptom was accompanied with a ptosis of the right upper eyelid, and a numbness of the right hand. I believe that nearly a year elapsed before the pupil was restored to its natural condition.

6. *Affection of the Circulation*.—If concussion of the brain be capable of disturbing the action of the heart, it is not remarkable that the greater injury arising from pressure should produce its effect on the circulation also. The effect however is not constant; and sometimes even where the other symptoms of pressure exist, there is no alteration of the pulse. Mr. Abernethy has observed that intermission of the pulse is a less frequent occurrence in cases of compression than in those of concussion of the brain. However that may be, I believe it will be found that pressure on the brain for the most part affects the action of the heart; not by producing actual interruption, but by causing its contractions to be either less frequent, or less forcible than natural. The influence of pressure on the brain on the circulation is sometimes very manifest in cases of depression of the bone of the cranium, where the depression is relieved by an operation. A child, three years of age, was admitted into St. George's Hospital having an extensive fracture of one parietal bone, extending into the adjoining portions of the temporal and occipital bones. Towards the posterior part of the parietal bone there was a considerable depression, with laceration of the membranes of the brain and of the brain itself. I assisted Mr. Gunning in an operation in which he removed a portion of the bone with a saw, and elevated the depression. Previously to the operation the pulse at the wrist was barely perceptible, but immediately afterwards it became distinct, and beat with considerable strength. A gentleman who held the child's hand during the operation, observed the pulse to be suddenly restored at the very instant of the depression being elevated. Another patient (a man) was admitted into the hospital having a fracture with depression of the right side of the frontal bone extending into the right parietal. The pulse beat no more than

forty times in a minute, but immediately on the depressed bone being elevated it rose to sixty in a minute.

7. *Sickness and vomiting*.—These symptoms occur in some cases of pressure on the brain from injury, but it may nevertheless admit of a question whether they should or should not be referred to the actual pressure. The same injury which occasions a fracture and depression of the cranium, or an extravasation of blood within the cranium, is likely to produce concussion of the brain also. In cases where the symptoms of pressure are the most distinct, and there is complete insensibility, there is no disposition to vomit; and where I have had occasion to apply the trephine on account of a fracture and depression, and there was no sickness previously, I have sometimes known the patient to become sick and vomit immediately on the depression being elevated.

The symptoms of pressure on the brain vary in different cases, not merely as they may exist in different degrees, but as they happen to be variously combined with each other. We find also that there is a great difference as to the period of their duration. Of two individuals, in whom the early symptoms appear to be equally urgent, one may die in the course of three or four hours, and another may survive for several days; and among those who recover, we may find some in whom the symptoms wholly subside in the course of a few days, and others in whom some remains of them exist after the lapse of several months, or even of years. Even in fatal cases the symptoms are not in every instance uniformly progressive, and it is not very unusual for them in some degree to subside, recurring afterwards with increased severity.

Where blood-vessels have been ruptured or wounded in other situations, secondary hemorrhage occurs in some instances at the end of a few days from the period of the injury having been inflicted. Does secondary hemorrhage ever occur within the cavity of the cranium? In one case, which came under my observation, I was led to believe that this actually happened, causing sudden death after three or four days of apparent convalescence. As I have met with no other instance of the kind, I conclude that such occurrence is very rare; but probably it would be more frequent, if it were not that in the practice of modern surgery, a very strict antiphlogistic regimen is usually pursued, for a considerable time after the occurrence of the accident. The following is a brief outline of the case to which I allude.

A man, thirty-five years of age, on the afternoon of the 8th of November, fell from a cart and struck his head against the pavement. A medical practitioner in the neighbourhood bled him, and he was afterwards brought to St. George's Hospital talking and reeling like a drunken man. He was again bled. On the following day he complained of head-ach, but was otherwise well. He continued without any symptoms until five in the morning of the 12th of November, when some of the patients

in the same ward heard him talking incoherently. The nurse called the house surgeon to him, but before he could arrive the man had become insensible, and was found lying motionless, with stertorous respiration and dilated pupils. Blood was taken from the arm, but the symptoms were not relieved, and he died in about half an hour after the commencement of the attack. On examining the contents of the cranium after death, a thin layer of blood was found extravasated in the cells between the tunica arachnoides and pia mater, where those membranes cover the posterior part of the two hemispheres of the cerebrum. In the lower part of the right anterior lobe of the cerebrum, the substance of the brain had been ruptured, and underneath this part, between the dura mater and tunica arachnoides, there was a collection of about two ounces and a half of blood. This last had all the appearance of a recent extravasation, and seemed to afford a satisfactory explanation of the sudden alteration in the symptoms, which immediately preceded the patient's dissolution: the hemorrhage in the first instance having in all probability been checked by the blood-letting, which was resorted to both immediately after the accident, and on his admission into the hospital.

SECT. 5.—*Wounds of the Brain and its Membranes.*

Wounds of the dura mater, greatly as they aggravate the ultimate danger of the case, do not in themselves add to the symptoms which immediately follow the accident. It is when the period of inflammation has arrived, and not until then, that the marks of punctured or lacerated dura mater show themselves.

The pia mater and tunica arachnoides are so thin and delicate in their structure, and so intimately connected with each other, and with the brain itself, that we cannot conceive them to be wounded, without the brain being wounded also. It would be idle therefore to treat of these two classes of injury as being distinct from each other.

The researches of modern science, have demonstrated that the brain is composed of various organs, intended to exercise very different functions: and the division of the substance of the brain made by the hand of the physiologist produces very different effects, accordingly as it detaches one or another of these organs from the rest of the nervous system. But those distinct results which are obtained with difficulty in experimental physiology, are not met with in cases of accidental wounds. The symptoms produced by the latter are always liable to be complicated with those of concussion, and in a great number of instances are also complicated with those of compression of the brain. Accidental wounds rarely affect the cerebellum and medulla oblongata, or even the more deep-seated and important parts of the cerebrum: and with respect to wounds of the cerebrum, such as are commonly met with, even without the complications produced by concussion, or depres-

sion of bone or extravasated blood, we find their effects to be so different in different cases, that they do not admit of being reduced to any general rule; and no data, which we have hitherto obtained, will enable us to predict the exact consequences to be produced by a wound of a given extent, or occurring in a given situation.

In illustration of this observation I may refer to two cases, related, the one by Morgagni,\* the other by Dr. Hennen.† In the first of these cases a man received a punctured wound from a sharp instrument, which passed between the eye and the roof of the orbit, penetrating through the latter into the substance of the cerebrum, to within a finger's breadth of the lateral ventricle. In the second case, the extremity of an iron ramrod entered the cranium, immediately below the nasal process of the frontal bone, and penetrated one inch into the anterior lobe of one hemisphere of the cerebrum. In each of these cases the wound was of the same kind, and very nearly in the same situation: but in one of them it was considerably deeper than it was in the other. It might well be supposed that there would have been some correspondence in the effects produced:—but what were the actual results? In Dr. Hennen's case, where the injury was the slightest, the patient was instantaneously deprived of life: while in Morgagni's case, where the injury was greatest, there were no symptoms whatever, and the patient was as if nothing unusual had occurred until the end of the third day, when suppuration was established.

Of these two cases, however, it must be allowed that the latter is to be regarded as being more in accordance with the general rule than the former. The experience of every individual, who has had the opportunity of seeing many cases of injury of the head, will afford examples of wounds penetrating into the substance of the brain, as well as of incised and lacerated wounds, in which the functions of the brain were not at all impaired, or only slightly impaired in the first instance. Even actual loss of the substance of the brain not unfrequently takes place without the occurrence of any urgent symptoms, and the patient may go on from day to day, with fresh portions of the brain oozing out of the aperture in the cranium, with his external senses perfect, his mental functions unimpaired, and free from paralytic affection.

It is not however to be supposed that there can be an extensive destruction of a part so important as the brain, without immediate death, or death in the course of a very few hours. In other cases in which the brain has been extensively lacerated, it has appeared to me that without the actual insensibility which follows concussion of the brain, there was a confusion of intellect beyond that which concussion usually produces. In many cases of wounded brain there are convulsive twitches

\* Letter 51. a. 57. † Military Surgery, p. 286.

of the muscles of the extremities. In a case in which there was fracture of the parietal bone, several splinters of bone having been driven into the substance of the cerebrum, on the splinters being removed, and when no evident cause of mischief remained except the wound which they had occasioned, the pupil of the eye of the opposite side remained preternaturally dilated. This is what might have occurred in consequence of pressure on the brain. It corresponds also to what we observe in cases of pressure, that wounds of the brain sometimes occasion an unnatural slowness of the pulse. But the more urgent symptoms of pressure are wanting; and the peculiar danger of wounds of the brain arises, in the great majority of instances, not from the immediate effects of the injury, but from the extensive and intractable inflammation which takes place afterwards.

SECT. 6.—*On some other Symptoms following Injuries of the Brain.*

The symptoms of which I propose to give an account in the present section also belong to the class of those which immediately follow an injury of the brain, that is, which show themselves previous to the occurrence of inflammation. I have however thought it better to give them a separate consideration, because there may be some doubts as to the exact nature of the injury of which these symptoms are to be regarded as the indication, and because there are several points respecting them which require to be elucidated by further observations.

I. A middle aged man received a blow on the head and was brought to the hospital with symptoms which were supposed to arise from concussion of the brain. These symptoms subsided in the course of one or two hours, but he remained afterwards completely deaf. His relations declared that his hearing had been perfect up to the period of the accident. He left the hospital at the expiration of three weeks without the smallest amendment.

A young woman received a blow on the head, by which she was stunned for a few minutes. After she recovered from the immediate effects of the accident, she found herself entirely deprived of the senses of smell and taste, and she was in this state when I saw her a month afterwards. The strongest and most pungent odours produced not the slightest sensation when applied to the nostrils; but they nevertheless increased the secretion of the lachrymal glands, or in common language made the eyes water, as under ordinary circumstances.

A middle-aged man slipped while walking, and struck the back of his head against the road; he was stunned for five or six minutes, then recovered so as to walk home. He saw objects double during that evening, and it was observed that he was deaf in one ear. He was kept awake by violent headach during the night. On the following day he had recovered from the double vision, but the other symptoms continued, and in addition to them

he discovered that he had entirely lost his sense of smell, and that there was also a partial loss of the sense of taste. He was bled several times, and kept on a low diet, and under this treatment the headach gradually subsided, and at the end of about four months he had recovered his sense of hearing. When he consulted me between five and six months after the occurrence of the accident, he was in the following condition. His pulse was 72 in a minute. He complained of a sense of noise in the right side of the head, especially in the morning and evening, but not during the night. He was impatient and irritable, especially when troubled respecting matters of business. He had no proper sense of smell, common odours were not perceived at all; but he *felt* the pungency of smelling salts, and they made his eyes water. With his taste he could distinguish bitter, sweet, and sour, but he was unable to distinguish flavours accurately. For example, he could perceive a difference between the taste of hops and that of sugar, but not between that of fennel and parsley; and the flavour of game was the same to him as that of other meat. Bitters had become disagreeable to him, though they had not been so formerly.

The late Mr. Grover of Hammersmith informed me of the case of a gentleman who had been under his care on account of an injury of his head, which entirely deprived him of the sense of smell. After some time, however, he began to recover of this symptom, and at the end of a year his smell was completely restored. I have already given an account of a case in which an injury of the head was followed by total blindness with permanent dilatation of the pupils, and this was found to have depended on a fracture and displacement of the bone in the basis of the cranium producing pressure on the optic nerves. But here there were other symptoms manifestly depending on compression of the brain itself: whereas no such symptoms existed in the cases which I have just related. It is indeed difficult to conceive that pressure on the brain should exist in so great a degree as completely to destroy an entire class of sensations, and at the same time be so partial as not to affect any other function of the nervous system. On the other hand it is also difficult to regard these as the effects of concussion of the brain: since it is one of the characteristics of concussion to produce no more than a diminution of sensibility, and that diminution, instead of continuing for months or years, is completely relieved in the course of a few days, and probably in a much shorter space of time. However produced, these are not the only examples which experience affords of partial nervous affections following an injury of the brain. Dr. Hennen gives the history of a patient who lost his sexual powers after a wound of the occiput. The same author observes, "The powers of speech are often lost while those of memory remain, and the sight is impaired while the hearing is perfect, and vice versa. I have met with numerous instan-

ces of this, and have had patients who told me that they could hear distinctly what I said, and distinguish my voice from that of others, and have repeated my words as a proof both of this fact, and of their retention of memory, while they could not distinguish my person or give utterance to their thoughts.”\*

II. In some cases after an injury of the brain we find the patient attacked by violent convulsions affecting the whole person, and entirely different from those slight involuntary twitches of the muscles which have been already noticed. These convulsions a good deal resemble those which constitute a fit of epilepsy, but are not, like the latter, uniformly followed by a state of profound sleep or stupor. They are more formidable in appearance than in reality, as it is not uncommon for the patient after the convulsions have subsided to recover without any unfavourable symptoms. A young man, a butcher, was standing under a beam of wood which supported a side of beef, when the beam gave way and fell. The side of beef came obliquely on his back, and the beam by which it was supported struck his head. He was not immediately stunned, but in about a minute he became insensible, and in ten minutes more he was seized with a fit, in which he was violently convulsed, so that four or five persons were required to hold him. He was bled, but without relief. The fit of convulsions lasted for nearly three hours, and then suddenly left him. He now complained of pain in the head, but was perfectly sensible. He recovered without any further symptoms, except that the pain in the head continued, and on this account he was bled twice or three times in the course of the ensuing week or ten days.

A gentleman on the 8th of September, 1825, was thrown from his horse, and falling on the pavement received a blow on the arm which occasioned a fracture communicating with the elbow joint, and another blow which caused the scalp to be separated for a considerable extent from the anterior part of the head, and also occasioned a fracture of the frontal bone, but without depression. He was taken up in a state of insensibility. He was in this state a few minutes afterwards, when he was seized with violent convulsions, his limbs being moved in various directions, and with such force, that it was with much difficulty that several persons could hold him. The convulsions continued for about half an hour, when they subsided, leaving him in a state of stupor. Blood was now taken from his arm; after which he began to regain his sensibility. On the following day his sensibility was completely restored, and he recovered without any further unfavourable symptoms.

In these cases the convulsions took place within a short period after the occurrence of the accident; but there are others in which the patient is affected in the same manner, after the lapse of several days. Here the

convulsions must often be combined with symptoms of inflammation, so that it may be difficult to determine whether they are to be regarded as connected with the original mischief produced by the injury, or as arising from the subsequent inflammation. The following case however seems to prove that in some instances at least the convulsions which occur even at this second period depend on the former cause and not on the latter.

A lad, 14 years of age, received a blow on the head, and became instantly insensible. He did not utter an intelligible word, nor could he be prevailed on to show his tongue, nor to swallow either medicine or the liquid nourishment which was offered to him. However, he moaned when disturbed, the pupils of his eyes were sensible to the stimulus of light, and there was neither stertor nor paralysis. These symptoms slowly subsided, and no new symptoms, such as could be regarded as the result of inflammation, had shown themselves, when at the expiration of five days after the accident he was seized with convulsions agitating his whole person. Blood was taken from him by cupping, but this afforded no relief, and in the course of the succeeding twenty-four hours he had as many as fourteen or fifteen attacks, each lasting from one to three minutes. On the following day the state of the pulse not being such as to indicate the necessity of the further abstraction of blood, I determined to pursue an opposite plan of treatment. He was prevailed on to take beef-tea with toast; this was repeated at short intervals, and from the time of his beginning to take more nourishment the convulsions abated, and in the course of another day had wholly ceased. From this time his recovery proceeded uniformly and favourably.

In two of the cases which have been just related the other symptoms were such as might have arisen, and probably did arise, merely from concussion of the brain. This, however, does not prove the entire absence of extravasation, and there are some circumstances which may lead to the suspicion that something more than concussion is necessary to produce such attacks of convulsions as those which have been described, and which at any rate show that they may arise from other causes.

First, I have observed in experiments on animals that a wound on the basis of the brain which causes extravasation of blood on the surface of that organ, generally produces convulsions previous to that state of stupor and paralysis which immediately precedes death.

Secondly, the ordinary symptoms of concussion occur, and indeed are more complete, immediately after the injury is inflicted than at any subsequent period; whereas, according to my experience, convulsions never occur until after a certain lapse of time; when extravasation may have begun to take place.

Thirdly, the following case occurred in St. George's Hospital, under the care of Mr. Keate. A man was admitted who had fallen from the top of a coach, and had struck his head. He was stunned, and continued insen-

\* Hennen's Military Surgery, p. 305.

sible after being brought to the hospital. At the end of two days, when he had begun to recover from this state of stupor, he was seized with violent convulsions, affecting not only the muscles of his limbs, but also those of his face. The first attack of convulsions continued about six minutes, but this was succeeded in the course of an hour and a quarter by four similar attacks, and in spite of a considerable quantity of blood being taken from the arm. At the end of this time Mr. Keate saw him, and made an incision through the scalp at that part which had received the violence of the injury. A fracture about an inch in length was discovered at the posterior part of the left parietal bone, extending into the lambdoidal suture with a slight depression. At this part Mr. Keate applied a saw, and removed the depressed portion of bone. A small coagulum of blood was found lying on the surface of the dura mater, and this having been exposed there was no recurrence of the convulsions.

I have not observed convulsions to take place where there are symptoms indicating the existence of considerable pressure on the brain. The pressure in these cases does not destroy the functions of the brain; it seems to act merely as a cause of irritation, and the operation of it may be compared to that of an exostosis, or other tumour, in producing fits of epilepsy. The circumstance of convulsions taking place after the lapse of some days when they did not take place in the first instance, may probably depend on the brain having been rendered more susceptible by the loss of blood and other methods of depletion, to which it was necessary to have recourse for the relief of the more early symptoms.

III. Occasionally after an injury of the head we find the patient in a state of furious delirium, raving and unmanageable. A man who had received a blow on the head was brought into St. George's Hospital in this condition, uttering loud exclamations, abusing and striking those who were near him, so that it was necessary for several persons to assist in holding him by force as if he were a maniac, while blood was being taken from his arm. As the blood flowed the delirium left him. He remained with slight symptoms of concussion; and these also gradually subsided, leaving the patient in a state of health. Cases such as this might lead us to regard this state of furious delirium as the consequence of mere concussion of the brain. But the same observations may be made respecting these cases, as respecting those in which there are convulsions. The absence of the more urgent symptoms of pressure on the brain does not absolutely prove that no degree of pressure actually exists: and instances occur in which this state of the sensorium is manifestly combined with depression of bone or extravasated blood. For example:—A middle aged man, who had received a blow on the head, was brought to St. George's Hospital an hour after the occurrence of the accident, in a state of raving delirium. There was a wound over the right eye-brow, and a fracture of the frontal bone

extending obliquely upwards with a considerable depression. The depression, however, was not elevated, as the delirium subsided on blood being taken from the arm. After this the man fell into a state of insensibility, from which, however, he could be roused, and then he complained of headach. On the following day he was more sensible, and from this period he recovered without any bad symptoms; but it was observed that the pupil of the right eye remained preternaturally dilated, and that it contracted very feebly on exposure to light.

A middle aged man fell from a cart, and struck his head against the wheel. In about half an hour he was brought to St. George's Hospital. He was sensible, and complained of pain in the head, but more of pain in one arm, which was discovered to have been fractured. At this time he had no other symptom except that the right pupil was more dilated than the left. There was a wound of the scalp, and a fracture with a slight depression of the anterior and inferior part of the left parietal bone. He was put to bed, and while his head was being shaved he became delirious, furious, and unmanageable, so that it was necessary to restrain him by main force. On being bled, he became faint, tranquil, but not perfectly sensible. In half an hour the faintness had subsided, and he relapsed into his former state of raving delirium. He was again bled, and became more tranquil, but still not perfectly sensible. In the evening, twelve hours after his admission, as he continued insensible, Mr. Gunning applied the trephine in the situation of the fracture, and removed a portion of bone. The man appeared to be relieved, and spoke rationally after the operation. On the following day he was quiet, and sensible when roused, but not so to ordinary impressions. Early on the next morning he fell into a state of stupor, with stertorous breathing, a slow pulse, and cold extremities, and soon afterwards expired. On dissection there was discovered a disjunction of the coronal suture in some degree separating the parietal and frontal bones from each other. From a drachm to a drachm and a half of blood was extravasated between the dura mater and the right side of the frontal bone, and the right parietal bone. There was also in some parts a slight degree of extravasation in the cells between the tunica arachnoides and pia mater. A small quantity of pus was found both between the dura mater and the bone, and between the tunica arachnoides and pia mater.

In another case where the patient was admitted into the hospital with the same symptoms of furious delirium, after the delirium had subsided he fell into a state of perfect stupor, from which he could not be roused until twenty ounces of blood had been taken from the arm: and when the immediate effects of the blood-letting had subsided he again relapsed into the same state of stupor. The pupil of one eye was observed to be preternaturally dilated, contracting in some degree, but imperfectly, on exposure to light. This patient ultimately recovered, and of course it

was not possible to be made acquainted with the exact nature of the injury which he had sustained, but I was led to regard the state of complete insensibility in which he for some time lay, joined with the dilatation of one pupil, as a sufficient indication of the existence of pressure on the brain to a greater or less extent.

From the evidence here adduced there seems reason to believe that furious delirium and convulsions occur after an injury of the head under nearly parallel circumstances. The former symptom, like the latter, may be produced by pressure on the brain, not however by such a degree of pressure as threatens completely to annihilate the function of that organ, but by that smaller degree of pressure which operates merely as a source of irritation. It must be admitted, however, that the subject is not exhausted, and that further observations are required for its complete elucidation.

[To be continued.]

From the Lancet.

#### **PATHOLOGICAL AND PRACTICAL OBSERVATIONS ON DYSMENORRHŒA.**

By JOHN MACKINTOSH, M. D., Acting Surgeon to the Ordnance, Physician to the Brown Square Dispensary, and Lecturer on the Practice of Physic, &c., in Edinburgh.

Dysmenorrhœa is a very painful affection, of frequent occurrence, and it is one which has always been an opprobrium to medicine. When menstruation is painful, it is generally scanty, and along with the discharge we frequently find shreds of membrane, like the *decidua uteri*; and occasionally a substance, the size of a hazel nut, is passed; indeed, when floating in water, it resembles a miscarriage at about the fourth or fifth week. Women who pass the shreds of membrane, suffer very severe pain in the region of the uterus, attended with some bearing down; but those who pass the larger masses are generally married women, and although of a different character, their sufferings are fully more severe than with children at the full period. Some are affected with this disease from the first of their menstrual lives; some not till after their marriage; and I have known it not to occur in others till after the birth of a first child, and in all these last instances which have come under my notice, the labour has been long and painful. Many women, who are affected with dysmenorrhœa, frequently have very little suffering during one period, but they complain much at the next, and at the third time they suffer the greatest torture. I have remarked, that in those who have, now and then, an easy time, it is immediately subsequent to the expulsion of a great deal of deciduous looking matter, or of the small mass like the miscarriage.

This disease has been described by all authors from the earliest periods, and it has in general been remarked, that women affected with it are barren. It is unnecessary to quote

the opinions of authors to show the uncertainty which has prevailed respecting the nature and seat of this disease, but I may state, that some suppose it consists merely in an inflammation affecting the mucous membrane which lines the uterus, while others think it is owing to a want of nervous energy of the uterine system, or to constipation, or exposure to cold and damp, &c., and the remedies applied are general and local bleeding, the warm and cold bath, opiates, laxatives, and warm clothing, but with how little *permanent* advantage every experienced practitioner can tell.

It always occurred to me, that there was some mechanical cause which produced dysmenorrhœa, and I had settled in my own mind, that it was one of those affections whose nature and seat were still open for investigation. For many years my attempts were baffled, perhaps, because my sphere of observation in the diseases of women, was not so extended as it has since been. In the year 1823, a gentleman called to say he had brought a curious donation to my museum, of an uterus without an orifice. Upon examining it minutely, I found a mouth; but it was so small, that it would barely admit a hog's bristle. It occurred to me at the moment, that a small orifice might possibly be the cause of the serious affection under consideration; and I have since had many opportunities of investigating this subject; I have also had the advantage of assistance from several of my pupils, and have now obtained many preparations, which were taken from the bodies of individuals who died of different diseases, particularly of phthisis, and whose histories proved that they had laboured under dysmenorrhœa from the very beginning of their menstrual lives. In these preparations of the uterus, the orifices, instead of being shaped like the mouth of the tench, are either circular, or nearly so, and some of them so small, as to be just capable of receiving a bristle, others are a little larger, and will allow a common-sized silver probe to enter, and some are a little larger still. This condition of the *os uteri* accounted quite satisfactorily for dysmenorrhœa; the menstrual discharge, after being secreted in the uterus, cannot readily escape; it distends the uterus, producing considerable uneasiness in the pelvic region, and is at last expelled by the contractions of the uterus. These uterine contractions cause increase of suffering, and a bearing down sensation, as in miscarriage. The continuance and frequent recurrence of this uterine irritation, will give rise sooner or later, to inflammation in the lining membrane of the uterus, which will account for the formation of the decidua, which is discharged like shreds of the membrane, or occasionally entire, when it resembles a miscarriage, but which, when examined, is found to resemble a soft coagulum of blood. Or, in consequence of the long-continued determination of blood to the uterus, the vessels may discharge a little blood into its cavity, and the mass may be thus formed.

That women should generally be barren

who thus suffer, strengthens the view which I have been induced to form. From the results of the accurate experiments of that ingenious and distinguished physiologist, Dr Blundell, of London, together with the information that has been collected from the aberrations of nature, as seen in cases of extra-uterine conception, and also from some of the vices of mankind, it appears certain, that the semen of the male must pass, at least, into the cavity of the uterus, before impregnation can take place. In this case it cannot get in, from the small size of the *os uteri*. These circumstances also readily account for the uncertain and unsatisfactory nature of the practice generally pursued.

My mind now became occupied with devising means, likely to cure women affected with dysmenorrhœa. Mechanical dilatation appeared to be the proper remedy, but I was afraid to carry it into execution, or, indeed, to propose such a measure to a modest woman, without being able to give an assurance, almost amounting to a certainty, that it would cure her, which I could not conscientiously do. And although I had constantly cases of this kind under my care, I never attempted the operation, nor, perhaps, should I till this day, if chance had not thrown the following case in my way.

A young woman, aged 22, came from the country to consult my friend and colleague, Dr. J. A. Robertson, who sent her to me. The female friend who accompanied her stated, that the menstrual discharge had never appeared, that she had always been a healthy girl till she had reached the age of sixteen, from which period she began to suffer, and to fall off. Regularly every month she complained of pains in the back and loins, together with a sense of weight and bearing down in the passage. At first she suffered slightly, and was soon able to perform her duties as a servant, but for the last two years she had never known what it was to have a day's ease; that she would submit to any thing to be cured and to have that which other women had. The girl appeared to be above the middle stature; the *mammæ* were undeveloped; she was of an awkward shape; her voice was harsh, and her skin coarse and dark coloured, so that had I not been told she was ill, I never could have guessed it from her appearance. Her abdomen was not tumid, but I was told it was occasionally swollen, particularly after meals, which seemed to me to be from flatulency. She appeared to be of a nervous temperament, and was exceedingly shy and timid.

Upon examination, my fingers passed readily into the vagina, which was considerably relaxed, and the uterus was felt much lower than natural, but I could discover no orifice. Dr. Robertson had previously discovered the same fact, but had not then communicated the circumstance to me, thinking that he might have been mistaken. I repeated the examination many times, and after feeling the spot where the orifice ought to have been, which was distinguished by a

small dimple, I attempted to introduce one of the smallest silver probes I could get made, but was unsuccessful in every attempt. It then occurred to me, that the malformation might be owing to an extension of the mucous membrane over the orifice, in which condition we sometimes see the urethra of a new-born male child. I determined upon giving her the chance, particularly as the means to be used would not certainly produce severe pain. Accordingly the sharp and triangular extremity of a silver probe was introduced, directed by the finger, and conducted to the part above described, and a perforation made by employing a rotatory motion; the instrument was then withdrawn, and the round point introduced, which readily passed up to the fundus of the uterus. For several days she complained of a good deal of burning pain, attended with a slight discharge of mucus, a little tinged here and there by bloody specks, and I did not think proper to interfere further, until the irritation had subsided. In about eight days I began the attempt to produce dilatation, which was persevered in daily, increasing the size of the instrument, and by the twelfth or thirteenth day I was able to introduce No. 6 male bougie (straightened) to the fundus of the uterus. On the following day there was the appearance of so much irritation, both local and constitutional, that I made no further attempt. In two days afterwards she menstruated, and has been regular ever since, and suffers neither pain nor inconvenience. Her health and strength soon recruited, and in a short time her appearance became quite feminine. She has called upon me several times since, at intervals of five or six months.

I have since dilated the *os uteri* in six cases of dysmenorrhœa, and I may mention generally that the success of the practice has been most satisfactory; in all the cases the women had suffered for years, and they have all menstruated regularly, and without pain, for the last six months. The highest number I have introduced is No. 8, and I have insinuated the instrument to the very fundus. I feel a delicacy in giving the particulars of the cases, for obvious reasons. The profession may, however, rely upon my statement, that it is, at least, a safe expedient, and one not necessarily painful. The only bad consequence which I have seen, was in one lady, who, during the operation, had severe rigours, which lasted for two or three hours, and which were followed by fever, and terminated in copious perspiration; the instrument was not re-introduced for ten or twelve days, and no such effect afterwards followed. I do not mean to assert that all cases of dysmenorrhœa are owing to a small *os uteri*. I believe that it is occasionally produced by inflammation of the lining membrane of the uterus; and also by scirrhus affections of that organ.

In one of the six cases the *os uteri* was large enough, but there was a contraction in the *cervix*, which would not allow the introduction at first of any thing larger than a common

probe. In my museum are two preparations which illustrate this subject; in the one the *os uteri* was completely closed up by disease, inflammatory action took place in the inner membrane, and above two ounces of pus were contained in the cavity of the uterus, dilating it to about the size which it acquires in the second month of pregnancy; in the other, the cavity in the body of the uterus is divided into two, the effect of previous inflammation, so that there was no communication between the upper compartment and the lower.

Since writing the above, a lady, the subject of a seventh case, has menstruated three times since the introduction of No. 6, steel bougie, without the slightest pain or uneasiness during the two first periods; but during the third, she experienced a slight uneasiness in the pelvic region, which, although trifling, deserves to be mentioned. She also complained of nausea and headach, which might have been produced by other causes. It ought also to be mentioned, that she previously suffered intense pain during every menstrual period for between six and seven years.

I have lately contrived a very simple instrument, which I think will enable me to complete the dilatation most effectually in less than half the number of operations, which, under such circumstances, is a matter of the very first importance. The result of the trials made with this instrument, shall be communicated to the profession at an early period, through the medium of your valuable and independent journal.

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From the London Medical Gazette.

#### OBSERVATIONS ON THE OPERATION OF LITHOTOMY. By M. le BARON LARREY.

If the operation of bubonocoele presents almost always difficulties in the performance, arising from the various and unforeseen complications which strangulated herniæ afford, we see likewise, in lithotomy, difficulties to overcome occasionally, which require all the skill and courage of the surgeon to surmount.

It is often difficult, sometimes impossible, *a priori*, to appreciate the nature of urinary calculi, and their relative situation with respect to the bladder, so as to determine the fittest mode of operating in each case: therefore an exclusive method cannot be recommended, or adopted; each case must stand upon its own merits; for that operation which would appear to be the least painful and most certain, is often contra-indicated in the particular instance in which it may have been proposed. The following cases will explain my meaning.

The subject of the first, was King; a soldier in the first Swiss regiment of the guard, who was admitted into the hospital in September 1825, complaining of violent stranguary and tenesmus, with fever, accompanied by incontinence of urine, which passed from him drop by drop. A stone was discovered to be the

cause of these symptoms, which appeared not only to be unequal on its surface, but adherent to the bladder; and, therefore, I expected to meet with difficulties in the operation, which was performed in the presence of MM. Ribes and Souberbielle. There was some difficulty in passing the sound into the bladder, in consequence of the size and situation of the stone; but the incision of the integuments, urethra, and neck of the bladder, was quickly effected by a strong straight bistoury. The introduction of the gorget, however, was equally difficult, and that of the forceps became absolutely impossible. The index finger being passed to the bottom of the wound, I discovered that the stone, covered with tuberculous asperities at the part corresponding to the incision, was adherent, at every other point of its circumference, to the mucous membrane of the bladder: it was necessary, therefore, to destroy these adhesions, which I, at length, succeeded in effecting; but afterwards, I was compelled to pass in the forceps, with their blades separated, in the same manner that the forceps are introduced between the cranium of the infant and the pelvis, in difficult labours. The first attempts to extract the stone were followed by the fracture of some of its asperities; but, at the *third trial*, the whole was brought away, with shreds of the mucous membrane, which covered about *three-fourths* of its circumference. Emollient and anodyne injections were then thrown into the bladder, a ligature was applied to the transverse artery of the perinæum, which had been divided; and the patient put to bed. Iced mucilaginous drinks were prescribed, with emollient clysters, and chicken broth for diet. A few minutes after the operation, King was plunged into a cold, emollient bath, on coming out of which he slept for several hours: that night and the following day passed without any bad symptoms. In the evening of the following day, he had a slight attack of fever, with some pain in the abdomen and tenesmus: two successive bleedings in the arm were practised, the baths were repeated, and anodyne emulsions prescribed to be taken at night, as well as embrocations of the oil of almonds upon the belly. From the fourth day, I no longer had any anxiety as to the issue of the operation, and in about a month the patient was perfectly well.

From the fifth to the eighteenth day, he had passed by the wound portions of white membrane, thrown off from the mucous surface of the bladder: at length this organ became gradually so much dilated that the patient was enabled to retain his urine from two to three hours.

The second case occurred in a child named Binet, 7 years of age, who had suffered for some time from difficulty in passing his water. Having sounded him, I discovered a small calculus, which could only be reached by passing the instrument in very deeply. It was not to be felt by the finger in the rectum; nevertheless, being certain of its existence, I did not hesitate to perform the operation. The in-

cisions were made rapidly and easily; but the extraction of the stone was attended with difficulty, on account of its adherency to the fundus of the bladder; a very long and fine pair of forceps were necessary to lay hold of it, and with these I succeeded easily. The adhering portion was covered with membrane: the same precautions were used as in the preceding case;—emollient injections being passed into the bladder, and the child put into an emollient and gelatinous bath, nearly cold, a few minutes after the operation. This child had scarcely any fever, and was cured on the nineteenth day.

**REFLECTIONS.**—It is difficult to carry the lateral operation of lithotomy to a greater degree of perfection than it has now reached, especially when the surgeon uses no other instrument than the bistoury, the action of which he can direct at pleasure. In this operation, two accidents are alone to be feared; but the surgeon thoroughly acquainted with anatomy, can readily avoid them. These are the wounding the rectum, and opening the pudica interna artery. The first is best avoided by emptying the intestine with clysters, or any other means, if it appear to be loaded with feces; as to the artery, that can only be reached by carrying the edge of the knife transversely against the ascending branch of the ischium, which must even be cut before this vessel can be wounded; therefore the hæmorrhage from the transverse artery of the perinæum is alone to be dreaded in this operation, the division of which cannot be avoided in any case. This is easily remedied by the application of a ligature, which should always be made, whether the bleeding be much or little. It is by attending to this precaution, not mentioned by authors, that I owe the success of my operations; for death is often occasioned by hæmorrhage, or else from the plugging of the wound, which bleeding, when it supervenes, renders necessary, and which is generally followed by inflammation, or by infiltration of urine into the cellular tissue; whilst, on the contrary, the application of the ligature prevents hæmorrhage, renders all plugging of the wound unnecessary, and affords a facility for putting all those means in use which are proper to dissipate spasm and to prevent the occurrence of inflammation, or of urinary fistulæ. The most efficacious of these means are emollient and anodyne baths, of the temperature of 20 or 21 degrees of Reaumur—[77 or 79 of Fahrenheit.]

From the above-cited cases, it appears that tearing the mucous membrane of the bladder is not so dangerous as might have been conceived; for it is very evident, in the first case especially, that this membrane was torn to a great extent, since patches of it were found attached to the stone. I think, then, that the most important rule, is to tie the divided vessels; a plan that should be followed in the course of these vessels, whether they are apparent or not. It is impossible that this precept can be too much diffused; for to it I attribute the constant success which has attended my operations of lithotomy; and if the

public in general knew that this was as certain for its result as other surgical operations, they would not resort to mechanical means of cure; which, though more gentle and less dangerous in appearance, are rarely found to be so in practice.

In the two cases above mentioned, what other method of operating could have answered? One of the surgeons (M. Souberbielle) wished that the high operation should have been performed in the case of King, but I rejected that plan, because I judged it to be impracticable, the bladder being retracted and adherent, in nearly the whole of its internal surface, to the asperities of the stone. It would, therefore, have been impossible to have passed the *sond à dard* of Frere Come above the pubis, without running the risk of piercing the bladder at any other point rather than the proper one. For the same reason, how could the concave blades of the forceps of Civiale have been placed between the stone and the bladder, to embrace this stone, and to pulverize it with the trephine, without piercing or tearing the parietes of the bladder? Would this proceeding have been more applicable in the second case, in which the stone adhered to the fundus of the bladder? The same objections would have applied to the double instruments for lithotomy of the ancients, lately again introduced by M. Dupuytren. Finally, had King been operated upon by the method of Celsus, it may be easily seen that the blades of the instrument could not have been introduced between the bladder and the stone without wounding the parietes of the viscus, where they were attached to the rough portions of the stone. The two individuals mentioned above, were presented to the Royal Academy in November, 1825.

From the London Medical Gazette.

**MEMOIR ON THE DISLOCATION OF THE HEAD OF THE RADIUS.** By C. ASTON KEY, Surgeon to Guy's Hospital, &c.  
*Read at the Hunterian Society, July 2, 1828.*

Having been led, in the course of some investigations into the subject of dislocations, to pay particular attention to the circumstances attending *dislocation of the head of the radius forwards*, and having noticed some facts connected with this accident not hitherto described by authors who have written on the subject, I venture to lay them before the Hunterian Society.

It need scarcely be observed that, to those who are much engaged in practical surgery, this accident is known to be so difficult of reparation as frequently to have resisted attempts at reduction under the most judicious hands; and for the information of those who have not witnessed this accident, and who are therefore not aware of the difficulty of reducing the bone, it may be sufficient to quote the highest authority on the subject, that of Sir Astley Cooper, who mentions in his work on Dislocations, that he has himself witnessed six

cases of this accident, four of which were unreduced dislocations. He adds a seventh case, which had also been left unreduced. Of these five (out of seven) unreduced cases, one occurred under the late Mr. Cline, "who, after the most varied attempts that his strong judgment could suggest, failed to reduce the bone; and the woman was discharged from the hospital with the dislocation unreduced." Another of the unsuccessful cases occurred to Sir Astley Cooper, who, "after continuing and varying the extension for an hour and a quarter, could not succeed in effecting a reduction."\*

The issue of these cases, under the hands of surgeons so eminent for a knowledge of their profession, is sufficient to show the difficulty attending the reduction, and to prove the importance of the subject; but at the same time it would lead us to suspect, that either the nature of the dislocation, or the principle of reduction, must be imperfectly understood.

In the dislocation forwards, the head of the radius is said to be thrown upon the external condyle of the humerus, and to lie over the coronoid process of the ulna; and in a dissection of an old dislocation of the kind, Sir Astley Cooper describes the head of the bone as resting in a hollow above the external condyle. In order to understand the nature of the dislocation, and the manner in which the muscles act in preventing reduction, I endeavoured to dislocate the head of the radius forwards on the external condyle, having first divided the coronary, capsular, lateral, and oblique ligaments; and also a portion of the interosseous: notwithstanding this free detachment of the head of the bone, I found that the radius could not be moved upwards toward the external condyle by any force that I could employ; nor indeed can such motion be given to the bone while the connexion between the radius and the carpus remains entire. Complete dislocation at its carpal extremity is requisite to allow this upward movement of the radius, which the fibres of the interosseous ligament alone can prevent.

\* The commonly received opinion as to the situation of the head of the radius arises from two circumstances; the striking of the head of the bone against the fore part of the humerus in the flexion of the fore-arm; and the examination of unreduced dislocations, in which the head of the radius is apparently lodged on the external condyle of the humerus. These circumstances, however, only take place in certain positions of the arm, as will be seen when the nature of the dislocation is understood.

A close examination of the several circumstances attending this dislocation, combined with the impossibility of the radius being thrown on the condyle of the humerus, will show, that the head of the radius passes forward upon the coronoid process of the ulna, resting upon that process and upon the tendon of the brachialis internus muscle; and a farther

investigation will also explain the difficulty of the reduction, as well as the appearance which the limb assumes under the accident.

The signs of this dislocation, as correctly described by Sir Astley Cooper, and as witnessed in one case by myself, are threefold. First, the arm cannot be perfectly extended. This arises from the brachialis internus tendon being compressed by the head of the radius, which thereby limits the extension of the fore-arm. Secondly, the power of flexion is limited to nearly a right angle, in consequence of the head of the radius striking against the brachialis internus and fore-part of the humerus, when the fore-arm is bent. It is to be observed, that in the flexion and extension of the fore-arm, the displaced head of the bone follows the motions of the coronoid process of the ulna, retiring from the humerus when the elbow is extended, and in the flexion of the joint moving with the coronoid process towards the humerus. Thirdly, the limb is in a state of semipronation, being more or less fixed in that position; and any attempt at rotation is attended with difficulty, and productive of pain. When we look for a cause of this fixed condition of the limb in the action of some of the muscles, we find the pronator teres and biceps relaxed, and the supinator brevis in its natural state. Muscular contraction, therefore, does not appear to fix the head of the bone in its new situation; but in the extended state of the interosseous ligament will be found to exist the principal, if not the sole, difficulty of reduction.

Attempts at reduction have always been made under the impression that it was necessary to disengage the radius from the external condyle by extension. It must be apparent, from the situation of the radius on the coronoid process of the ulna, that extension alone can effect nothing towards the reduction. Nor, indeed, is the principle on which extension by the hand is adopted, correct; for extension by the hand cannot be made to act on the radius independently of the ulna: as long as the ligaments connecting their carpal extremities are entire, they are virtually one bone, and are equally extended by a force acting through the medium of the carpus. It is, however, true that this dislocation has been reduced, while extension has been forcibly made, as in a case of Sir Astley Cooper, in which he placed the arm bent over a sofa;—but in this position of the limb, it is highly probable that forcible supination was at the same time taking place—a movement calculated to reduce the dislocated bone when it is not much advanced on the coronoid process. But that extension, as a means of reduction, is inadequate, is proved by the circumstance of five out of the seven cases given by Sir Astley Cooper having been left unreduced.

The impediment to reduction appears to be a band of the interosseous ligament, about one-third down the fore-arm, which is violently stretched by the separation of the radius from the ulna, and retains the head of the radius on the coronoid process. Upon the extent to

\* Sir Astley Cooper on Dislocations, page 420, 3d Edition.

which the interosseous ligament is torn, will depend the ease or difficulty of the reduction. In cases where the interosseous ligament is extensively torn, and the head of the radius not firmly bound down, supining the hand, while the head of the bone is pressed outward, will enable the surgeon to replace it. But in a more difficult case, when the supination of the limb fails, in consequence of the tension of the interosseous ligament, the surgeon can convert this opposing band of ligament into an *auxiliary* in the attempt at reduction, *by forcibly pronating the hand*. This can be understood by observing the twisting of the interosseous ligament, in the ordinary position of the dislocation, and the effect of supination and pronation upon its fibres. In supination, the lower fibres of the ligament are relaxed, while the upper are rendered tense; in pronation the contrary takes place. The first attempt at pronation is attended with difficulty; but as soon as the spine of the radius becomes turned toward the ulna, the interosseous ligament draws the head of the radius outward and backward into its place. Some assistance may be obtained by pressing the head of the bone outward, and bending the arm, to relax the brachialis interior muscle.

From the Transactions of the Medical and Chirurgical Society of London.

**CASES OF TUMOURS IN THE ABDOMEN ARISING FROM ORGANIC DISEASE OF THE STOMACH; WITH REMARKS.** BY EDWARD J. SEYMOUR, M. D. (SEC.) Fellow of the Royal College of Physicians, London, and Physician to the Asylum for Recovery of Heal.h.

Several cases have recently fallen under my observation, in which tumours in the abdomen of considerable size were found to arise from organic disease situated in the stomach, they appear to me worthy the attention of the Society; first, because very few such cases are on record, and secondly, because in two of them the symptoms which have been considered to characterize organic disease of this viscus were altogether absent.

Among the numerous cases of disease of the stomach related by Morgagni,\* one only is to be found in which a tumour was perceptible during life, and in this instance the pain in the stomach, to which the patient had been long subject, and the constant vomiting which had existed during the latter period of his life, gave sufficient evidence of the seat of the disease.

In fourteen cases related by Lieutaud,† two only are mentioned where the tumour formed by the disease was perceptible externally, and in these the symptoms of pain, vomiting, heart-

burn, &c. were sufficient to draw the attention of the physician to the real state of the case.

In Dr. Monro's work on Morbid Anatomy,\* a very remarkable case is detailed, in which "a lady had suffered during some time from pain in the epigastric region, indigestion, and wind in the stomach and bowels." On examining the body, a tumour was found on the right side of the navel, of an oval shape and about the size of an orange, which at the time was supposed to be lodged in the colon. On inspecting the body after death, the stomach was found to have fallen down as low as the navel; on opening it there appeared a tumour adhering by its neck to the villous coat. The surface of the tumour "was smooth, and the body of it so firm, solid, and tough, that it was cut through with some difficulty."

Dr. Baillie observes in his work on Morbid Anatomy, "where the person is much emaciated, and the cancerous swelling is situated near the pylorus, or along a part of the great curvature of the stomach, it may be felt in the living body by a careful examination by the hand." This implies only that in certain cases such enlargements may be discovered, whereas in the cases which I am about to relate the tumours were visible and of very considerable size.

The essential symptoms of cancer or fungus hæmatodes of the pylorus, enumerated by authors, are pain in the region of the stomach aggravated on taking food, frequent vomiting sometimes mixed with blood, often occurring about half an hour after solids or fluids have been swallowed, sensation of weakness, occasional syncope. As the disease advances, the vomiting increases in frequency, and resembles coffee in colour; and there are often accessions of hectic fever with great emaciation.

The disease very rarely, if ever, attacks persons under forty years of age, and is more common in women than in men. It has been very frequently observed in persons subjected either to great fatigue of mind or anxiety. In a great majority of cases there is a remarkably exsanguine appearance in the countenance, even early in the disease.

In the numerous cases related by M. Lieutaud, vomiting was always present, and in the greater number acute pain was experienced in the stomach. M. Vicq d'Azyr, in his very able article *Anatomie Pathologique*, in speaking of this disease, says, "It is worthy of remark, that as there is *always vomiting* when the seat of the malady occupies the pylorus or its neighbourhood, so deglutition is impeded or altogether obstructed when the disease attacks the cardiac orifice."

"When the disease," says Dr. Monro, "is seated in the stomach, there is great pain in the organ affected, with all the usual symptoms of indigestion, very frequent nausea and vomiting, and the occasional rejection of blood by vomiting, and the patient dies completely exhausted."

\* Epist. 29. Art. 6.

† Liber primus de Læsionibus Abdominis, Obs. 941.

Although these symptoms are so common as to be necessarily enumerated in the history of the disease, yet they are by no means uniformly present, two of them sometimes existing in a slight degree only, and occasionally being altogether absent, viz. pain and emaciation.

A very remarkable case is related in the practice\* of Dr. Pemberton, where extensive disease existed in the stomach, and no symptom whatever was present during life to mark its nature. A similar case is related by De Haen, where periodical vomiting was the only suspicious symptom, in a patient whose appetite, circulation, and digestion, appeared to be perfectly natural, the evacuations being sufficient in quantity, and of healthy colour.†

In the recent very laborious work of M. Andral, on diseases of the abdomen, several cases of fungus hæmatodes of the pylorus are related; in the greater number of which pain denoting disease of the stomach was altogether absent, but the vomiting, and emaciation, and exsanguine appearance of the patient, together with the occasional detection of a hardness in the situation of the smaller extremity of the stomach, rendered it difficult not to perceive the nature of the disease. This author is of opinion that no symptoms exist, which can in the living body point out the diagnosis between this disease and chronic inflammation of the stomach.

The first case which I shall relate is one which I had an opportunity of observing in St. George's Hospital, under the care of Dr. Chambers; the characteristic signs of the disease were undoubtedly present in this case, but its extent and the size of the tumour were very remarkable.

CASE I.—Ann Row, æt. 39, unmarried, a cook. Admitted July 11, 1827. Has been subject to occasional pain in the abdomen for several years past, not however sufficiently severe to confine her to bed. About Christmas last she was attacked with vomiting of blood and diarrhœa, with very violent pain in the belly. About two months ago she perceived a tumour at the inferior part of the left hypochondrium, extending to the umbilicus.

The tumour, which is hard, unequal, and very tender on pressure, occupies the whole of the epigastric and the umbilical region, extending to within an inch of the symphysis pubis and to the right iliac region; at this latter part (an inch to the right of the navel) it is more elevated, and there is a strong pulsation communicated through it. She vomits occasionally after taking food, but not always; sometimes when the stomach is empty. She describes what she vomits to be bitter and sour. Bowels very much relaxed, tongue clean and moist and of natural colour; no catamenia for three months; pulse 96, very weak; urine scanty; she is much emaciated.

12th Sickness very distressing, bowels

open. (Haust. Salin. Efferves. 6tis. horis c. T. opii ℥ v.)

16th. *Hirudines viij tumori. Fetus papaverum abdomini admovend. R Bismuth. Subn. Oss. Pulv. Tragacanth. C. gr. v.; M. Ft. Pulv. t. d. Rept. alia.*

18th. There is an equally diffused swelling of the parts about the right clavicle, extending to the right axilla, which is very tender to the touch; the mamma is not affected, the right arm and hand are œdematous. (*Hirudines viij tumori. Fetus partibustumidis admov. P.*)

20. P. 23d. Sickness less severe, constant purging, great tenderness in the swollen part, pulse natural, tongue healthy, skin cool, emaciation increased. (*Hirudines viij regioni tum. Postea applic. Cataplasma Lini.*) *R Acidi Hydrocyanici Medic. ℥ j.; Decoct. Hordei ʒj. M. Ft. haust. t. d. sum. Rept. alia.*

25th. Died.

26th.  *Sectio Cadaveris.*—The cardiac extremity of the stomach was healthy; but on cutting the anterior surface of the pyloric portion the coats were found considerably thickened, and on the inner surface an irregular tumour presented itself, occupying about two-thirds of the circumference of the stomach, and only leaving the anterior part free. The tumour began about the situation of the pylorus, and its greatest length was about five inches extending towards the left side. It projected about an inch into the interior of the stomach, the surface being very uneven, several round masses rising upwards from the body of the tumour. The surface was for the most part of a reddish yellow colour, some parts nearly brown, and here and there complete sloughs had been formed. The surface of the duodenum and of the stomach was very vascular around the tumour.

In the centre of the tumour an opening about an inch and a half in diameter, with sloughy circular margin, led backwards into a cavity containing about two ounces of fetid pus. The whole surface of the cavity being covered with a brown sloughy membrane like the margin of the opening, its parietes were formed by adhesions between the stomach, colon, and duodenum anteriorly, and by the spine behind. At the margin of the opening of the stomach nearest the duodenum, a sloughy tumour about the size of a small orange projected from the general mass into the abscess; and still more to the right side another large tumour was perceptible, both from the front of the abdomen and at the bottom of the diseased mass of the intestines and stomach, having the duodenum, colon, and stomach adherent to the anterior surface. This was the only part of the whole disease which had not yet ulcerated, and it seemed to be composed of glands united together; it was soft and pulpy and of a light colour, like the usual appearance of fungus hæmatodes. The remainder, where ulcerated was also soft, and resembled very much the usual surface of a tumour composed of fungus hæmatodes when it has ulcerated through the common integuments.

\* Abdominal viscera.

† De Inflammatione Membran. p. 182.

CASE II.—Mr. C. æt. 59, a gentleman who had always enjoyed good health and was remarkably temperate in his habits, but much occupied by anxious professional business, consulted me in the month of November, 1825, being affected with pain in the region of the bladder, particularly felt after voiding his urine, which was high coloured and deposited freely uric acid. The warm bath and the use of soda and opium shortly relieved these complaints; a visit to the sea-side and the moderate use of tonics completely restored him.

About November 1826, he mentioned to me that he was occasionally troubled with water-brash, which he described as a small portion of tasteless fluid rising occasionally into his mouth, unattended by pain or any uneasiness whatever. His appetite was extremely good, sleep undisturbed; he had no pain in any part of his body. His pulse was not strong, but regular, and of natural frequency, and he described himself to be in good health. He was recommended twenty minims of Liq. Potassæ in lime water twice in the day, but the inconvenience appeared to have been so slight that he did not comply with the prescription.

On the 13th March, 1827, while visiting another patient in the family, I observed that Mr. C.'s countenance and manner betrayed considerable indisposition, and I inquired if he were suffering from return of pain in the bladder. He replied he thought he had taken cold, and that he was much harassed by business. He said he felt as if he required opening medicine. I ordered him an aperient and desired he would lie in bed in the morning that I might examine his abdomen, as on pressing him through his dress there appeared some tenderness present.

14th. The patient being in bed, the symptoms were as follow: bowels freely open from the medicines, dejections loose, but of good colour, pulse 100, extremely weak, urine very turbid, tongue red and shining, appetite good, great sensation of debility, with an exsanguine appearance of the countenance, the less remarkable as the patient had always been unusually pale.

About midway between the umbilicus and superior anterior spinous process of the left ilium, a tumour was observed of the size of a large orange, extremely hard, extending over about half an inch to the right side of the umbilicus, and an inch below it. This tumour was adherent to the integuments, was rather moveable, and there was considerable tenderness on pressure. Notwithstanding the size of the tumour, its tenderness, and its prominent figure, the patient, until my examination, was totally ignorant of its existence. The apparently rapid growth of the tumour, its hardness and irregularity, combined with the bloodless appearance of the patient, and the great and sudden loss of strength experienced, induced me to believe that the disease was of a malignant nature. A dozen leeches were ordered to the part, and a consultation took place in the evening with Dr. Nevinson. Dr. Nevinson was likewise of opinion that the

disease was of a malignant kind, but no decision could be formed as to which of the viscera it affected particularly. (Hirudin. xij tumori; capiat Pil. Sapon. c. Opio gr. iij h. s.). R Mist. Camph. ʒx. Sp. Æther. Nitr. ʒss.; Confect. Arom. ʒj.; M. Ft. haustus 4tis horis sum. (Light nourishment.)

15th. A consultation took place with Mr. Brodie, who agreed in the opinion that the disease was fungus hæmatodes. The leeches were ordered to be repeated. Evaporating lotions to the tumour. The internal medicine to be repeated.

On the 18th, the tumour having increased, a consultation took place with Mr. Brodie and Sir A. Cooper. The latter gentleman was of opinion that the great intestine on the left side adhered to the parietes of the abdomen, that the inner coat had ulcerated, and a tumour was formed whose contents consisted of gas, ill-conditioned matter, and fæces. Poulitices and fomentations ordered. The soap and opium pill repeated at bed-time. R Infus. Gentian. C. ʒx.; Infus. Rhei. ʒij.; Pulv. Ipec. c. Opio, gr. iij.; Subcarbon. Sodæ exsicc. gr. v.; M. Ft. haustus t. die sumend.

23d. Some fluctuation being perceived in the tumour, an opening was made to the left; a little above the umbilicus with a lancet, about two ounces of fætid sanious pus escaped from the orifice. Some hemorrhage occurring, the pulse in the evening became extremely small and feeble, tongue red with a brown centre, countenance much sunk, bowels purged. R Pulv. Cret. C. ʒss.; Confect. Arom. ʒj.; T. Opii ʒ. v.; Mist. Camphoræ ʒx. M. Ft. haustus 4tis horis sumend. (Vini Rubri ʒij ter in die.)

26th. The opening discharged freely, pulse 100, strength much improved, aphthæ in the mouth. R Infus. Cuspariæ ʒx.; Confect. Arom. ʒj.; Pulv. Cretæ C. ʒj. M. Ft. haustus ter die sumend.

The relief experienced by letting out the confined matter was of very short duration. The tumour enlarged as the cavity of the abscess filled up, and the condition of the patient on the 17th of April was as follows: The tumour occupies the whole of the umbilical region, being about six inches in breadth, and four in length. No pain whatever is experienced on pressure, or at any period. The cavity of the abscess filled up about one half. Bowels slightly relaxed. No vomiting or nausea. Tongue clean, less red and shining. Appetite good. Sleeps well. Pulse 100, weak. R Infus. Cascarillæ ʒx.; Canell. Alb. in Pulv. ʒss.; T. Opii ʒ. iij.; Syrupi ʒss.; M. Ft. haustus ter in die sum.

It now appeared expedient to endeavour by all the means in our power to check the growth of the tumour, and in such a case the various remedies which have been insisted on by authors, for promoting the dispersion or absorption of morbid growths, were fairly to be tried, however small the hopes of success which resulted from their employment.

Several blisters were applied in succession over the tumour, without affording any ad-

vantage. The tumour appeared inert, producing no pain on pressure, or during the whole process of digestion, which was uniformly to all appearance healthy, one natural evacuation being voided in the twenty-four hours; and when (which was a very rare occurrence) this was deficient, a small dose of castor oil, relieved the difficulty. The only bad symptom was the sense of extreme debility, and occasionally slight syncope.

On the 20th of May, a drachm of weak mercurial ointment was ordered to be rubbed in over the tumour daily, and three grains of blue pill given at bed-time. The cascarrilla and canella, from which the patient expressed himself to derive relief, was continued. This course was persevered in for nearly three weeks, and given up without appearing to have in any way contributed to the diminution of the tumour, or the amendment of the patient's general health.

The action of iodine is at present little understood; but that it occasionally exercises very extraordinary power in the dispersion of morbid growths, is now generally admitted, at the same time that in the present state of our knowledge its apparent want of uniform success, and the terrible influence it exercises over the nervous system, even some weeks after its use has been discontinued, require great caution in its administration.

Half a drachm of the ointment of hydriodate of potass was rubbed in every night and morning, and five drops of the tincture given twice in the day for more than a fortnight, when the increased sense of fainting and diminution of the patient's strength obliged its discontinuance.

The beneficial effect occasionally produced by the internal use of the caustic alkali, especially in steatomatous tumours, suggested the propriety of employing this remedy. Twenty drops of the liq. potassæ were ordered to be taken thrice daily, in a little barley water, this quantity being gradually increased to twenty-five minims five times in the twenty-four hours, which was borne without the slightest uneasiness. During three weeks that the use of this remedy was continued, a sensible amendment was perceived. Strength increased; the skin became of a healthier colour and the tumour certainly was somewhat diminished. In consequence of this amended state, the patient left town for his seat in the country, in the middle of July. On the first of August he returned to London, having perceived an increase in the tumour during the preceding two days, and having experienced a return of the rising of tasteless fluid into his mouth, a symptom which had wholly left him for several months.

My attention having in the mean time been called to the case of Row, which I have first detailed to the Society, I was satisfied that the malignant growth was in the stomach itself, and accordingly informed the patient's friends. This opinion was confirmed in consultation by Mr. Brodie and Dr. Chambers. After the patient returned to London, the extr. conii

and the liquor arsenicalis were employed in full doses, but without any perceptible good effect. The patient continued to decline, his hands and feet were cedematous, and his strength became so greatly impaired that he required the support of considerable quantity of stimulants, in order to maintain life and warmth.

After growing weaker and weaker through the month of September, he expired on the 2d of October, without pain, having experienced a feeling of complete exhaustion, and presented an appearance of the utmost emaciation for several days previously.

The most singular circumstance attending this case was the perfect manner in which digestion was performed during the progress of so extensive a disease of the stomach. The patient's diet consisted of broth, arrow-root, plain animal food, and white fish, and as the disease advanced, he was permitted to drink weak brandy and water with his dinner, which added greatly to his comfort by counteracting the extreme sensation of debility. At no period of his disease did he experience any pain after taking food; at no period was his food returned by vomiting. The only circumstance which could draw the attention of the physician to disease of stomach was the water-brash, but this occurred rarely in very small quantity and was attended with no pain. The appetite continued natural until two days before death.

The body was opened twenty-seven hours after death by Mr. Brodie, assisted by Mr. Caesar Hawkins. On the external surface of the body several spots of purpura were perceived, and a tumour was easily felt through the parietes of the abdomen, with an opening in its centre, a little above and to the left side of the umbilicus, discharging some dark purulent fluid. The cavity of the abdomen contained about three quarts of water; on the removal of which, the tumour was found to be formed by the stomach, adhering extensively to the parietes, to which the transverse part of the colon and the omentum were also joined. The stomach was opened on the posterior part, and the cardiac portion and duodenum were found to be quite healthy, the pyloric half alone being the seat of disease. It appeared to consist of a thickening of the coats of this part of the stomach, in some parts above an inch in thickness, with an irregular tumour growing from its whole circumference, of the nature of fungus hæmatodes. The whole interior surface was ulcerated, and several portions of the tumour projected into the cavity of the stomach. The tumour was soft, and highly vascular in the inner part, and gradually became firmer and whiter towards the peritoneal surface, whence several white bands ran in an irregular manner towards the interior of the tumour. The anterior part of the stomach was the thickest, particularly where it adhered to the muscles of the abdomen; and in it several abscesses were discovered, one of the largest of which was the cavity in which the opening on the surface of the ab-

domen terminated. The œsophagus near its junction with the stomach contained a small cyst of fluid, resembling an hydatid in appearance, and of the size of a filbert. The liver was rather darker than usual, but otherwise healthy; except that in the left lobe several tubercles were observed of the size of a pea, of a white colour, and of the consistence of soft cartilage. All the other viscera appeared sound.

**CASE III.**—The following case I had an opportunity of observing under the care of Dr. Hewett, physician to St. George's Hospital, who has obligingly permitted me the use of his notes in his hospital case-book.

John Rae, æt. 40, applied to be admitted Wednesday, September 12th, 1827. About fifteen weeks ago, being in robust health, he fell suddenly on his back from a height, and was taken up insensible. He soon recovered his faculties, and did not appear to have sustained any serious injury. About twelve weeks ago, he experienced a difficulty in the digestion of his food. To use his own expression, "his victuals did not appear to digest properly, but to stop for three or four hours at a spot" which he pointed out, and which corresponded with the cardia; the food then passed onwards without being rejected by vomiting. These symptoms were removed by some pills, in the course of three or four weeks. Pulse 100, regular and soft; tongue foul. He has had no evacuation from his bowels since last Sunday week, with the exception of one costive stool, after having taken castor oil. On Saturday afternoon he fainted twice from weakness, not from pain, while making ineffectual efforts to pass the evacuation. (Capiat ol. ricini ℥ij. statim, et 4tis horis donec semel respond. alvus. Injiciat enemata oleos. ℥ij. post horam unam. R. Mist. camphor. ʒxj.; sp. æther. sulph. ʒss.; syrapi ʒj. M. ft. haustus appropinquante syncope sumend.)

13th. Three evacuations from the medicine, of a dark muddy colour, but presenting no traces of blood. Urine reported to be high coloured, nearly resembling porter in appearance. Bowels have been torpid during the last nine or ten weeks, but he did not notice the colour of his evacuations. He does not now experience any pain, except on forcible pressure about the epigastrium. Towards the right as well as the left hypochondrium and umbilicus, there seems to be some induration of the stomach, and perhaps also of the liver. Pulse 100, regular, soft. Skin natural. Tongue muddy, no yellowness of conjunctiva; he has a peculiarly exsanguine appearance, but has never had any hemorrhage. That the tumour is of a malignant character, is rendered probable by the expression of countenance, the rapid emaciation, and general progress of symptoms. Capt. ol. ricini ʒij. c. m. Injiciat enema oleosum vespere. R. Submur. hydrarg. gr. j; extr. conii gr. viij.; M. ft. pilulæ ij. 8vis horis sumend.

14th. The examination to-day leaves no doubt that scirrhus of the stomach exists to a considerable extent, more particularly affect-

ing the pyloric portion. Tongue clean and moist, his appetite is good, and he feels no inconvenience after swallowing his food. He was now ordered fourteen drops of the solut. hydriodatis potassæ (hydriod. pot. ʒss. aq. distill. ʒi.) every six hours. Extr. conii gr. vj 6tis horis intermedium; repet. enema oleos. interm. calomel. (Beef tea.)

He pursued this plan, augmenting gradually the medicine, on the 19th to twenty drops, on the 27th to twenty-four drops, with at least no disadvantage. His bowels were regular, he was entirely free from pain, and he relished his food. On the 1st of October he was attacked with diarrhœa, which caused the use of the solution to be suspended, and opiates substituted for it. The purging, though occasionally restrained, continued to increase, and he died, apparently exhausted, on the 15th of October.

In the different examinations which were made subsequently to the 14th of September, it was observed that a great mass of the tumour varied its situation according to the position of his body, descending nearer the umbilicus if he sat up in his bed, and nearer the right or left hypochondrium, according as he lay on his right or left side. Dr. Hewett also pointed out the peculiarity of the continuance of the appetite and the exemption from vomiting in this case, as he had done in the case of John Clapp, who had died some months previously, while under his care in the hospital, with a similar but still more extensive disease of the stomach and duodenum.

*Setio Cadaveris.*—The greater part of the stomach seemed healthy, but at the pylorus a tumour was found, as large as a man's fist and nearly globular in shape, occupying the anterior and lower part of the pyloric extremity. A small part projected over and was attached to the duodenum, but most of the tumour formed part of the circumference of the stomach in the situation mentioned, leaving the posterior and upper part of the pylorus free from disease, and not even thickened. The tumour, near its circumference, was hard and white in texture, apparently attached only to the outer part of the coats of the stomach; but in the inner surface of the diseased mass the coat had ulcerated, and a sloughy mass was exposed having a cavity in the centre which communicated with the cavity of the stomach, with irregular projections of a dark brown or blackish colour. The arch of the colon adhered slightly to the tumour, but was unaffected by the disease. A portion of the œsophagus which was cut off with the stomach, and which was about one inch and a half in length, was very much thickened and hardened in its muscular texture, the mucous coat being still healthy, and the cardiac portion of the stomach was also free from disease where it joined the œsophagus.

The liver had a large quantity of soft white tubercles, with yellow portions intermixed, and in some parts more vascular than usual. Where they were distinct their diameter was one or two inches, and more vascular in the

centre; but many of these had coalesced, so as in some parts to lose the tubercular appearance. They were soft and easily broken down, and could readily be detached from the rest of the liver, which was quite healthy in appearance; and the whole liver being enlarged, the actual quantity of healthy structure was not much less than usual.

The transverse branches of the vena portæ seemed quite choked with a similar diseased structure, which adhered to the inner coat, and extended into many of the smaller branches, so that if a portion of tumour was torn, the vessels filled with the new structure could be separated from the actual tubercles, and were seen extending like cords into the healthy structure of the liver, although in a section it was difficult to distinguish the cut surface of the tumour in the vessels from the tumour which was external to their coats. It was difficult to see any channel by which the blood could have passed, so completely were the branches of the vena portæ obstructed; yet in the healthy part of the liver the vessels were seen to be still pervious.

In the last two cases, it is to be observed, that tubercles were found in the liver, in the second case in a crude state, and in the last in a very advanced stage of the development. These are exactly the tumours described by the French authors, M. Laennec and Andral, under the name of (*tumeurs encephaloides*), and of the symptoms of which, during life, with the appearances on dissection, the latter author has lately given a very detailed description in his valuable work "*Sur les Maladies Abdominales*."

There can be little doubt that the disease in the stomach and that in the liver are of the same nature, modified only by the structure in which they are found, and (unlike true cancer, which appears often a local disease, affecting parts in juxta-position, and, secondarily, the constitution,) to be the result of the same action of vessels in different structures at the same time. This would appear from the following facts.—1st. By the observations made by Morgagni, Farre, Langstaff, Wardrop, Bayle, Laennec, and Andral, of the simultaneous occurrence of this organic disease in different viscera. Dr. Farre has related a case where tubera of a structure similar to those which I have shown to the Society were found in the brain, bronchial glands, liver, and kidney, in the same individual. M. Bayle relates an instance where he found them in the brain and lungs. In the collection of preparations of morbid parts in the possession of Mr. Brodie, there is a specimen of the fungus hæmatodes of the liver (*tubera diffusa*), scirrhus of the breast, and a disease of the uterus, apparently similar to what has been described by Dr. Clarke under the name of cauliflower excrescence, taken from the same individual. Another circumstance worthy of observation, in the third case, is the obstruction in the transverse branches of the vena portæ, by a deposition of matter similar to that which composed the tubera in the liver. In Mr. Langstaff's

excellent paper on fungus hæmatodes, in the eighth volume of the Transactions of the Society, several cases are related (pages 285, 304,) in which the veins in the immediate vicinity of the diseased structure were found choked by a similar deposition.

This subject, viz. the matter of fungus hæmatodes being found in veins unconnected with alteration of their coats, and in the centre of large coagula, consequently, probably, arising from an alteration in the chemical composition of the blood, has recently attracted the attention of the Académie de Médecine at Paris, in consequence of two papers on the subject by M. Velpeau.\*

At the meeting of the Academy M. Beclard stated that he had found this formation, in one case, in the interior of a clot which filled the heart and principal blood-vessels. In another case a similar mass was found filling the iliac veins and vena cava.

What, then, is the nature of the disturbance in the due performance of the laws of the economy, in its circulation, absorption, or secretion, which immediately precedes the formation of these diseases?

It appears to me to present none of the ordinary phenomena of inflammation, nor is its termination in any manner similar to the terminations of that morbid process as far as they are at present understood, as effusion, suppuration, deposition of lymph, or hepatization. It arises often without the unfortunate patient being aware of its commencement, and proceeds without pain, redness, or swelling, or heat of the affected part, these not being observed until its size, or encroachment upon neighbouring parts, produces secondary attacks or alterations in contiguous textures, which rouses the attention of the patient. The exsanguine appearance of the patient, even at a very early period, and the uncommon depression of vital power which he experiences, would lead to the belief of a constitutional cause, either an alteration in the constituents of the blood, from which these diseased products are separated by the ordinary secreting power of vessels, or from a morbid alteration in the secreting powers themselves, or from both of these causes.

Before concluding, I may be permitted a few remarks on the treatment of a disease, which consists only, in our present state of knowledge, in the alleviation of pain, or in directing means to retard its progress. In several cases I have found pain and vomiting, when they attend this affection, effectually relieved for a considerable time by the administration of the prussic acid.

CASE I.—A middle aged woman was attacked with all the symptoms of this complaint; constant pain; aggravated on taking either food or medicine, which resisted all usual remedies. Venesection, leeches to the pit of

\* *Revue Médicale*, February and March, 1825.

the stomach, blisters, full doses of conium, hyoscyamus, belladonna, and opium, failed in giving the smallest relief. Two minims of the prussic acid given twice in the day procured a calm of a week's duration, and afterwards, whenever repeated, some relief was obtained. The dose was carried up to  $\text{m. iv.}$  thrice in the day, beyond which quantity it appeared dangerous to employ it.

CASE II.—A woman,  $\text{æt. 45}$ , who had been exposed to severe affliction, complained to me of pain in the region of the stomach, aggravated on taking food; constant vomiting; a hardness was perceptible in the great curvature of the stomach: no ordinary preparation had relieved her sufferings, which had lasted four months. Two minims of the prussic acid, directed thrice daily, had the effect of producing an entire suspension of the symptoms during a fortnight.

It ought to be stated here, that the preparation used was that known under the name of Scheele's medicinal acid.

In cases where pain and vomiting are not present, I should be induced to employ large doses of the liquor potassæ, from the advantage derived temporarily in the second case, even at an advanced period. I need scarcely observe, that this remedy is only adapted to similarly insensible tumours. Rest appears to be essentially necessary, exercise uniformly promoting the rapid increase of the disease.

From the *Lancet*.

ON THE SAFETY AND NECESSITY OF AMPUTATION IN SPREADING MORTIFICATION. By GEORGE BUSHE, M. D. of the Royal College of Surgeons in Ireland, and Assistant Surgeon to the Forces.

The catalogue of surgical writers can boast of no better names than Pott and Sharp; to them we are indebted for many sterling improvements in surgical science; but yet these celebrated men committed errors, which have been partly rectified by succeeding authors. When we read their sentiments on the operation of amputation for mortification, we cannot but regret that they did not adopt a more impartial line of research; which, when aided by the vast experience they must have enjoyed, would have induced them to alter what they wished might be received as an axiom, viz., that amputation should never be had recourse to, in mortification, until the deadening process had ceased, and a line of separation had been established. To substantiate this maxim it was urged, that no matter what care might be taken to distinguish between the diseased and healthy parts, the operator very likely would be deceived, by finding that, though the integuments appeared to be, and indeed were perfectly sound, the deeper-seated parts, as the cellular tissue, and muscles surrounding the bone, were in a state of gangrene; and that if even the divided parts were healthy, the same morbid affection would, in all probability, attack the stump, and soon

prove fatal; finally, that the shock of the operation, when added to the debility produced by the disease, would seriously endanger the patient's life. These opinions, derived from such high authority, naturally impressed the surgeons of England with a determination only to amputate for mortification, after a line of separation had been established;\* and even in the present day the same doctrine is publicly inculcated, by some of the first teachers in the empire; and it is merely from a conviction of the unstable foundation upon which this doctrine has been erected, that I am induced to write this short paper; which has for its object to prove the necessity there exists for, and the safety that may follow amputation, in cases of spreading traumatic gangrene.

B. Larrey, in his "*Memoirs de Chirurgie Militaire*," tells us, that at Toulon, in 1796, he amputated in a case of spreading traumatic gangrene, with the best success; and that in 1801, at the siege of Alexandria, in Egypt, he again repeated this practice with the same success; and that from this period many of the surgeons of the first rank, in the French army, pursued his example; and, from their experience, we must conclude, that so far from being a hazardous, it was generally a safe, line of practice. But the Baron is not the only person who has recommended this derivation from the maxim of Pott and Sharp, for Miher, who wrote in 1799, distinctly says, that mortification succeeding to gun-shot wounds, is the only case that requires *immediate* amputation; and Mr. Lawrence and A. C. Hutchison, both relate successful cases where amputation was performed for traumatic mortification, before a line of separation had been established.

I have now twice observed amputation terminate favourably, when performed for spreading traumatic gangrene; one case was operated on by the late Professor Todd; and the other was a patient of my own. The subject of Mr. Todd's operation was a wretched woman of the town, about 25 years of age, and greatly emaciated by dissipation; she was admitted into the Richmond Hospital, Dublin, in the summer of 1823, on account of a compound dislocation of the left ankle-joint, attended with extensive laceration of the ligaments; to which, in the space of eighteen hours, succeeded spreading mortification, accompanied with general sinking of the vital powers. On the beginning of the third day, amputation was performed below the knee; and a complete recovery followed.

The other case, that which fell more im-

\* No person can more admire men, who have improved science and literature, than I do; but I could wish that others would agree with me, in not even receiving, as correct, without a strict inquiry, matter from the most renowned in our profession; for I do think, that many unpretending individuals may detect serious errors in the writings of the most notorious members of the faculty.

mediately under my own care, and altogether bore a disheartening aspect, I shall, by way of concluding this paper, detail more at length.

Sweetman, æt. 28, of sallow complexion, and delicate frame, by trade a labourer, on the 12th, Dec. 1827, whilst engaged in attending masons on a scaffold, 47 feet high, (near Chatham) fell to the ground; by which he sustained the following injuries, viz. dislocation of the right femur upwards and backwards; dislocation of the knee backwards, and compound luxation of the ankle-joint of the same member. He was immediately removed to the parish hospital, where he was soon visited by surgeons Hope and Bryant; who reduced the luxation of the knee, closed the wound of the ankle, and placed the limb in the semi-flexed position, having previously made many fruitless attempts to reduce the luxation of the hip. After reaction had taken place, blood was freely abstracted from the arm, and subsequently the usual treatment was steadily pursued. From the date of the accident, until the 15th, he went on favourably; but at this period he complained of excruciating pain; the limb became tumid, and a black spot appeared in the neighbourhood of the wound. Bark and opium were administered in large quantities, but they did not appear to have any beneficial effect, as the deadening process extended, accompanied with dreadful suffering; and on the evening of the 18th, (when I first saw him) the medical gentlemen above mentioned, and Messrs. Robertson and Tribe, were present. When we found that the limb was quite livid, even above the knee, and covered with large vesicles containing a dark-coloured fluid, attended with great tension and tumefaction of the thigh, and an emphysematous condition of its cellular tissue, and also that of the lower part of the abdomen. His face was pallid; skin rather cold; forehead and breast covered with a clammy perspiration; tongue tremulous; and his pulse weak, soft and quick. Seeing that death would certainly take place in a few hours, as the mortification was extending rapidly towards the trunk, and the vital powers quickly sinking, nothing appeared to forbid the only alternative, (amputation) but that the shock of the operation, when added to the functional depression under which he laboured, might prove suddenly fatal.—However, as his friends were urgent for the operation, I immediately removed the limb below the trochanters; during the operation the loss of blood was very inconsiderable, and no further depression of the vis vitæ ensued. He had a large opiate, and warm beef-tea *ad libitum*. On the following day, when I visited him, I was informed that he had a good night, having slept seven hours; his skin was natural; pulse more full, firm, and less quick. The stump was free from unnatural tension, or other untoward appearances. It would be useless to follow this case further, as it presented nothing remarkable; the stump healed rapidly, and he was soon discharged the Hospital, well.

Chatham, March 1, 1828.

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From the Repertoire General de Anatomie, &c.

OBSERVATIONS POUR SERVIR A L'HISTOIRE DE L'HYPERTROPHIE DU CERVEAU. Par M. DANCE, Agrégé à la Faculté de Médecine de Paris.

Hypertrophy of the brain has been recently treated of by some authors as a primitive disease of that organ; but are we possessed of any well defined examples of this affection? Is the brain, surrounded as it is, by an unyielding osseous envelope, susceptible like other viscera of a morbid augmentation of nutrition? What are the anatomical characters of this lesion? and what the symptoms by which it is accompanied? These are queries which remain to be solved, and which lead to the supposition, that the disease in question has been admitted rather as a possible occurrence, than as a demonstrated fact. By hypertrophy we do not mean to designate the augmentation of volume resulting from inflammation of the brain, from serous or sanguineous congestion of its substance, or from extravasation into its cavities. In these cases, the afflux and stasis of the fluids increase the apparent volume of this viscus, but they are not incorporated or identified with its substance, as happens in genuine hypertrophy, which consists essentially in a preternatural increase, either in number or volume, of the constituent molecules belonging to each organ. Now, the brain is actually susceptible of undergoing this aberration of nutrition, and consequently of acquiring a volume disproportionate to the capacity of its osseous covering. The cases which follow appear to establish the reality of the disease, but are not sufficiently numerous to furnish materials for a complete history; they will serve as a foundation upon which a superstructure may be raised, as new facts accumulate; we shall content ourselves with the reflections and deductions which immediately flow from them.

Case 1.—Lespinats, a lapidary, æt. 26, was admitted into the Hôtel Dieu, March 11, 1826. He was a man of ordinary stature and regular conformation, hair black, complexion pale but clear, muscular system not very strongly marked, intellectual qualities well developed. At the age of 14 he received a blow with a hatchet on the top of the head while in the act of boarding a vessel; he was not deprived of consciousness at the time, but some days afterwards was attacked with delirium, and remained seven months indisposed from the consequences of the injury. (No mark of the injury is at the present time perceptible.) From this period, but only in very cold weather, he has been liable to pain in the head, to mitigate which, and to guard against the cold, he habitually uses a fur cap. Six years ago he was admitted into the Hôpital St. Louis for prurigo, of which he was entirely cured by the warm bath. Two years later he entered La Charité for what was called a swelling of the face, arising probably from erysipelas; he was discharged cured at the expiration of fourteen days. He asserts positively that he has never contracted any

venereal disease, or laboured under any chronic affection of the skin, such as darts. During the last year he has been troubled with epistaxis, occurring two or three times a week; the hemorrhage was so considerable about four months ago, that the application of ice to the head was necessary to arrest it; since which period, the hemorrhage has occurred less frequently, and in diminished quantity, without any augmentation of the pain of the head. The patient pursued his ordinary avocations, had a good appetite, and his health was only interrupted during cold weather by the pain just mentioned. Three weeks ago he was attacked with headach, tolerable in the first instance, but soon accompanied with troublesome throbbing, and a humming noise in the ears; his appetite failed, bilious vomitings supervened, and he with difficulty continued his employment fifteen days longer. During the last five days the headach had greatly increased in intensity, accompanied with constant and harassing insomnia. Five leeches were applied behind each ear, and twelve papers of calomel, the dose of which is unknown, was administered.

*Symptoms.*—Mercurial fetor of the breath, and whitish superficial ulcers upon the gum of the lower jaw, arising probably from the calomel; violent frontal cephalalgia, without elevation of the temperature of the skin, or flushings of the face; pulse small and slow; anxiety and continued complaints relative to the pain of the head; tongue moist and natural; constipation. *Prescription.*—*Barley water, pediluvium, enemata, and diet.*

On the evening of the 12th, he had a violent paroxysm of oppressive headach, during which he was continually changing his position, and crying out *my head, my head*; he complains of coldness of the extremities, the eyelids are closed as if to exclude the light, his pulse is small, slow, and somewhat irregular; the surface preserves its ordinary temperature, the face is neither hotter nor more flushed than ordinary. *Sinapisms to the feet.*

13th.—Severe pain in the head, with violent exacerbations, in the intervals of which the pain is supportable, but never ceases entirely; he complains of a sensation like that of a stream flowing in the interior of the cranium, with humming in the ears; the eyelids are closed, skin cool, pulse small, slow, and slightly unequal; tongue large and moist; breath less fetid than before; the ulcerations are healing. *Pediluvium with mustard, purgative enemata, diet.*

In the course of the day two or three similar paroxysms occurred with equal violence; toward evening, the pulse beat only from 45 to 50 pulsations per minute.

14th.—Groaning; complete insomnia during the whole night, same slowness of pulse, forcible contraction of the eyelids and all the superior part of the face. At nine in the morning, a paroxysm supervened, similar to those of the preceding day; the patient was incessantly seeking relief in change of posture; sometimes he sat up in bed; at others, he

pressed his forehead against the pillow, and appeared more calm when strong pressure was made upon his head; he uttered cries expressive of pain, and believed himself doomed to certain destruction; his cheeks were rather more flushed than yesterday, but the temperature of the skin and slowness of the pulse remained as before. *Warm bath, with cold effusions upon the head, infusion of linden-flowers and orange leaves, pediluvium with mustard, diet.*

The pain was somewhat mitigated in the first instance, by the cold effusions, but returned with increased violence when the patient had resumed his position in bed.

18th.—Short sleep, abatement of the intensity of the headach, same slowness of pulse. *Cold compresses to the head, pediluvium, enema, pisan us before.*

The succeeding days, the paroxysms appeared to diminish in violence, the patient obtained some moments of repose, and complained less vehemently. But on the evening of the 20th, a paroxysm occurred as violent as any of the preceding; his pulse was feeble, and beat only fifty strokes a minute; this state continued through the whole night. On the 21st he complained of intolerable suffering in the interior of the head, he was unable to bear the light, the pupils were contracted, pulse slow; the temperature of the skin gave no indication of febrile excitement.

The bath, with cold effusion over the head, was again proposed, the preceding bath having been followed by some alleviation. At three in the afternoon he walked to the bath and remained in it three quarters of an hour; a single cold effusion was made upon his head. At his exit from the bath he swooned, fell into the arms of his attendants, and died in less than fifteen minutes. The house surgeon, suddenly summoned to his assistance, found him agitated by some convulsive movements; the pupils were largely dilated.

*Necropsis 41 hours after death.*—Notwithstanding the long space of time which had intervened, the rigidity of the body was very great. There was no trace of fracture of the skull, and the head presented nothing remarkable in relation to its form and dimensions. The dura mater was applied upon the cerebral substance, and appeared to be distended beyond measure by the swelling of the subjacent parts; its colour was generally violaceous; very little blood was found in the sinuses; the edges of an incision, made into this membrane, separated spontaneously, and permitted the brain to protrude in form of a hernia. All the circumvolutions of the brain, especially at its superior part, had acquired a great increase of size, almost double their natural volume; they were flattened, and so crowded one against another, that the intervals which naturally separated them were with difficulty distinguishable, so that the convexity of the hemispheres presented an uniform surface, without either elevation or depression. The arachnoid and pia mater, intimately adherent to each other, and to the circumvolutions, appeared thinned

by their close approximation; these membranes were not injected, and could not be detached from their adhesions without rupturing them. The whole substance of the brain resembled the white of an egg hardened by boiling; its weight and density were considerable; it did not yield upon pressure; subjected to moderate extension, it became elongated without rupture, and returned upon itself like an elastic body when the extending force was withdrawn; no trace of vessels, or red points were perceptible; on the contrary, all the cortical substance appeared paler, and the medullary matter whiter, than natural. Not a drop of serum was found in the ventricles; their cavities were diminished to half their ordinary size, by the approximation of their parietes. Lastly, the brain and cerebellum having been removed, not a drop of fluid was found in the cranial fossæ; all the surface of the arachnoid was as dry as parchment. The tuber annulare participated, in a slight degree, the condition of the brain, but the cerebellum and spinal marrow presented nothing unusual in their volume, consistence and colour. The other organs were in a state of perfect integrity.

This disease is very distinct from any other cerebral affection hitherto described, and assuredly merits the name of hypertrophy of the brain, if regard be had to the fundamental characters of the lesion. Augmentation of volume, of weight, and of consistence, without apparent disorganization, are the characteristics generally assigned to hypertrophy, and all these phenomena were present in the case narrated. The brain had acquired such an increase of volume, that after having accurately filled all the cavity of the cranium, it had violently reacted upon its parietes, as is evinced by the flattening and intimate approximation of the circumvolutions, the coarctation of the ventricles, the thinning of the membranes, the contraction, and even the occlusion of a great number of cerebral vessels, a circumstance which may serve to explain the dryness of the arachnoid, and the absence of injection or of red punctation of the whole cerebral mass. This state of condensation, taken in connexion with the increase of the constituent molecules of the brain, will account also for the increased specific gravity which this viscus presented.

But what connexion can be traced between this singular aberration of nutrition, and the symptoms which we have observed? Does it not seem, in considering the importance of the functions of the brain, that intelligence, mobility, and sensibility, should have been greatly disturbed? The patient, notwithstanding, experienced no considerable derangement of the faculties. Shall we say that this aberration of nutrition, being only an exaggeration of the natural condition, must not be assimilated, in regard to its effects, to changes which are the consequences of a process essentially morbid? Shall we say, moreover, that the pressure which resulted from this preternatural increase of the brain, acting very gradually, like certain tumours, which are developed within the cranium,

ought to be insensible during a long space of time? Or that this pressure, exerting itself equally upon all parts of the brain, should have induced general paralysis, almost immediately followed by death? These explanations, however, will not appear very satisfactory, when we shall have compared the preceding case with those which are still to be related. We need not be surprised at the extreme violence of the pain, when we reflect on the great constriction, amounting almost to strangulation, which the brain must have undergone. Does it not appear that the patient endeavoured to lessen distention excited by the brain against the parietes of the cranium, when he sought relief by forcibly pressing his head against the pillows? Do not the slowness and smallness of the pulse indicate that the nervous influence, necessary to the continuance of the circulation, was diminished by this state of compression?

What account shall we render of the previous history given by the patient? Did not the blow upon the head, which he received at the age of 14, induce a morbid predisposition in the brain, although it left no impression upon the bones of the cranium? Does not the headach, which from this period made its appearance, indicate the ancientness of the disease, and the slowness of its progress? Did not the affection of the face, for which he was treated during fifteen days at La Charité, concur to augment this determination towards the encephalon? Lastly, does not the epistaxis to which he was subject, point out a congestive tendency towards this part, and was not the aggravation of the symptoms the consequence of its sudden suppression? The influence of all these causes appears to us very probable, and from their *modus operandi*, one would be led to suppose that the brain had long been the seat of a chronic inflammation, or rather of a state of sur-excitation, suited to accelerate its mode of nutrition; for true inflammation ordinarily presents other characters, and we believe that the alterations of the assimilative function are not absolutely identical with those of inflammation, although both may recognise the same causes.

However that may be, in proportion to the rarity of a disease should be our endeavours to discover its distinctive symptoms; the following are those which it appears to us should be taken into consideration in the present instance: the direct or indirect and protracted action of several causes upon the brain, cephalalgia returning in violent paroxysms, during which the patient uttered cries and groans, sunk his head in the pillows, and was incessantly changing his posture; a cruel and harassing insomnia; a pulse slow, small, and sometimes unequal, descending as low as 45 or 50 pulsations in the minute; contraction of the eyelids, as if to prevent the admission of light, while neither the temperature of the skin, nor suffusion of the face, gave any indication of determination to the brain.

As distinctive symptoms, however, these do not appear to possess much importance; seve-

ral of them are found in other diseases, and we frankly avow that to us they appeared to indicate a fungus of the dura mater, or some other tumour pressing against the parietes of the cranium; moreover, the following cases teach us that alterations identical in their nature, do not always give rise to the same phenomena.

*Case 2.*—A young man, æt. 24, of ordinary stature, and apparently lymphatic temperament, was admitted into the Hôtel Dieu, January 15, 1823. His physiognomy, his expression and his manners, announced some degree of stupidity and idiotism; he answered slowly and in monosyllables to all our questions, and his replies were rarely correct; he complained acutely of his head, but his complaints were only transitory; he had no fever, his pulse even appeared slower than natural, and there was no derangement of any function except that of intelligence; sometimes his thoughts appeared to ramble, and he spoke when alone, of things relating to his occupation, as if some one were present to reply to his questions; most commonly I found him sleeping, hidden beneath the bed clothes, awakened with difficulty, and regarding me on such occasions, with an air of imbecility; a head also badly organized, was little adapted to enlighten us respecting his previous history. His mother came on one occasion to the hospital, and stated that he had not been *a very intelligent man*; that he had never given signs of complete derangement, that he had never been attacked with convulsions, but that he frequently complained of his head; and lastly, that he had been fifteen days in the condition above mentioned. Some antispasmodic medicines were given, a blister was applied to the nape of the neck, and for some days he appeared to grow better. His physiognomy was more open, he left his bed, walked through the hall, ate with appetite, and conversed very rationally with his comrades; he had no fever.

On the sixth day of his admission, he was suddenly attacked with convulsions, with rigidity of the extremities and grinding of the teeth: we were sent for immediately, and upon our arrival, found that the paroxysm had ceased, it was not protracted beyond three minutes; the patient was composed, and complained only of soreness of his extremities. The two following days, he experienced five similar paroxysms, occurring both during the night and day; they were preceded by copious vomitings of greenish matter, and did not last longer than the first. (Twenty leeches were applied to the mastoid processes, and sinapisms to the legs.) The last paroxysm made its appearance on the morning of the ninth day: there was general relaxation, flaccidity and insensibility of the extremities, profound coma, dilatation of the pupils, stertorous respiration, and occasionally convulsive agitation of the extremities; this state of things continued during the day, and terminated in death at seven in the evening. Three hours before, we visited him, and found him very nearly in the same condition; the right eyelid had fallen down, and

the left was elevated, the pupils were insensible and largely dilated, noisy respiration, coldness of the extremities, the inferior, were so rigid, that they could scarcely be flexed; a frothy saliva issued from the mouth, and lastly, the pulse could not be felt.

The body was examined twenty-four hours after death; I shall only point out the principal circumstances which were observed. The meninges *applied immediately upon the brain*, appeared too small to contain it. All the circumvolutions of the brain presented a *very marked flattening*, and so closely were they approximated, that they appeared glued together. The substance of the brain itself was *very firm and compact*, such as it appears when prepared for anatomical demonstration; it presented also a *remarkable dryness*; neither *serosity* nor *reddish points* were observed upon the surface of incisions made into its substance; its colour was of a *duller white* than ordinary; no fluid was found in the ventricles, nor throughout the great cavity of the arachnoid; no other alteration was perceptible. The cerebellum retained its natural consistence; the spinal marrow was not examined; all the other organs were sound.

We had placed the preceding, in the number of anomalous cases, and at the period when it occurred, were unable to determine to what species of disease the symptoms and lesions just described, should be referred; we even omitted to speak of the condition of the meninges, and to note the dimensions of the ventricles. It is evident, however, that the anatomical characters of this disease, are entirely similar to those which we have indicated in the preceding case. In both, the same expressions are employed to designate them, and in both we find flattening of the circumvolutions, induration of the cerebral substance, dryness of its parenchyma, absence of red points where incised, and lastly, a duller white than ordinary; an additional proof is found in the healthy condition of the cerebellum in both instances, certain evidence that the induration of the brain was not a natural phenomenon. In these two cases, the whole substance of the brain was attacked with hypertrophy, a circumstance which should be particularly noted, for it sometimes happens that partial indurations are found, the consequence of inflammation, excited especially by the development of accidental tumours of very slow growth; but in such cases, the brain surrounding the indurated points will sometimes be found to have undergone the softening process, and the whole cerebral mass will never be found to have experienced such a homogeneous alteration. Admitting that inflammation is the cause of this alteration, it must also be admitted that it implicated at the same time the whole substance of the brain, affecting every part in the same manner and same degree, notwithstanding the difference of anatomical composition. Now it is not thus that inflammation proceeds. In the same organ, and especially in one so complicated as the brain, congestion, softening, suppuration and

induration are frequently all observed together. These considerations appear to us well adapted to elucidate the question, whether the lesions just described belong immediately to inflammation, or are the consequence of the perversion of the nutritive function. On the latter supposition it will be readily conceived how the brain, subjected to an uniform assimilative movement, may undergo an increase, and consequently acquire a consistence, equally uniform, if the superabundant materials of nutrition flow into its substance, an effect which it is difficult to connect with a cause so variable as inflammation.

But if, in both these cases, the lesions have been similar, the symptoms presented some differences which it may be useful to point out. Thus the first patient complained of constant pain in the head, subject to violent exacerbation; the second, while in the hospital, complained only temporarily of this pain; one appeared to enjoy the free exercise of his intellectual faculties, the other had a stupid expression, he was accounted a man of limited intelligence, and this state appears to us more accordant with the nature of the lesion. The latter experienced, several days before his death, several attacks of convulsion attended with loss of consciousness, in one of which he died; the former had no convulsions till the moment of his death. But along with these differences we also find some points of resemblance. Both complained of long continued pain in the head, transitory, but subject to return; and this paroxysmal exasperation of the disease appeared toward the conclusion, in one case, under the form of violent exacerbations in the cephalalgia, and in the other, under that of convulsive paroxysms; in both instances, bilious vomiting made its appearance some days before death, a symptom common to many cerebral affections; in both there was a remarkable slowness of the pulse, and death occurred unexpectedly in the midst of convulsions. We may add, that they were nearly of the same age, and endowed with similar constitutions; that in one the disease appeared to succeed to causes sufficiently manifest, and that in the other it arose spontaneously.

This memoir had been written for some time, and we had, the last year, communicated it to the Société Anatomique, when glancing over the *Journal de Clinique*, (tome 1er, No. 87,) we observed a case analogous to those we have just related; an analysis of it is given here in order that a comparison may be instituted between them.

[Here follows an account of the case already detailed in the *Journal of Foreign Medicine*, Vol. I. page 476, after which Dr. Dance continues.]

In this case also, the changes observed on dissection were characteristic of hypertrophy of the brain. The patient had for several months experienced a progressive derangement of the intellectual faculties, very analogous to the natural stupidity of the subject of our second case, but he had never been subject to convulsions; he was suddenly attacked with almost universal

paralysis of sensation, which continued for several days, an affection which did not supervene in the other cases till the last period in existence; lastly, no mention is made of pain of the head, or slowness of the pulse, symptoms so prominent in the cases which we have related; it should be stated, however, that the patient was seen only a few days before his death, and that very little information could be obtained of his previous condition.

The following case recently occurred in the Hôtel Dieu:—

A painter, æt. 30, of an athletic constitution, was transported to the Hôtel Dieu, March 15, 1828, a prey to epileptiform paroxysms, which recurred three or four times in the course of the day. We saw him on the morning following his admission; he appeared stupidified, spoke with remarkable slowness, and with extreme hesitation, apparently arising from defect of memory, for he was long in preparing his replies, and repeated them variously as if to assure himself of their correctness. He did not comprehend all the questions which were addressed to him, but gave us, notwithstanding, information conformable in some points to that which we obtained directly from his wife. His pulse was elevated and frequent, countenance flushed, the skin hot, respiration interrupted by involuntary sighing; there was no deviation of the labial commissures, or paralysis of the extremities.

During the last six years he had frequent attacks of *hemorrhage*, (*coups de sang*,) from the head; from time to time he was affected with *stunnings*, (*etourdissements*,) which were protracted to three or four minutes, when he lost the use of his senses, and resembled a man *completely stupid*. About three years since, having received a fall from the fourth story of a house, these stunnings were converted into genuine epileptiform paroxysms, recurring at first after long intervals, which afterwards were so much shortened, that he experienced four or five attacks in the course of the same day. His character was habitually sombre and taciturn; he was benumbed, (*engourdi*,) and frequently complained of head-ach and pain in the stomach. Venesection always had the effect of mitigating the paroxysms, and diminishing the frequency of their recurrence, while they were as invariably exasperated by the spirituous potations to which he sometimes resorted. On the 12th of March, three days previous to his admission, having drunk two glasses of punch, he was attacked the following night with the most violent convulsions. A physician was called in, who directed the application of twenty leeches to the epigastrium, from which the patient derived no advantage. He was bled on his entrance into the hospital, but the loss of blood appeared to aggravate his symptoms. During the night of the 16th three paroxysms followed each other in rapid succession; the patient uttered a single cry, his whole body became rigid, and his eyes were directed convulsively upwards; from the statement of the assistants,

these paroxysms strongly resembled those of epilepsy.

On the morning of the 17th, the patient had an alarmed expression of countenance; he did not reply to the questions put to him, and soon fell into a comatose condition. There was general loss of sensation and motion, eyelids closed, pupils contracted, mouth half open, tongue turned upwards with its point directed to the arch of the palate, stertorous respiration, profuse perspiration, pulse accelerated, exceeding 140 strokes in a minute, occasionally tension and rigidity of the extremities, followed by general collapses; death took place at 10, A.M.

The body was examined at 9, A.M. of the following day. The cadaveric rigidity was considerable; no trace of injury was perceptible on the cranium, with the exception of a cicatrix, limited to the integuments of the right side of the forehead. The bones composing this cavity were from four to five lines in thickness, and equally solid throughout; the external surface of the dura mater was traversed by numerous ramifications of the meningeal arteries; a considerable quantity of blood was found in its sinuses; it accurately enclosed the whole mass of the brain; examined on its internal surface it presented a general violaceous aspect, arising from its transparency, for this appearance was chiefly observed along the course of the subjacent vessels, which were filled with blood. The arachnoid presented a degree of aridity truly remarkable; not a drop of serum was found in all its cavity, neither in the ventricles, at the base of the brain, nor at the commencement of the spinal canal. The pia mater was intimately adherent to the substance of the brain, and ruptured very easily; all the circumvolutions were uniformly flattened and approximated to each other, so as to leave no interval between them.

The brain was apparently not so readily extensible as usual, but was not firmer upon pressure; it was neither moister nor drier than natural; the incisions, however, which were made into its substance were clean, and their angles could be maintained open without breaking them; some red points were perceptible, but their number was not considerable. The ventricles were dry and their parietes in contact with each other, without being, however, sensibly contracted. Nothing unusual was observed in the tuber annulare and cerebellum. Not a drop of fluid flowed from the spinal canal when the subject was stood upon its head; the medulla preserved its natural consistence, but opposite the second lumbar vertebra, traces were observed of an ancient fracture of this bone, which was flattened, and the spinous process was wanting, having probably been absorbed. At this point the vertebral column was slightly inclined forwards; towards its termination the spinal marrow appeared slightly softened; this fracture was probably occasioned by the fall to which we have alluded. All the other organs were sound, with

the exception of the stomach, which was slightly injected.

Apprehensive of being led astray in pursuing an unknown route, we are not certain that this case should be ranked with those which we have before related, but we deem it worthy of attention in itself, although the resemblance is not absolutely strict. Admitting its close analogy to epilepsy, a disease which is still a problem in pathological anatomy, it may be asked whether the condition of the brain and its membranes does not account for the phenomena which we have observed; whether this condition has not some relation to that which we have described as characteristic of hypertrophy of the brain? Does not the uniform flattening of the circumvolutions indicate that the substance of the brain had experienced a considerable turgescence, an augmentation of volume disproportionate to the capacity of its envelopes? Would not the ancients who employed the words *strictum* and *laxum* to explain the origin of many diseases, have seen in the dryness of the cavities of the brain and the rigidity of its fibres, the cause of the convulsions? It is true, that the consistence of this organ differed little from what is ordinarily observed, but may not hypertrophy present different degrees, according to the period of its duration? Shall we say, on the contrary, that the epileptic paroxysms to which the patient had long been subject, had, by *producing congestion* in the brain, deranged its mode of nutrition, and that consequently they were rather the cause than the consequence of the alterations which we have described? Would it be unreasonable to suppose that the bones of the cranium, ossifying prematurely and becoming preternaturally thickened, opposed the free development of the brain, by encroaching upon the cavity in which it is contained? We propound these queries, confessing, at the same time, our inability to resolve them; it may, however, be remarked, that the habitual torpor of the patient, the frequent headaches which he experienced, the state of stupidity in which he was found by us, together with the convulsions which terminated his existence, bear a strong resemblance to the symptoms observed in the patients attacked with hypertrophy of the brain.

*General conclusions.* It follows from the cases which we have related, 1st, that the brain is susceptible of a morbid increase of nutrition, characterized by the flattening and approximation of its circumvolutions, the coarctation of its ventricles, a preternatural consistence and whiteness of its medullary and cineritious substance, and a remarkable aridity of its parenchyma and of the cavity of the arachnoid, the texture of this viscus not appearing sensibly altered; 2d, that this hypertrophy has been invariably observed involving the whole mass of the cerebrum to the exclusion of the cerebellum; 3d, that far from augmenting the energy of the brain, it tends on the contrary to diminish, pervert, and suspend it, by reason of the pressure which it necessarily occasions against the parietes of the cranium; 4th, that the symp-

toms of this hypertrophy having varied in different subjects, they cannot yet form the basis of a precise diagnosis.

Nevertheless, this affection appears to develop itself very gradually under the influence of very obscure causes; as predisposing, we may mention, adult age (all our patients were from twenty to thirty years of age); and as exciting causes, contusions of the head, and frequent determinations towards this part, of which the first and fourth cases are respectively examples. But notwithstanding that all these causes appear to act in a phlogistic manner, we are inclined to the belief, from reasons elsewhere exposed, that this disease appertains essentially to derangement of the nutritive function. It makes its appearance by violent cephalalgia, subject to exacerbations, (first case), an obtunded condition of the intellectual faculties, (second case), a perversion of the same faculties, (third case), frequent stunnings accompanied by stupor, (fourth case); at a later period it gives rise to repeated attacks of convulsions, (cases second and fourth), or else it suddenly produces an almost total paralysis of sensation and motion (third case). The pulse is slow, and the temperature of the skin natural (cases first and second). Lastly, death unexpectedly supervenes during a paroxysm of convulsions, as occurred in the first, second and fourth cases.

This rapid analysis of the symptoms of hypertrophy of the brain, a disease which, in common with the greater number of those affecting this viscus, appears to be variable in its symptoms, will serve rather to indicate, than to supply the deficiency in our diagnosis. It is hoped that the cases which we have related will tend at least to evince the necessity which exists for further investigation.

From the Edinburgh Medical and Surgical Journal.

#### ON THE POWERS WHICH MOVE THE BLOOD IN THE CAPILLARY CIRCULATION.\*

In the work now quoted, which we believe is hardly known in this country, are contained some very curious experiments and speculations on the nature of the force which moves the blood in the capillary vessels. As the subject is one of much attraction at present, —when so many theories, all said to be founded on the basis of facts, have been in vain conceived with the expectation of accounting for the phenomena, —we are sure the reader will not regret the opportunity of becoming acquainted with another hypothesis, which certainly has the merit of novelty to recommend it, and which is also deduced, whether legitimately or not we shall not inquire, from facts of extreme interest, of unquestionable accuracy, and, so far as we remember, perfectly new. We are astonished that they have been

before the public since 1821, without having been noticed in any scientific journal of Britain, France, or Germany, which we have had an opportunity of consulting.

We shall commence with an account of his experiments, the merit of which ought to be considered quite apart from the use which the discoverer has made of them; and as the work which contains them is very scarce in this country, we shall relate the leading experiments nearly word for word. The original, it should be remarked, is in Latin.

The author, Ferdinand Frederick Reuss, in a preliminary essay on what he calls the *hydragogue*, or aquapellent property of electricity,\* has related several experiments to prove the existence of a new property in galvanism, the power, namely, of impelling water from the positive to the negative pole. He appears to have been led accidentally to study the phenomena in question, in consequence of observing the movements in the water of the particles of oxide of copper formed when the positive pole of his battery was made of that metal.

After describing the particulars of this motion, he proceeds to more satisfactory experiments. "This impulse," says he, "towards the negative pole is better seen, when the water is contained between two parallel glass plates three or four lines apart from one another, and when the wires are immersed at right angles to the water, being made of gold or platinum, and covered with glass tubes sealed at their extremities to the wires. It is convenient also to keep the wires steady yet moveable by thrusting them through two pieces of cork which rest on the edge of the vessel. If into the apparatus so prepared common water be poured, and the battery charged, the calcareous matter, which is separated by the decomposition of the salts of lime in the water, and causes turbidity, will not only show by its own movements a continual flow of the water from the positive pole in lines curved downwards, and then bending upwards to the negative pole, but will likewise delineate the course of the current by particles adhering to the surface of the glass plates." \* \* \*

"But the repulsive power of the positive and attracting power of the negative pole is seen much more satisfactorily, if the water in contact with each pole is separated by a stratum of porous matter, so that the particles which are impelled from one towards the other pole may pass through the interstices, while the cohesion of the interposed body may prevent other particles from passing in the opposite direction by their own gravity. Which conditions, in fact, are easily fulfilled

\* *Electricitatis Voltanæ Potestatem Hydragogam tanquam novam vim motricem, a se detectam, denuo proposuit, ejusque in naturæ operibus partes investigare tentavit S. O. Ferd. Fred. Reuss. (Lect. in Conventu d. vi. Oct. 1817.)*

\* *Commentationes Soc. Physico-Medicæ Mosquensis, II. ii. 327.*

by taking a tube of the form of the letter U, filling its curve with sand, and its straight, upright limbs with water. The two wires being now immersed in the water in the two limbs, and the galvanic circle consequently completed, the water will be observed slowly to sink in the positive, and rise in the negative end of the tube. With a voltaic pile composed of ninety-two silver rubles and as many plates of copper, a tube  $7\frac{1}{2}$  inches long, and wires made of platinum,—I found that in fifteen or twenty minutes, the water stood ten lines higher than before in the negative, and as much lower than before in the positive end of the tube. On interrupting the galvanic circle, the water soon returned to its original level, and on restoring the continuity of the circle, the transfer recommenced. Next day, fourteen hours after the experiment began, the positive limb was empty, the negative limb was full to overflowing. After observing that it continued thus for four days, I interrupted the galvanic circle again, and the water soon returned to its equilibrium of level."

"A similar but much more wonderful result was obtained when the interposed substance consisted of clay. I prepared a quadrangular prism of moist potter's clay, ten inches long, and two inches in breadth and thickness. At each end of this prism, and five inches apart from one another, I fixed, half an inch deep in the clay, two glass tubes three inches long, an inch in diameter, and open at both ends. I then poured into each tube an inch of water, and covered each of them (loosely, however, so as to allow a passage to the air,) with a cork, through which the two wires of the battery were passed down to the middle of the water. The pile consisted this time of seventy-four double plates, such as those used in the last experiment. The following phenomena took place. As soon as the electric circle through the wet clay was completed, which was indicated by the escape of air-bubbles from the water, the clay bottom of the positive tube began to swell and raise the sand, with a thin stratum of which I had covered it to keep the water from becoming muddy. In the course of half an hour the clay appeared softened to mud, a part of which pierced through the sand, and projected upwards like a little hill. By and by the pyramid discharged from its apex a muddy liquid, which ran down the sides like lava, and soon formed over the sand a layer of mud three lines in thickness. This beautiful appearance brought forcibly to my recollection the accounts given of the phenomena of mud volcanos. The explanation obviously was, that, when by the impulse of the positive pole the water was thrust towards the negative pole upon the clay at the bottom of the tube, the clay was softened, and the water in subsequently piercing the clay, threw the mud upwards and around it, just as a gimlet does while piercing wood. For half an hour no change was observed in the level of the water in either tube, and the sand and clay remained in the negative tube quite undisturbed. But when three hours had elapsed,

the level of the water in the negative tube had risen one line, in twelve hours it had risen two lines and a half; and in the meantime, the whole water of the positive tube had entered the clay, and the surface of the mud was about two lines lower than the *original situation* of the water. The following night the mud in the positive tube had sunk so far that the wire was not immersed in it; consequently the electric circle was interrupted. Nevertheless I found the level of the water in the negative tube a quarter of a line higher than before, and the clay in the positive tube was firm on its surface. The positive wire being then depressed so as to restore the continuity of the galvanic circle, the clay of that side gradually became firmer and more dry, while the water rose in the negative tube. At length in two days the latter had attained its highest level, namely,  $3\frac{1}{2}$  lines above its original surface, while the clay in the positive tube had become so dry as to crack. In two days more the cracks were larger, but the level of the water in the negative tube was not higher, probably because any farther increase it might have received was compensated for by evaporation. That no more than one-third of the water which disappeared from the positive appeared in the negative tube,—is to be explained partly by its having evaporated, partly by its having been diffused and retained in passing through the clay between the tubes."

In addition, therefore, to the properties already known to be possessed by the electric current, such as its power of effecting chemical combination and decomposition, of imparting magnetic properties to bodies, of exciting the muscles to contraction, it is also endowed with the power of impelling fluids in a direction from positively to negatively electrified bodies. This power, which Mr. Reuss denominates the *Vis Electricitatis Hydragoga*, he resorts to freely for explaining some terrestrial phenomena, such, for example, as the flow of springs from rocks too high for the water to reach by pressure from another source. But, in particular, he has applied it to explain the movement of the blood through the capillary vessels of the circulating system of animals. We believe it is unnecessary to spend any arguments here in showing that all the theories yet devised for that purpose have completely failed. The action of the heart is completely inadequate to maintain the capillary circulation, as is shown very satisfactorily by our author, Mr. Reuss, as well as by former physiologists; and with regard to the discoveries of Dr. Barry, notwithstanding what has been said in a former number of this Journal, the writer of the present notice will of course be permitted to express his particular opinion, (derived from the objections of Dr. Arnott,\* those of Mr. J. W. Turner,† and certain views and experiments of his own, which it would be foreign to the object before him to detail,)

\* Elements of Physics.

† Edin. Med. Chir. Transactions, iii.

that the force imputed by Dr. Barry to the venous suction generally, and to the suction power of the chest during inspiration particularly, has probably been overrated to a great degree by that ingenious experimentalist,—and that the force in question never can account for the passage of the blood through the capillaries.

Mr. Reuss's theory of course is, that the arterial system is in a state of positive, and the venous system in a state of negative electricity; and if he could establish this fact by observation and experiment, and not by hypothesis merely, on which its existence at present depends, there could be no difficulty in allowing that he had pointed out by far the most probable cause, and at all events *one* powerful cause of the capillary movements of the blood. As it stands, it appears to us peculiarly interesting, as adding to the facts and arguments formerly advanced by Wilson Philip, Dutrochet, Prevost, Dumas, and others, a new and important fact in support of the analogy, if not identity, of the nervous with the galvanic principle.

Mr. Reuss's paper, containing the experiments already detailed, is followed by another consisting of an elaborate exposition of his theory. The most important part of the second paper is an analytic view of the several powers which contribute, or have been supposed to contribute, to the movement of the blood in the circulating system generally,—an analysis undertaken by him with the view of showing that the whole of these powers combined cannot account for that part of the circulation which consists in the passage of blood through the capillaries,—and an analysis in which he is amply successful, and would have been so, though Dr. Barry had made his discoveries before Mr. Reuss wrote. The subsequent part of the second paper, containing the arguments with which Mr. Reuss endeavours to uphold his theory, is ingenious, but too hypothetical for the physiologists of Western Europe, and for British physiologists in particular.

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From the London Medical Gazette.

ESSAYS ON SYPHILIS. By JOHN BACOT, lately Surgeon to the First Regiment of Guards.

[Continued from page 314.]

In my last essay I adduced some proofs, in my opinion quite conclusive, that the venereal disease had been noticed in Europe at least ten years before the return of Columbus from the discovery of America; that it had then begun to excite serious uneasiness, on account both of its virulence and novelty, the former being sufficiently evinced by the death of several eminent persons in consequence; among whom may be reckoned Francis the 1st, Henry the 3d of France, and the Duke of Modena; the latter by the numerous and conflicting explanations of its origin, and the causes of its invasion at that particular period. A much

more difficult task, however, awaits me in endeavouring to point out the real source of the infection; it is, in fact, a Gordian knot, which not being able to untie, I must endeavour to cut; first observing, however, that I am not the only medical writer who has been glad to escape from this dilemma by the same short route. I will, therefore, mention to you a few of the opinions that have been held from time to time on this point. Sydenham supposes the venereal disease to be a native of Africa, and that it does not in reality differ essentially from the yaws; others, believing that it has been known in Hindostan from time immemorial under a name implying its origin from Persia, are of opinion that it is an Asiatic distemper; though there are not wanting authorities who deny this assumption, and affirm that it was conveyed to the East Indies from Europe by the Portuguese. Sprengel, who has bestowed much pains and displayed great learning in this research, is inclined to think that syphilis is a product formed from the combination of elephantiasis with the plague that ravaged Europe in the 15th century; whilst Swediaur, in direct contradiction to the tenor of his previous arguments, ends by suggesting, that perhaps syphilis may have travelled all round the globe; that it may have been nearly extinct in one country whilst raging in another, and that such as it was when it began to spread itself in Europe in the 15th century, it had infected humanity several thousand years before in Persia, in Thibet, and Hindostan; in short, the only point he seems to contend stoutly for is, that it was not brought from America to Europe. Now it is not unfair, I think, to presume, that when four or five different explanations of the same event are given, that nothing in reality is known about the matter—a remark that appears to be peculiarly applicable to this research: all we can truly assert, is the improbability, or rather the impossibility, of its having been derived from America, because that is contradicted by dates and historical facts, which cannot be perverted at pleasure; and the probability of its having been first noticed among the Spaniards, and carried by them into the more southern parts of Europe. I shall not pursue this fruitless search any farther, but proceed to acquaint you with some of the theories of the first writers, as to the causes of its invasion, and then describe the symptoms from the writings of eye-witnesses, and more especially from the accounts of those who experienced its attacks in their own persons: but here again we shall find ourselves involved in a labyrinth of contradictions and absurdities, especially when I come to relate the different modes in which the disease was communicable, according to the testimonies of former ages. In laying before you this evidence, the great difficulty I have to encounter is the selecting from among such a crowd of authors; but, of the earliest writers, (those who published between the years 1498 and 1508) I have quoted chiefly such as either from situation or acquirements are most particularly deserving of attention, or those

who detail only what they themselves have seen, or experienced in their own persons. The first remarkable circumstance that strikes us in this research is, that the most ancient writers, such as Peter Maynardus, Marcellus Cumanus, Grunpeck, and others, with one consent ascribe the disease to the malignant influence of the planets; and they go so far as to assert that its approach was predicted at least twenty years previous to the date of their labours; and here, if my object were only to amuse, I might quote such a mass of absurd jargon, such deep and unintelligible astrological learning, that one is at a loss which to admire most, the folly of those who wrote it, or the infatuation of those who read, and believed in its truth. The first author who sought for a rational explanation of the phenomena was Nicolas Leonicens, who published his treatise in June 1497: in this work he ascribes the invasion of the morbus gallicus to the inundations which had deluged Italy about that time, and though after him each new writer starts some fresh theory, we hear but little of the dreams of astrologers.

It has been asserted by many modern authors, who echo the stories told by their ancestors, that the mode in which the venereal disease was communicated then differed very materially from that which is now solely recognised: we are informed, that at first, it was so contagious as to be caught by means of the clothes, by touching an infected person, by a kiss, or by even breathing the same atmosphere; and yet if the original authorities are consulted, it will appear that this belief was not held in the first instance; for Marcellus Cumanus, who wrote in 1495, declares, that when he was encamped with the Venetian army at Novarro, (which John Howard has mistaken for Navarre) he saw many of the squires and foot soldiers of the lords of Milan, who suffered with pustules on the face and over the whole body, commonly beginning under the prepuce, or upon it, or on the penis itself, at first appearing like millet seeds, and with a trifling itching. This author mentions buboes also, which indeed, under these circumstances, could hardly fail of being sometimes present. A very few years later Alexander Benedictus of Verona, in speaking of the dreadful nature of this infection, observes, "*Muliebras partes pudendas infestare miserabiliter cœpit morbus gallicus, unde illud prostitutarum virus totum orbem infecit, tanta earum partium fœditate, ut quæcunque blandiori venere proci facili arcerentur videres faminas ore venerem pulchritudine superantes, quæ suo fœdissimo amplexu, infinitos libidine intemperantes sera penetentia afflixere.*" It will scarcely be necessary for me to suggest to you how many motives may have contributed to foster the belief which soon after this time began to be spread abroad, that impure commerce between the sexes was not the only mode of taking the disease; but even Gaspar Torella, who was physician to Caesar Borgia, and dedicated his first work on Pudentagra to that prince, although he that it was most commonly caught by con-

tact, yet, as if afraid that he had admitted too much, adds, that it may possibly be acquired in other ways, such as from bad diet; but it is curious enough to perceive, that in the relation of his cases we find the modern legitimate way of taking the disease always recorded; for example, speaking of one of his patients, he says, "*Rem habuerat cum muliere habente pudendagram;*" of another, "*per viam contagionis fuit infectus;*" whilst a third acquires the disease by sleeping in the same bed with his afflicted brother. Thus, also, Montagna the younger, writing to the Cardinal Viceroy of Hungary, who was then labouring under the complaint, very cunningly declares it to proceed from an epidemic disposition of the atmosphere; but he adds, with great truth, that it always begins in the parts of generation. Alexander Benedictus also, who was present at the action at Foro Novo, where the combined forces of the Venetians, and other Italian princes, were defeated by Charles the 8th, on his return to France from Italy, in the month of July 1495, distinctly says, this new disease arose in the parts of generation: his words are these: "*On this account (the nervous structure of the penis) from the venereal congress, a new, or at least a disorder hitherto unknown to the physicians, called the French disease, was brought to us from the west, by the malignant influence of the planets, and broke out at the time these affairs were going on, &c.*" I quote this passage because Swediaur has affirmed that none of the early writers had the least notion that the parts of generation were concerned in the first invasion of the disease; and he mentions this very writer, Benedictus, as an authority to that effect. In the first years of the 16th century, Cataneous gives us, among the proximate causes of the disease, either coition, or sleeping a long time with, or drinking after a diseased person: and later in the same century, we find, that among the articles of accusation against Cardinal Wolsey, it was urged that he, being infected with the venereal disease, had whispered in the king's ear. In the succeeding century, however, the conviction of its being communicable by the commerce of the sexes alone, becomes nearly as well established as in the present day; so that we may, without much hesitation, attribute the belief of its epidemic qualities to have arisen either from the credulity of the times, or more probably, as an easy method of avoiding the scandal and disgrace that would necessarily have attached itself to the numerous dignified sufferers, many of whom were ecclesiastics of the highest rank.

I know that the possibility of the venereal disease having been communicable in various other modes has been a favourite opinion of some recent authors; and that they have urged, in support of this doctrine, the parallel instances of the yaws, the sibbins, and of a new disease which has lately appeared in Canada, an account of which was published by Dr. Bowman: but it seems to me that none of these instances are at all in point; for in the first place the yaws has been described

from the first as a contagious disease, in the common acceptation of the word; and it never has been believed, nor is it now thought, that connexion between the sexes is necessary for its production; it is only possible to be communicated once during the person's life, in all which particulars the sibbins and the Canadian disease agree with it; so that, in order to render this article of any force, it should be shown, either that the yaws and sibbins differ now from the descriptions formerly given of them, or that the venereal disease is still to be caught by conversation, touching the person or clothes, or breathing the infected atmosphere, as was formerly said to have been the case. I should not have thought it necessary to have bestowed so many words on a point which appears to me to be perfectly untenable, if it were not that in reading some modern treatises, the above arguments are insisted upon; and I would not have it supposed that I have overlooked them from ignorance, or because I conceived them incapable of being answered.

I now proceed to detail some of the leading symptoms of syphilis, as recorded by the most distinguished and eminent writers within the first forty years after its invasion, and I shall select them from the writings of Marcellus Cumanus, of Gaspar Torella, and John de Vigo: these authors describe the appearance of small pustules on the genitals, attended with some degree of itching, followed in a few days by violent pains in the arms, legs, and feet, attended with large pustules, or ulcers, and which were cured with difficulty, sometimes lasting a twelvemonth or more: the bones became affected with swellings, the hair fell off, the eyes were sometimes destroyed, as well as the nose; the mouth and throat were ulcerated, the uvula corroded; and, finally, the disease killed, rather by inducing some other complaint than by the mere force of the symptoms themselves; and when it had once become confirmed, a palliative cure only could be obtained. Thus it will be perceived, that excepting in the rapidity of the march of the disease, the principal features were the same in the early part of the 16th century as at this time; they are mitigated in severity, but in kind they remain unchanged. It is to be observed, that buboes are mentioned in the early part of this history, although it has been said that they were first noticed by Francastorius, but that is scarcely worthy of a refutation; since as long as ulcers on the genitals have been known, so long must inflammation and suppuration of the inguinal glands have sometimes followed as a consequence. The same assertion respecting a gonorrhœa has been also made by Howard and others, but it is a mere inaccuracy, for this symptom is distinctly mentioned by Alexander Benedictus, who wrote about the year 1497, as well as by Jacob a Bethincourt, in 1597; and therefore there can be no pretence for saying that Fallopius was the first who included this among the number of venereal symptoms. I have already observed, in my former essay, that the use of

mercury in the cure of many cutaneous affections was known to the Arabians, and brought into notice in Europe by Theodoric, in the 12th century; and as applicable to the cure of syphilis, it is to be found among the modes of cure recommended by Grunpeck, in 1496, in conjunction with bleeding, purging, &c.: his receipt for mercurial ointment contains, indeed, a great many extraneous ingredients, with about one-sixteenth part of quicksilver; yet it is to be observed that the employment of such remedies was very generally condemned at that early period, and they were consequently almost exclusively employed by empirics or uneducated men; so that we find many medical writers warning the profession against their use, and trusting entirely to evacuations, baths, and various kinds of liniments: among these, Gaspar Torella is distinguished by the violence with which he opposes the mercurial inunction; however, he gives us several prescriptions of this kind, but adds, "*supradicta unguenta, tanquam a peste fugienda sunt.*"

There can be no doubt that the want of skill of the practitioners of those days, their ignorance of the effects of mercury, of its accumulative powers, and of its occasionally capricious action, must have occasioned many untoward events; indeed, the mode of employing the remedy then in vogue, together with the belief that the venereal poison was expelled by the mouth, will sufficiently account for much of the mischief that ensued. We are told, for instance, that the patients are to be anointed between two fires, twice every day, from the upper arms down to the hams, and from the hips to the feet, until the mouth was made sore; then they were to be kept warm, whilst the flux from the mouth was continued. Hence it arose, that finding all common methods of cure unsuccessful, and taught by experience the baneful consequences of a rash employment of mercury, any new remedy that presented itself was seized upon eagerly. Such a remedy was announced to have been discovered in the West Indies, where it was called guaicum or huaicum wood; it was brought to Europe first in the year 1508, by Gonzalvo Ferrand, and got into very general notice a few years later, in consequence of its curing a great number of persons, and especially Ulrich Von Hutton, who published an account of his own case, which very much tended to extend the reputation of this remedy: it will not, however, a little derogate from the presumed virtues of this wood, when we find, that even this case was only palliated by its use, and that so far from being a pure case of syphilis, it is evidently one in which mercury had been injudiciously administered, and where the patient was suffering from a mixed distemper. This will be readily conceived when we learn that Hutton had actually undergone the mercurial treatment eleven times with only partial relief, and that he had been a sufferer from a disease, supposed to be venereal, from the age of nine years. No great length of time elapsed before it was discovered that

many of those believed to have been cured by the decoction of guaicum, relapsed; this was of course attributed either to the wood itself being adulterated, or to some essential part of the process being neglected. Hence it became the fashion for those whose circumstances would admit of the expense of the voyage, to transport themselves to the West Indies, in order to undergo the Indian method of treatment. Mr. Pearson has given a very curious extract from M. Louis's work, in which the method of cure practised in America is detailed: from this narrative we learn that two young Frenchmen of rank, who had in vain endeavoured to obtain a cure in Europe, were recommended to embark for St. Domingo. Upon their arrival the Viceroy's physicians advised them to remove to Puerto Rico, where the cure of the disease was usually undertaken by females. They were treated in the hut of a native in the following manner. She bruised, and cut with her teeth the small branches of a young guaicum tree, and boiled them in an open vessel; they were made to drink a chopin of this decoction every morning, at two or three draughts; then they were ordered to walk out, to exercise themselves in fencing, or else they went to work in a gold mine, not far from the village, for the space of two hours; then, returning home covered with sweat, they changed their shirts, and dined; drinking only water. About three o'clock in the afternoon they drank the same quantity of guaicum decoction as in the morning, and performed the same exercise: thus, without any other remedy, they were perfectly cured in six weeks, without suffering any other inconvenience than a swelling and inflammation of the gums, of which they presently got well, after having been bled by pricking them in several places with a very sharp-pointed fish-bone. The nodes they had on their bones disappeared; all their nocturnal pains gave way in fifteen days; their appetites returned; and, in short, they went back to France quite well, and remained so ever after. Notwithstanding, however, these and many similar histories, there were not wanting men of great reputation, who contended that the guaicum could not be relied upon alone, and who still advocated the employment of mercury in all obstinate cases; and that this wood did not long maintain its pristine character, may be concluded from the introduction of other vegetable remedies, each of which was ushered into notice with the most unlimited and unqualified praises: the most extolled of these were the China root, and the sarsaparilla: the first of these roots, however, soon lost much of its reputation, for it was prescribed to the Emperor Charles 5th, but without effect; and in truth, as a single remedy, was soon superseded by the sarsaparilla, until at length it became the fashion to unite both these recently imported articles with the guaicum; thus giving origin to the decoction of the woods, so famous in latter times; and which, among the changes of fortune to which medicines are subject, as well as every thing else, has again obtained a

consideration nearly commensurate with that which it enjoyed even on its first introduction.

During the remaining portion of the 16th century, a great difference of opinion existed among medical men as to the respective merits of the mercurial and vegetable modes of treatment: names of the greatest reputation and authority are opposed to each other on this point: thus Fallopius condemns, in strong terms, the use of the mercurial ointments, and mentions, among the consequences, excessive salivation, mania, tophi, vertigo, &c.; observing, that many preferred perishing rather than undergoing the mercurial discipline, under which relapses were frequent, and caries of the bones, in particular, one of the most usual consequences. This is very strong language, and it is the more to be remarked, because Fallopius was a man of no common ability, attainments, and character; he was as remarkable for the estimable qualities of candour and disinterestedness, as for the splendour of his talents; and, without question, he spoke his genuine and unbiassed sentiments when he extolled the cure by sarsaparilla, as the *via regia*, and condemned the mercurial treatment, as "omnium curationum acerbissima;" and so it undoubtedly was, according to the mode of administering it practised in that day. Ambrose Paré, on the contrary, who wrote not much later in the same century, takes quite a different view of the matter; he mentions four methods of curing the great pox, as usually recognised among practitioners: the first, by the decoction of guaicum, being not severe; but he observes that it is not able to do more than palliate, it cannot extinguish the virus of the disease. Mercury, which was the next method, was employed in four different ways—by inunction, by fumigation, by plasters, and by pills: of these different modes, that by plaster was soon abandoned, whilst the internal exhibition of this mineral, but little employed until the close of the 16th century, then began to be pretty generally recommended, in conjunction with the external use of mercurial ointments, or liniments; and as emperors and kings will lead the fashion even in the introduction of new remedies, so it happened that the pills of Barbarossa obtained at this time great reputation, in consequence of their having been used by Francis the First, King of France.

The seventeenth century has to boast a list of writers on syphilis not much less numerous than the age which preceded it, but they will not detain us so long. The first thing that I shall observe in this portion of my history, is the decided improvement in the composition of the mercurial ointments, which, in the first instance, were composed of a farago of useless ingredients: we now find, that not only these were expelled, but the strength of the preparation was materially augmented. In this century, some authors began to appreciate the virtues of sarsaparilla, chiefly in removing the consequences of mercurial treatment—such as debility, pains in the joints, &c.;

but there is very little novelty in this, and I shall therefore content myself with giving you the general practice of that time, as recorded by two of our own countrymen—Sydenham and Wiseman. According to the former of these authors, the practice of inunction, as employed in his day, was, indeed, a most formidable process. The ointment he used was composed of hog's-lard and mercury, in the proportion of two ounces of the former to one of the latter; and of this, one-third part was directed to be rubbed by the patient into his arms, thighs, and legs, for three successive nights, avoiding both the axillæ and penis. After the third unction, the gums generally swell; but if not, eight grains of turpeth mineral are ordered to be given; the salivation is directed to be brought to a flow of about two quarts every twenty-four hours, and if it diminishes before the symptoms vanish, then a scruple of calomel is to be prescribed occasionally; and it appears that the patient during the whole of the time, was kept in the same sheets and clothes, unless the salivation proceeded to such a height as to endanger his life. Sydenham remarks, that mercury alone cannot succeed in curing an exostosis; and, from what he says respecting regimen, it is evident that it was the usual custom to keep the patient in bed during the whole process, and to enforce the most rigid abstinence. It seems to have frequently been the fashion, at this period, to go to France, for the purpose of undergoing a cure; and this the doctor explains as attributable to the belief in the superiority of the climate.

Wiseman gives himself no trouble to inquire into the origin of the disease, but his Treatise is, nevertheless, well deserving of attention, on several accounts. He remarks that the pox is caught either mediately or immediately: by the former he means, where an infected child sucks a sound nurse, or *vice versâ*. He ridicules the common tales, as to the propagation of the disease by sleeping in the same bed, wearing the clothes, or drinking out of the same vessel with one so affected. Wiseman was, in fact, a practical man, with strong common sense, and great knowledge of the world; and, therefore, paid little attention to theories that flattered the self-love, or tended to save the reputation, of his superiors. He is the first author who observes, from his own experience, that it often happens some men will be infected, whilst others shall escape with impunity, from the embraces of the same woman: of this he declares that he saw repeated instances whilst serving in the king of Spain's navy. "I have known," he says, "twenty men lie with one and the same woman, the same day, and only one of them affected, though the rest equally deserved it." He speaks of gonorrhœa as the first symptom, though not always so; and his enumeration of the symptoms does not differ from the generality of the writers of his age. Another peculiarity attending this disease is mentioned by Wiseman—the curious fact that many people are in the habit of fancying themselves infected, and the great difficul-

ty that is often found in persuading them to the contrary. Of the cure of the venereal disease Wiseman entertains but little doubt, unless the patient has previously undergone mercurial inunction ineffectually; and if he has been salivated, appeared well for some time, and then relapsed, he has still a more unfavourable opinion of the case. He is particular in directing venesection, before the commencement of the mercurial treatment; and seems to believe that, by this means, assisted by purging, the remedy is more efficacious and better borne by the patient. After enumerating several internal forms of exhibiting mercury, the following description of his mode of procuring a salivation, which I have considerably abridged, presents itself to our notice:—The patient is to have his bed near a fire; the windows, if the weather be cold, must be covered with blankets; or a more proper place is a stove, if the patient can bear it. The ointment is to be rubbed in either by the surgeon or the invalid, beginning from the feet, and then proceeding up the legs, and thighs, and hips, to the spine of the back, even as high as the neck, including the hands, arms, and shoulders; the belly is to be avoided. As the parts are rubbed, they are to be covered up; the head is to be wrapped up with a napkin, tacked to the cap round about the ears, and fastened before, to keep the chaps warm. Afterwards, the patient is to be put into a warm bed, and have a posset drink; and this ceremony may be repeated twice a-day, unless salivation is brought on too quickly. Many directions are given for cleaning the mouth, and a rolled clout is to be placed between the teeth, to prevent the chaps from closing. This precious process lasts from twenty to thirty days; after which, sweating is to be observed, of which three methods are detailed; and a whole chapter is devoted to the consideration of the specifics, together with formulæ for their preparation—of these, sarsaparilla, China root, guaicum, and saponaria, are the chief.

The above specimens will, I conceive, be sufficient to give a general idea of the mode of treatment employed towards the close of the 17th century, and, therefore, it is easy to imagine the number of victims such practice must have produced, and we may well comprehend the horror with which the pox was regarded in those days, and why it was made use of as one of the bitterest imprecations, since it would appear to be almost impossible to escape either mutilation or death from the disease or the remedy. One conclusion may however be drawn from this account; it is quite evident that neither the sarsaparilla nor the guaicum possessed the reputation formerly attached to them; that they had fallen to the rank of mere secondary agents, employed more for the purpose of palliating particular symptoms, or of restoring the tone and vigour of the constitution after the completion of the mercurial course, than as really endowed with any specific power over the disease itself; nevertheless, there were not wanting practitioners in those days, who entertained opi-

nions relative to syphilis more in conformity with the views which have lately caused so much discussion in this country. Of these, David Abercrombie is the most remarkable. he published a short dissertation on syphilis in 1684, in which he condemns mercury entirely, and declares that the vegetable remedies are alone sufficient to effect the cure of nearly every form of the disease, though he admitted the necessity of *occasionally* employing mercurial pills; but later in life he seems to have changed, or at least modified, his opinions very much, and contents himself with recommending the substitution of the *mercurius dulcis* for the mercurial inunction, and restricts his censures of the mineral remedy to the condemnation of salivation in patients of certain habits and constitutions.

This milder method of administering mercury began in the early part of the 18th century to obtain many advocates and followers; a warm discussion took place between these practitioners and the favourers of the older doctrines. In 1732, we find a very hot controversy carried on between Daniel Turner and Chicoyneau, of Montpelier, on this point of practice; and it must be confessed that if our countryman has not the best of the argument, he exceeds him by far in violence of invective. Among the eminent men who contributed to moderate the severity with which it had been customary to administer mercury, the name of Boerhaave must not be forgotten; he stood forth as a warm champion of the decoctions of sarsaparilla and guaiacum, and was greatly influential in bringing the profession to a more just and temperate appreciation of the powers of mercury. It is well known with what zeal this subject was taken up by his commentator, Van Swieten, who having the control of the medical department of the army, at Vienna, sent a certain number of soldiers to the hospital of St. Mark, in order to ascertain the merits of the milder plan of treatment by the corrosive sublimate, and all of them so sent, with the exception of six, who were affected with incurable caries of the bones prior to their admission into the hospital, were discharged cured. To this successful experiment must be ascribed the prevalence of the same practice in most parts of Germany to this day. But opposed to the employment of mercury, we must not forget to mention the names of De Blegny, and more especially of the great Morgagni: the first of these writers was decidedly adverse to the use of mercury, and the latter makes use of the following remarkable expressions: "When I went to Bologna, as a young man, both the external and internal use of mercury was nearly deserted, and I never heard of its being used during the eight years that I remained there, either one way or other, in the treatment of the venereal disease."

But notwithstanding these and other authorities, we have repeated proofs in the first half of this century that the state of practice in this disease was far from settled; that cases of the most severe suffering, rebellious to the usual

methods of cure, were then so common, that new remedies were eagerly sought for and brought into notice, enjoying an ephemeral reputation only to give place to what was already established: among these the volatile alkali was loudly extolled by M. Peyrehle, but it is not necessary for me to do more than mention the fact: this remedy soon sunk into oblivion: the same may be said of the meze-reon root, the powers of which were indeed supposed to be restricted to the cure of nodes and osteopic pains, and which still holds a place as an ingredient in the compound decoction of sarsaparilla. This medicine again became the object of investigation and inquiry by Sir William Fordyce, who has given an account of his experiments in the Medical Observations and Inquiries; and the conclusions to which he arrives are so strong, and so much in unison with what we now hear, that I am tempted to quote them. He says, that this preparation of the sarsaparilla will commonly remove, in a very short space of time, venereal headaches and nocturnal pains, and, if persisted in, will always effect a cure. In emaciated or consumptive habits (according to the same respectable authority,) from a venereal cause, it is the greatest restorer of flesh, strength and colour: when the throat, nose, palate, or the spongy bones in general, are affected with a slough or caries, it will commonly complete the cure, if persevered in long enough, provided a mercurial course (he means by inunction) has preceded the use of the sarsaparilla; and farther, he adds, it will, perhaps, always cure whatever resists the power of mercury; and it is therefore probable that we may find, in mercury and sarsaparilla combined, a certain cure for every case that can be properly called venereal.

We see here how very closely Sir William Fordyce advances to the very line of practice advocated and employed by many surgeons of the present day; but yet at that period his experiments made but little impression upon medical men in general, for we are told by Mr. Bromfield, almost at this very time, that he never saw a single instance in which the sarsaparilla cured the venereal disease without the assistance of mercury, either given with it, or taken previously; and Mr. Pearson remarks, that his own observations coincide entirely with those of his predecessor. Still, however, so many obstinate and difficult cases from time to time occurred, even in the practice of those who employed mercury in the most approved manner, that professional men did not abandon the search after some remedy that might possess the same power over the disease, without bringing those evils in its train which mercury gave rise to. Among these, for they were very numerous, opium, cicuta, and the nitrous acid, may be especially named, since their pretensions were upheld by authors of great reputation, and extensive trials were made of their virtues, with at least partial, or temporary success. Thus, with regard to opium, it was tried very extensively in America, and had a warm advocate in Dr. Michaelis; but excepting

that it was occasionally found to overcome nocturnal pains, and still more frequently to allay the irritation caused by a previously profuse exhibition of mercury, it seems to have had no real power over the disease. The same remarks apply to the effects of cicuta; but the nitrous acid has a stronger claim upon our attention. Its employment was much more general; the number of cures performed by it, or at least during its use, were so great, and its admirers were so enthusiastic in its praise, that it continued for many years to make a great impression on the public mind, and bade fair to supersede entirely the mercurial treatment: that it did not do so, we now can well understand, because we know that primary symptoms will get well either with or without any specific mode of treatment; but as sore throats and eruptions were too apt to succeed to these local cures, and as it was not imagined that simple means would also very frequently overcome these, we need not be surprised that the nitrous acid followed the fate of so many other remedies, and was at last neglected as a cure for syphilis, though it still maintains its reputation as a therapeutical agent in other diseases. One of the reasons that contributed to support the reputation of this remedy was the obvious effect it had in producing inflammation and swelling of the gums, and as mercury possessed a similar power, many theorists imagined that the medicinal effects of both remedies were the same, and hence arose the hypothesis that mercury owed its curvative powers to the oxygen contained in the majority of its preparations.

It will be perceived from what has been said, that all the efforts made by surgeons at various periods to supersede the employment of mercury, were so far from succeeding, that at the close of the 18th century, almost in our own days, its supremacy was thoroughly established, and in the most triumphant manner: it was generally believed that those unfortunate persons who failed to obtain a cure, or who had suffered the loss of the spongy bones of the palate and nose, or became affected with exostosis or caries of the larger bones, might ascribe their misfortunes to the use of too little, rather than to a superabundance of the remedy; and although other medicines were occasionally combined with the mercury, and sarsaparilla was frequently prescribed as a restorative to the constitution towards the termination of the cure, yet mercury was the *sine qua non*—it was given indiscriminately for every breach of surface on the genitals—scarcely could any cutaneous affection escape the suspicion of a syphilitic origin—nocturnal pains were generally condemned to inunction without mercy or discrimination—and the state of the venereal wards of our public hospitals will not easily be forgotten by those who are old enough to have witnessed the disgusting details they afforded—nay, I am sorry to observe, that this evil has scarcely been abolished entirely in our own days.

I have now brought down the history of syphilis to within thirty or forty years of the

present time, and have omitted, I trust, no material facts connected with it: I might have added an account of the various forms of mercurial medicines invented and lauded by different practitioners, but the properties, and relative merits of these different preparations, will more properly belong to that portion of my work devoted to the treatment of the symptoms, and I shall therefore now beg leave to offer to your consideration a remark or two which appear to arise out of the statements I have made, since history would be little better than a mere record of dates, unless we endeavoured to draw from it some useful inferences. In the first place, then, we have seen that at a certain period of the 15th century, a new and terrible disease is announced, rebellious to all the therapeutical means used in those days, attended by a train of symptoms loathsome in the highest degree, and spreading so universal an alarm, that the governments of several countries thought it necessary to provide an asylum for those affected with it, and to separate them from the rest of the population: this has been offered as a proof of the superior malignancy of the disease when it first made its appearance, as well as of its possessing a contagious property, independent of the common means of communicating it by the commerce of the sexes: but surely this inference is drawn rather too hastily: that in the course of time the disease has become milder, there can be but little doubt; but the absurd regulations of a barbarous age, when the nature of the disease was so totally misunderstood, and the laws of epidemics were no less so, certainly afford but little solid ground for believing that this was a contagious disease, in the usual acceptation of that term; and in confirmation of this opinion, I may remark, that the seclusion of the venereal patient was abandoned in so short a space of time, as to demonstrate pretty clearly, that the opinions of medical men had changed, not that the disease had thus suddenly altered its character.

2dly. We have seen that mercury was very soon discovered to possess a peculiar power in arresting the progress of the disease, but, as might be expected, this novel remedy was employed without measure or moderation; and most probably, in many cases of an ambiguous nature, not really syphilitic; so that the fatal results of the treatment on one side, and the disease on the other, led to the temporary, but almost total abandonment of mercury as a remedy: here we cannot but be impressed with the very strong evidence given us by men of the first character, as to the curative powers, not of one vegetable remedy only, but of several in succession, and which at length almost entirely superseded the mercurial treatment. We may indeed readily conceive, that both the guaicum and sarsaparilla derived much of their reputation from their employment in those cases where the constitution had been broken down by, or saturated with mercury; yet still we cannot doubt that the venereal disease must frequently have yielded to the use of those remedies, or how

can we account for such men as Fracastorius, Fallopius, Fernelius, Palmarius, and a host of other authorities, giving it the preference in their practice? Still, however, mercury, though lowered in fortune, was not entirely abandoned; and some years later we find it again enjoying its pristine reputation, until it received another rude shock from Boerhaave, after which it recovered its character, until it became at length thoroughly established in public opinion, and acknowledged by a consent, almost universal, to be the sole safe reliance of the practitioner in the cure of the disease. One thing, then, appears certain, that the natural history of syphilis was still utterly unknown, or rather, that it had never been inquired into at all. Numerous and learned indeed had been the disquisitions into the nature of the poison, and the seat of the infection; all the sects of medicine had in their turn applied the philosophical theories of the day to the explanation of the phenomena; but the safe, the only rational plan of inquiry, that by experiment and induction, had never been resorted to at all; it was reserved to a later period, and originated in our own country, the birth-place of that sound philosophy to which the present advanced state of all the arts and sciences is chiefly attributable. But before I enter upon this branch of my subject, there is one writer who more especially demands some notice; I mean Mr. John Hunter, who published a treatise on the venereal disease in the year 1786: this is a work on many accounts highly deserving of notice, and will, in its proper place, receive a due portion of our attention. At present I have to remark, that the labours of Mr. Hunter obviously led the way to much that has been more fully developed by others; his researches into the nature of the venereal poison, his original notice of certain affections, resembling syphilis, as well as numerous other novel and ingenious ideas scattered throughout his work, evince the original and comprehensive mind of that great man. It has often been lamented that Mr. Hunter undertook this inquiry without much previous knowledge of what had been written by his predecessors; but whilst I admit the fact, I beg leave to deny the conclusion drawn from it: I conceive, on the contrary, that by entering on his task totally unprejudiced, and drawing solely from the resources of his own mind, he has dispelled more errors, and did more towards elucidating this curious and long contested subject, than any man who went before him. If he did not pursue the inquiry to its fullest extent, he at least opened the path for future research; and the same stamp of originality is to be found in this work as distinguish the rest of his labours: that it has many faults, some of them of a serious nature, I certainly must admit: want of perspicuity has been ascribed to it by Mr. Hunter's most enthusiastic admirers, and it will be my duty to point out to you, in the proper place, many contradictions, and even some practical directions, which are now justly exploded. There is, however, yet

another writer whose labours demand a little of our notice, though, by a fatality which is often observed, and not to be accounted for, his work made but little impression on the public mind, and seems now to be almost forgotten: I allude to Dr. Clutterbuck's pamphlet, published in 1799, and entitled, *Remarks upon some of Mr. J. Hunter's opinions on the Venereal Disease.* The most remarkable passages of this work relate to the belief of the possibility of curing many forms of the venereal disease, not only without mercury, but without medicine of any kind; or in plain language, admitting that they might undergo a spontaneous cure. Thus you perceive how very nearly this gentleman advanced to the very conclusions which have since been the result of direct experiment; and that, in fact, as a late excellent writer has remarked, he may justly claim the merit of having distinctly pointed out to us that the mere circumstance of a disease giving way, and being cured without mercury, is no proof that the case is not venereal.

[*To be continued.*]

From the London Medical Gazette.

**SOME ACCOUNT OF A CASE IN WHICH THE UTERUS, IN A STATE OF MALIGNANT ULCERATION, WAS SUCCESSFULLY REMOVED.** By JAMES BLUNDELL, M.D., Lecturer on Physiology and Midwifery in the School of Guy's Hospital.

Mrs. A. B. æt. 50, of gray eyes, tranquil disposition, broad in her make, and disposed to obesity, was seized with offensive discharge from the vagina, soon followed by eruptions of blood in large quantity, so that, according to her own report, frequent faintings were produced, and the blood occasionally sank through a bed about twice as thick as a sofa-cushion, collecting on the floor; and day after day, for months together, with little intermission, one or two pints of blood were discharged.

Although Mrs. A. B. in her general conversation, is by no means prone to hyperbole, it seems evident that she must have greatly overrated the quantity of these daily floodings. Certain, however, it is, from her repeated and considerate declarations, that very large quantities of blood were lost during a period of many months; and though, with the exception of some small œdema of the legs, there were no signs of general dropsy, the paleness, coldness, and weakness, and the frequent attacks of faintness, of complete delirium, showed pretty clearly that much vascular inanition had been produced. In other particulars, the patient's condition was not altogether discouraging; for the bowels were regular, and the appetite was occasionally good; and the appearance, though cachectic, and perfectly similar to that of other women perishing under malignant ulceration of the uterus, was not such

as to indicate a constitution wholly unfit for surgical operation.

The woman having been under the care of three or four different practitioners before I saw her, I deemed it proper to examine immediately with great attention; when I found that the womb was moveable, and about as large as a goose's egg—that its mouth was broad, open, and of cartilaginous hardness—that it manifested the usual marks of malignant disorganization, in which also about one-fourth of the contiguous vagina was involved; and, further, that on the surface of the diseased mass was formed an ulcer, about as broad as a shilling. The adjacent structures appeared to be healthy enough—the bladder and rectum were sound, the inguinal glands were not enlarged, whence it was presumed that the lumbrics were perhaps healthy; the ovaries could not be felt to exceed their ordinary bulk, and there evidently was no tangible enlargement of the liver, spleen, kidneys, or omentum, all of which were examined with the nicest care. The breathing was easy; the pulse, various in its frequency, ranged between 115 and 120 in the minute; and the patient, though certainly very much debilitated, had sufficient remains of strength to walk to my house (the distance of a furlong,) though not without considerable difficulty. To be short—it seemed clear at this time, that the case was ulcerated carcinoma of the uterus, as it is called, and that extirpation was the only remaining remedy.

The bowels having been cleared, and the patient being resolved to submit to the operation, on the 19th of February, 1828, I determined to remove the diseased parts without further delay. For this purpose, having placed the woman in the obstetric position usual in this country (on the left side I mean,) close upon the edge of the bed, with the loins posteriorly, the shoulders advanced, the knees and bosom mutually approximated, and the abdomen directed a little downwards towards the bed, I began the operation.

*First Stage of the Operation.*—I commenced by passing the index and second finger of the left hand to the line of union between the indurated and healthy portions of the vagina; the finger being converted into a cutting instrument (varying with the exigencies of the operation,) by means of a moveable knife, which requires a word or two of description. The blade of this knife, not unlike that of a dissecting scalpel, was mounted upon a long slender shank, which, including its large handle, was about eleven inches in length; and with this stem the blade was united, so that its flat, or plane, formed with the stem an angle of 15 or 20 degrees. The first and second fingers of the left hand then being in the back of the vagina, contiguous to the diseased mass (as before observed,) by taking the stem-knife in my right hand, I could at pleasure lay the flat of the blade upon the front of these fingers, and urge the point of the instrument a little beyond the tip. The apex of the fore-finger being in this manner converted into a cutting point, by little

and little I gradually worked my way through the back of the vagina, toward the front of the rectum, so as to enter the recto-vaginal portion of the peritoneal cavity, frequently withdrawing the stem-scalpel, so as to place the point within the tip of the finger, and then making examination with great nicety, in order to ascertain whether the vagina was completely perforated, minute care being necessary in this part of the operation to avoid wounding the front of the intestine.

*Second Stage of the Operation.*—A small aperture having been formed in this manner, in the back of the vagina, through this opening the first joint of the fore-finger was passed, so as to enlarge it a little by dilatation and slight laceration (safer than incision.) This done, and a cutting edge being communicated to the finger, by placing the plane of the blade in such a manner that its incisory edge lay slightly advanced beyond the side of the finger now lying in the aperture, after drawing the point of the instrument within the tip of the finger, which operated as a guard, I proceeded to make an incision through the vagina transversely, that is, in a direction from hip to hip; for this purpose carrying the finger with its cutting edge, from the opening in the vagina already made, to the root of the broad ligament on the left side, so as to make one large aperture. I then took a second stem-scalpel, formed on the same model as the preceding, with this difference, that the incisory edge lay on the other side of the blade; and laying this instrument on the fore-finger as before—in such a manner, however, that the cutting edge lay forth on the other side of the finger (to the right of the pelvis, I mean.)—I carried the finger thus armed from the middle of the vagina, where the former incision commenced, to the root of the broad ligament on the right side; so that, at the end of this, which was the second step of the operation, the diseased and healthy portions of the vagina behind became completely detached from each other, by a transverse incision, which stretched across the vagina, between the roots of the broad ligaments immediately below the diseased parts. At this time the intestines could be felt hanging about the tips of the fingers; but the blade of the scalpel lying on the finger, in which it was as it were imbedded, the risk of a wound, whether by point or edge, was completely prevented.

*Third Stage of the Operation.*—The back of the vagina, then, having been divided in this manner, I urged the whole of the left hand, not of large size, into the vaginal cavity—and the more easily because the woman had borne children; afterwards passing the first and second fingers through the transverse opening along the back of the uterus—this viscus lying, as usual, near the brim of the pelvis, with its mouth backward, its fundus forward, and a little elevated just above the symphysis pubis. This manœuvre premised, under full protection of these fingers, now lying between the womb and the intestine, taking a double hook, mounted on a stem eleven inches long, I pass-

ed it into the abdominal cavity, through the transverse aperture, along the surface of the fingers already mentioned; and laying it in front of them, near their tips, I converted these fingers into a sort of sentient tenaculum, which, with little pain to the patient, I pushed into the back of the womb, near the fundus, and then drawing the womb downward and backward, towards the point of the os coccygis, as I carried the fingers upward and forward, I succeeded ultimately in placing the tips over the fundus in the manner of a blunt hook; after which, by a movement of retroversion, the womb was very speedily brought downwards and backwards, into the palm of the left hand, then lodging in the vagina, where, at this part of the operation, the diseased mass might be seen distinctly enough, lying just within the genital fissure.

*Fourth Stage of the Operation.*—The process of removal being brought to this point, the diseased structure still in the palm of my hand, remained in connexion with the sides of the pelvis, by means of the fallopian tubes and broad ligaments, and with the bladder by means of the peritoneum, the front of the vagina, and interposed cellular web,—parts which were easily divided, so as to liberate the mass to be removed. The broad ligaments were cut through, close upon the sides of the uterus, and in dividing the vagina great care was taken to keep clear of the neck of the bladder and ureters. This division of these attachments, and the removal of the diseased mass, constituted the fourth step of the operation. Some bits of indurated vagina, altogether not larger than the common bean, were left in the pelvis, to be removed at some future period, should symptoms require. This fact is worth recording.

To this circumstantial account of the operation may be added a few remarks. The intestines did not protrude. About an ounce of blood was lost when the back of the vagina was divided, three or four more ounces following when the vagina was cut in front. Ligatures, tenacula, and forceps, were in readiness to secure the vessels, but these were not required.

The intestines were felt at one time only, namely, when two fingers were lying out through the opening in the vagina behind. Of course some pain was felt when the first incisions were making, and when, as in ordinary obstetric operations, the hand was urged into the vagina; but the principal distress was occasioned by drawing down the uterus, when the retroversion was accomplished, and the ligaments were put upon the stretch.

The pains and complaints scarcely exceeded those observed in instrumental deliveries. The patient lay in the ordinary obstetric position, and required no restraint. The insertion of the hook into the back of the uterus did not occasion much suffering. The operation, from first to last, occupied about an hour, but much of this time was spent in reposing and considering what might best be done. With better instruments, and greater activity,

the whole operation might most probably be completed in five minutes. In obstetrics, however, celerity is considered to be in itself a secondary merit, and the operation was conducted on obstetric principles. The general range of the pulse was between 120 and 130, a frequency common in delivery by instruments.

When the last gush of blood was observed, the pulse became imperceptible in the wrist, returning however in the course of ten or fifteen minutes. A few ounces of spirits were administered to the patient as the operation proceeded. Throughout the process the forefinger of the left hand was the principal instrument, and the scalpels and hooks were employed merely as the means of arming the finger for its various operations. The professional friends who favoured me with their presence were, Dr. Elliotson, Mr. Callaway, Mr. B. Cooper, Mr. Key, and Mr. Morgan. An accident deprived me of the presence and assistance of my friend Dr. Roots. The operation was not undertaken at a venture, but in conformity with certain principles laid down in two papers read before the Medico-Chirurgical Society; the first of them in the year 1819, and the last in the year 1823. The latter, which was not published, contains the proposals for other abdominal operations. The fundamental principles of these operations, as there stated, are rested upon numerous observations made upon the human body, and a sufficient number of experiments upon brutes. Should the case here narrated come before the eyes of the public, I hope it may tend to diminish any unreasonable prejudices against experiments and experimenters. The feeling is respectable, but by the designing it may be misdirected. In Lisfranc's operation I conceive there must be some misapprehension. I think I run no risk in saying, that by his method of procedure, as understood here, what the English accoucheur means by cancer of the uterus, must frequently be irremovable.

It is now five months since the parts were extirpated, and the patient is fat and well, and designs to return to her husband. The interception of the access to the ovaries is a complete security against extra-uterine impregnation. The head of the vagina is closed by the bladder, which lies upon it. The recovery was easy enough, but as the details may, perhaps, be deemed desirable, they shall be communicated at an early opportunity. The patient had been ill for eight or nine months before the operation was performed.

From the Quarterly Journal of Science, Literature, and Art.

ON THE MECHANISM OF THE ACT OF VOMITING. By MARSHALL HALL, M. D., F. R. S. E., &c. &c.

Two opinions have divided physiologists respecting the nature of the act of vomiting. It was originally and long thought that this act consisted simply in a sudden and forcible

contraction of the stomach itself. Afterwards Bayle, and Chirac, and more recently M. Magendie, considered that the stomach is inactive, and evacuated by being subjected to pressure by the simultaneous contraction of the diaphragm and abdominal muscles.

It appears to me that neither of these opinions is correct. M. Magendie distinctly proves by actual observation, and by the substitution of a bladder in the place of the stomach, that the contraction of this organ is not usually subservient or necessary to the act of vomiting. I refer to the interesting paper\* of that eminent physiologist for the more full elucidation of this first question. I proceed to state such observations as appear to me to controvert the second, and to establish that view of this subject which I have myself been led to adopt.

It is obvious that, if vomiting were effected by a contraction of the diaphragm, it must be attended by inspiration. If this were the case, the fluids ejected from the stomach would be drawn into the larynx, and induce great irritation, events which are not observed. These events are, indeed, effectually prevented by an accurate closure of the larynx, a fact observed in an actual experiment by M. Magendie, who makes the following observation:—"Dans le vomissement, au moment où les matières vomies traversent la pharynx, la glotte se ferme très-exactement."† It is astonishing that this observation did not lead its acute author to see that, under such circumstances, a contraction of the diaphragm, unless the thorax followed precisely *pari passu*, was impossible.

Complete vomiting has been observed, too, in cases in which the stomach had entirely passed through a wound of the diaphragm into the thorax, and in which it could not, consequently, be subjected to the action of that muscle.‡ In some experiments, vomiting was observed also to take place, although the diaphragm had been paralyzed by a division of the phrenic nerves, or its influence subtracted by a division of its anterior attachments.§

This view of the subject is still further confirmed by facts, which I now proceed to state, which prove that the act of vomiting is an effort, not of inspiration, but of expiration. This is obvious enough, indeed, on a mere observation of the state of the thorax and abdomen during vomiting. The larynx is evidently abruptly and forcibly closed, the tho-

rax drawn downwards, and the abdomen inwards.

Such, indeed, appears to me to be the precise nature of the act of vomiting, in ordinary circumstances. The contents of the thorax and abdomen are subjected to the sudden and almost spasmodic contraction of all the muscles of expiration, the larynx being closed so that no air can escape from the chest, and the two cavities being made one by the floating or inert condition of the diaphragm. The mere mechanism of the act of vomiting differs little, therefore, from that of coughing, by which, indeed, the contents of the stomach are frequently expelled: the larynx, in the former, is, however, permanently,—in the latter, only momentarily closed; and there is, doubtless, a different condition of the cardiac orifice and of the œsophagus.

It appeared to me, from these views of this subject, that, if an opening were made into the trachea, or through the parietes of the thorax, the effort of expiration constituting the act of vomiting, would issue in expelling the air through these orifices respectively, and the evacuation of the stomach would be prevented; and I determined to submit the fact to the test of experiment. I took a little dog, made an ample opening into the windpipe, and gave a few grains of the sub-sulphate of mercury. The animal soon became sick. The first efforts to vomit induced a forcible expulsion of air through the orifice in the trachea. These efforts soon became very violent, however, and the stomach at length yielded a part of its contents. It was perfectly evident that the violent contractions of the abdominal muscles pressed upon the viscera of the abdomen so as to carry the diaphragm upwards to its fullest extent, and that at this moment vomiting was effected. The act of expiration was so forcible, that a lighted candle placed near the tracheal orifice was several times extinguished. In a second experiment, a free opening was made into the thorax between the sixth and seventh ribs of the right side. The lung collapsed partially only. During the first efforts to vomit, air was forcibly expelled through this orifice, the lung was brought almost into contact with it, the stomach was not evacuated. But as the efforts to vomit became extreme, a portion of lung was driven through the thoracic opening with violence and a sort of explosion, and at the same instant the stomach yielded its contents. These experiments appear to admit only of one explanation, of one conclusion,—that the act of vomiting is a forcible expiratory effort, the larynx being firmly closed, and the diaphragm perfectly inert.

It must be regarded as singular that M. Bourdon, by whom the action of the expiratory muscles, in their various "efforts," has been so well investigated,\* should have adopted other views of the act of vomiting.

\* Mémoire sur le Vomissement, par M. Magendie. A Paris, 1813.

† Mémoire sur l'Usage de l'Épiglotte dans la Déglutition, p. 3, note.

‡ Such a case is mentioned by Wepfer. A similar one was also recently witnessed by Dr. Webster and Mr. Hunt. The whole of the stomach was found in the thorax, having passed through a wound of the diaphragm. There was repeated vomiting of a substance resembling coffee-grounds.

§ Œuvres de Car. Legallois. A Paris, 1824, tom. ii. p. 104.

\* Recherches sur le Mécanisme de la Respiration, &c. Par Isid. Bourdon. A Paris, 1820.

It is not intended to state that the act of vomiting is simply such as I have described. There are many facts which appear to show that the œsophagus is not without its share of influence in this act, and it is plain that the cardiac orifice must be freely opened; for mere pressure upon the viscera of the abdomen will not, in ordinary circumstances, evacuate the contents of the stomach. To effect this open state of the cardiac orifice, it is probably necessary that the diaphragm should, indeed, be in a relaxed rather than in a contracted state.

A singular and interesting fact was noticed by M. Magendie, of which he has not given any explanation. During the state of nausea which preceded the act of vomiting, in some of his experiments, air was drawn into the stomach. I am disposed to think that this effect was produced in the following manner: the larynx being closed preparatorily to the act of vomiting, an attempt at inspiration is made before the effort of expiration. In this attempt, air is drawn into the œsophagus, the larynx being impervious, and it is afterwards probably propelled along that canal into the stomach itself. It is not improbable, too, that, in some instances of vomiting, in which the action of the abdominal muscles was subtracted,\* a similar effort of inspiration has drawn substances from the stomach into the œsophagus, which has eventually expelled them by an inverted action. Neither of these phenomena could result from any action of the diaphragm, and much less from contraction of the abdominal muscles. But it is easy, by closing the larynx and attempting to inspire, to draw air into the œsophagus. A similar act, if very forcible, might draw a portion of the contents of the stomach through the cardiac orifice.

Such, then, appears to be the nature of the act of vomiting. How different is this act from one in which the diaphragm does, indeed, contract suddenly, under similar circumstances of closure of the larynx,—viz. singultus: the action of the diaphragm being an effort of inspiration, air is apt to be drawn into the œsophagus with considerable noise; and there is occasionally pain, not only about the insertions of the diaphragm, but about the closed larynx.

From the London Medical Repository and Review.

MEMOIRES DE L'ACADEMIE ROYALE DE MEDECINE.—*Memoirs of the Royal Academy of Medicine.* Vol. I. with Plates. Paris, 1828.

There is no society in the known world where more talent, learning, and industry are displayed, in pursuit of medical science, than in the French Royal Academy of Medicine; a work, therefore, coming from such a body of able men must greatly interest the profession from one end of the civilized globe to the other. This learned Institution was established in December, 1820, by an Ordinance of Louis

XVIII. It was instituted for the special purpose of answering the demands of government in every thing relating to the public health, principally to examine into the history, nature, and treatment of epidemics; of the particular diseases of different countries; of epizooties or the epidemic maladies of animals; to examine every thing relating to legal medicine; for the propagation of vaccination; for the examination of all new remedies, and of secret remedies, of mineral waters both natural and factitious, and of all other subjects connected directly or indirectly with the healing art.

The Academy is divided into three sections—one of medicine, one of surgery, and one of pharmacy—and each is composed of Honorary and of Titular Members, of Associates, and of Adjuncts. Of the titular members, a certain number is to consist of veterinary surgeons; foreigners of eminence are admitted as associates and adjuncts. This learned institution consists of from seven to eight hundred members in all, among whom are several foreigners of distinction. Each section elects its own members, honorary and titular, and its adjuncts. The associates are elected by the academy at large, but the honorary and titular members, and the associates, must be approved by the king before the election is definitive. The election of the adjuncts is confirmed by the academy itself.

The academy holds its meetings either in a body, or in sections. A general sitting is holden every three months, and a sitting of each section takes place twice a month. At the general sittings the affairs of the academy at large are discussed and settled; and all scientific subjects of great importance, requiring the aid of all the sections, are brought forward and discussed on these occasions. The sittings of the sections are devoted to objects of science connected with the special pursuit of each; and if the subject under discussion in one of the sections requires the aid and knowledge of one of the other sections to elucidate it, these two unite for the purpose of discussing it in common. The general Bureau of the academy is composed of a President of Honour, whose office is perpetual, of a temporary President, of a Secretary, and of a Treasurer; and the Bureau of each department consists of a President, a Vice-President, and of a Secretary.

These are a few of the ordinances under which the Royal Academy of Medicine is governed—an institution which reflects more lustre on the French nation than the conquests of Napoleon ever did, when at the highest pitch of his glory, and an institution which forms a good pattern of imitation for all other civilized nations.

A great part of this volume of memoirs, which consists of about 830 pages, quarto, is occupied by the Inaugural Discourse of the president, and by Elogies on the following eminent characters, delivered by the president and secretary; viz. Corvisart, Cadet de Gassicourt, Bertholet, Pinel, Beauchêne, and Bourrou. These, and some other Discourses,

\* Œuvres de Legallois, tom. ii. p. 105.

take up about 340 pages. The rest of the volume consists of six memoirs from the section of medicine, four from the section of surgery, and six from the section of pharmacy. For the present article we shall take one of the surgical essays as a text.

*a. On Penetrating Wounds of the Chest.* By  
M. le baron LARREY.

Whatever proceeds from the pen of this surgical veteran will be viewed with much interest. A long career of practical experience, and opportunities almost unequalled by any other modern practitioner, added to a mind expanded by general science, form the basis upon which the works of this celebrated individual are founded. The principal objects of the present memoir are, to show the changes which take place in the form of the thorax consequent on lesions penetrating that cavity; to prove the necessity of and to show the best mode of performing the operation, for empyema when there is reason to believe that much blood is extravasated into the bag of the pleura; and to prove the advantage of the new method of treatment in these lesions, namely, of simply letting out the extravasated blood and of keeping a tent in the depending wound made by the operation, over the old method, of injecting astringent lotions into the cavity of the chest.

"Penetrating wounds of the chest vary, 1st, according to their form and directions; 2d, according to their depth and the nature of the lesion of the organs; 3d, according to the effects of this lesion on the vital properties of these organs, &c." When a sword or any other clean cutting instrument, penetrates the sac of the pleura, without producing any lesion of the lungs, or of the nerves or vessels of the thoracic parietes, the wound is simple, and requires merely to have its edges brought into contact, for an union to take place. The patient must be kept quiet, he is to take cooling drinks, and, if necessary, to have blood locally abstracted from the neighbourhood of the wound. These are all the means that are requisite to be resorted to, generally, in wounds of this simple nature.

But when the injury is complicated with lesions of the thoracic organs, it assumes very different characters. When the lungs, for instance, are wounded, immediate effusion of florid, frothy blood takes place; oppression; emphysema sometimes round the wound; spitting of blood; severe local pain; respiration is short and laborious; the visage palid; the vital forces feeble; the pulse small, accelerated and tremulous; animal heat is diminished, and the feet become cold; the patient sinks into a state of great anxiety, and frequently sighs. If the wound be large and deep, and attended with the above described symptoms, and if the patient be left to himself, death generally follows very soon; for, first, there is no resistance opposed to the blood poured out of the divided vessels; second, the irritation produced in the injured vessels and in the lungs themselves, and the contact

of the external air, act as causes in keeping up hemorrhage, and in producing nervous spasm. Baron Larrey's views with respect to the influence of the external air in keeping up hemorrhage from divided vessels, are different from those of Mr. Abernethy, who, in his lectures, says that arteries will often bleed when the injured parts are imbedded in blood, but the bleeding ceases when the cut extremities of the vessels are exposed to the air. We certainly find this to be the case on the surfaces of stumps, where the parts are fairly exposed; but in penetrating wounds, which are generally filled with blood, so that the air is prevented from coming fairly in contact with the wounded vessels, the pressure or resistance which the effused blood exerts on these vessels, must act materially in preventing or in diminishing further effusion. This fact is exemplified in aneurisms arising from puncture of the arteries. The wounded artery pours out its blood into the cellular membrane, which forms a sac, the fluid contained in which prevents further effusion; whereas, if the wounded vessels, partially cut across, were fully exposed, and deprived of any pressure, fatal hemorrhage would take place in some instances, in which, from the barrier opposed by the effused fluid, only a few drachms of blood is thrown out. This is a fact which Baron Larrey insists on with respect to wounds penetrating the chest, complicated with lesion of the thoracic organs.

If we treat these wounds after the old method, still used by many practitioners, we augment the irritation of the injured parts; we provoke acute inflammation of all the organs contained in the cavity, and produce, incessantly, a renewal of the hemorrhage. Thus, introducing a canula into the chest, for favouring the evacuation of the fluid; the suction or the absorption of the fluid; the introduction of tents into the wound to keep the edges asunder—means which form the basis of the ancient method—can only tend to aggravate the mischief without suppressing the effusion, which is immediately reproduced with additional force, if previously stopped, when these applications are used.

It is seldom, except when the ribs are either cut across or fractured, that the intercostal arteries are injured by the instrument in its passage between these bones, for these vessels are protected behind by the edge of the groove in which they are imbedded, and their anterior extremities, where they are most exposed to the action of the instrument, are so small that little hemorrhage would take place, even if they were divided. Besides, these arteries, so free in their course, easily retract, and become obliterated by the slightest resistance. It is sufficient to close the wound to put an entire stop to the bleeding from these arteries. Experience has often verified this assertion to M. Larrey; and he has never found it necessary to apply a ligature to the intercostal vessels, nor has he used any other means than simple compression to prevent effusion from them.

A lesion of the pericardium alone is easily distinguished from that of the lungs. In wounds of the former, the blood which flows from the chest is not frothy; the oppression is less than when the lungs are injured; but the beatings of the heart are quicker; the local pain is very acute, and is accompanied with spasms, extending, more or less distant, along the course of the phrenic nerves; for it is seldom that the pericardium is wounded without some injury being done to the branches of these nerves.

Baron Larrey has never had an opportunity of verifying, by post mortem examination, whether certain wounds of the heart be susceptible of cure, but he is of opinion that they are.

The author remarks, that the evil arising from the presence of blood in the cavity of the pleura, is not to be compared to that produced by an attempt at sucking out the fluid through canulæ, or by syringes, as has been generally recommended; and that the effused blood, unless the quantity is very great, will be absorbed, "not by the lymphatic vessels, as has been believed until the present day, but by the capillary veins, which ramify on the internal surfaces of the serous membranes, or in the cellular tissue." As absorption goes on, the surrounding organs develop themselves, and expand gradually so as to fill up the vacant space. Mr. Larrey has witnessed a great number of facts of this kind, where remarkable spontaneous cures took place, and with promptitude in proportion to the youth of the subjects and to the efficacy of the artificial means employed to assist the powers of nature. These means consist in closing the wound so as to exclude the entrance of air; in general and local blood-letting during the inflammatory period; in the application of cupping-glasses, blisters, and moxa, to the affected side, &c.

But if the effusion is considerable, and if it has taken place suddenly from lesions of some of the large vessels, so as to fill entirely one of the thoracic cavities, the resources both of nature and of art are then insufficient to remove the extravasated fluid, and a counter-opening becomes indispensable, unless the wound, which gave rise to the extravasation, be in a position low enough to give vent to the effused blood. It is necessary to be acquainted with the symptoms indicative of the quantity of blood in the thoracic cavity, in order to enable us to judge of the probability or improbability of its being absorbed. The first symptoms by which we are here to be guided are, the force of the primitive hemorrhage, the depth of the wound, and the size of the wounded vessels; but these symptoms can only lead to an inference of the mischief done. The symptoms which point out great extravasation are, the extreme debility or collapse into which the patient falls, which is manifested by the smallness and feebleness of the pulse, pallor of the skin, coldness, beginning at the extremities, and extending from thence over all the body. Other more characteristic symptoms succeed these first phenomena. The side in

which the extravasation has taken place is more elevated than the other, the ribs are separated to a greater distance from each other, and no motion is carried on in them. *Percussion, instead of imparting a heavy and obscure sound to the ear, emits a sound clear and sonorous*, a phenomenon which M. Larrey has oftentimes verified, though it appears contrary to the laws of physics: the patient is always desirous of reclining to the side of the injury, and of retaining, as much as he can, a sitting posture; because then the fluid presses less on the mediastinum and the diaphragm, than if he lay on the opposite side, or flat on his back.

These are the leading symptoms which the author mentions as characteristic of much effusion in one of the thoracic cavities, and which point out the necessity of performing the operation for empyema. But this operation is not to be resorted to until there is a certainty that the internal hemorrhage has ceased. The cessation of the effusion is announced by the development of warmth over the system, by the elevation of the pulse, by a return of colour to the skin, and by an augmentation of all the vital forces; respiration is more free; the pulsation of the heart is stronger and more equal; in fine, the sanguineous fluid which escapes from the wound, if not united, is no longer of a florid, vermilion colour, but is, on the contrary, black and carbonized. This internal hemorrhage is arrested by the obliteration of the wounded vessels and the obstruction of their coats, owing to an adhesive inflammation taking place in the points of contact.

These symptoms of reaction or return of the vital powers, coupled with those indicative of non-absorption of the effused fluid, prognosticate the necessity for making a counter-opening for the discharge of the extravasated blood. Our learned author proposes the following questions;—In what quantity of fluid should we be justified in resorting to this evacuation? Is the fluid to be evacuated partially and gradually? or is the whole of it to be abstracted at once? In deciding the first question we must be guided by the severity of the symptoms; but with respect to the others, experience has proved to the author that it is better to accomplish a complete evacuation of the fluid at once and uninterruptedly; for if any be allowed to remain after the air has had access to the thoracic cavity, putrefaction takes place, followed by all the evil effects which usually occur under such circumstances.

M. Larrey recommends the incision to be made as far backward as possible; if on the right side, between the eighth and ninth ribs, counting from above; if on the left, between the ninth and tenth ribs. The air must be prevented as much as possible from entering the chest during the operation, as well as after it. In order to accomplish this, the skin is to be drawn up over the rib before the incision is made, so that, after the fluid has been evacuated, it may slide down again and cover

the opening leading into the thoracic cavity.

After the fluid has been totally evacuated, one end of a slender fillet of lint, covered with cerate, is to be introduced into the cavity of the chest, and the other end fixed to the dressings. This fillet will favour the discharge of the fluid, prevent the adhesion of the wound, and keep the air from entering the thoracic cavity. This is to be allowed to remain in for about eight days, till suppuration has taken place in the edges of the wound, when it then becomes fistulous, and gives ready exit to any discharge which may take place from the cavity.

If the wound of the chest has been produced by a ball, the effects are according to the parts injured and the course which the projectile followed in the thoracic cavity; whether it penetrated to any great depth, or whether it lodges in the cavity: It is seldom that these foreign bodies, unless very small, penetrate the chest without producing a fracture of some of the bones forming the parietes. In these cases it is necessary to enlarge the external wound, in order to be able to extract any splinters of bone which may be loose and divested of periosteum, if any exist. This must be done with great caution, for fear of injuring the blood vessels running along the lower margin to each rib. Any spiculæ of bone standing out from the fractured surface must be removed by a pair of nippers. This being done, if the ball have not made its exit, every point of the side opposite to its entrance is to be examined, and, if found, it is to be extracted when that can be accomplished. If, on the contrary, the ball lodges in the thoracic cavity, we sound the wound with great caution, and, if discovered, it is to be immediately extracted.

Baron Larrey has seen many subjects in the parietes of whose chests balls of different sizes had remained for years, without causing any remarkable inconvenience. In others they have caused great irritation by their presence in these cavities, and have produced serous or purulent empyema, so as to require a counter-opening to be made for their extraction; and this operation is often very difficult to accomplish.

The change which some of these wounds produce in the conformation of the chest is very remarkable, when the operation for empyema has been performed. Baron Larrey has given two drawings to represent this change. One is that of a soldier, who received two wounds, during a single combat, in his right side. The instrument in its course divided a great part of the lungs and the root of one of the intercostal arteries. Profuse internal hemorrhage immediately took place, attended by great depression of the vital functions. The patient, however, rallied beyond all expectation; but on the third day he committed an excess, in eating bread and meat, which produced a renewal of the hemorrhage. This had scarcely stopped when he committed another excess, which threaten-

ed his life. The hemorrhage, however, ceased the third time, and after having allowed the vessels some days to close their extremities, M. Larrey evacuated the effused blood. The patient ultimately recovered; but his physical characters underwent a total metamorphosis; the functions of the respiratory organs, as well as of the heart, received remarkable modifications, but such as that life and health resumed a new equilibrium, as firm as that which existed previous to the accident. The side of the chest on which the injury was inflicted became considerably reduced in its circumference; the ribs lost a great part of their curvature, and came into immediate contact with each other; the shoulder of the same side sank much lower than the other; the trunk inclined towards that side; the pulsation of the heart was no longer felt on the left side, for the organ receded towards the right, and could be felt beating on the right side of the sternum, against the cartilages of the eighth and ninth ribs. The diaphragm was considerably elevated on the right side, with all the viscera suspended to its vault; a part of the right lung became hepatized and increased materially in volume, in order to produce a complete obliteration of the enormous void which the evacuation of the fluid had caused. The axillary vessels and nerves became enveloped in this fleshy inorganic mass, and the arm and abdominal members were reduced to a state of atrophy; whilst the left cavity of the chest became dilated in the same proportions, and the parenchyma of the lungs of that side acquired double its dimensions; thus the patient respired by that lung alone.

These are the leading points touched upon by the author in his memoir on wounds penetrating the cavities of the chest.

From the *Lancet*.

#### ON THE BILATERAL OPERATION FOR STONE, as performed by Baron DUPUY-TREN.

The origin of the bilateral operation is involved in some degree of obscurity; Broomfield in England, and Chaussier and Beclard in France, were led by reflection on a passage in the twenty-sixth chapter of the seventh book of Celsus, to suggest this operation; and, from the words of Celsus, it is not improbable but that the bilateral operation as now performed, was the one which he described. Be this as it may, a point of no great importance, the merit of being the first to perform it on the living subject is now generally, and, we believe, correctly, ascribed to Baron Dupuytren. His mode of operating is as follows:—

The patient being placed as in the lateral operation, and the staff being introduced and given to an assistant, the operator, with the right knee resting on the ground, places the thumb of the left hand on the tuberosity of the right os ischii, and the fore-finger of the same hand at the base of the scrotum, and, in

this way, puts on the stretch the skin of the perineum. With the other hand, the operator takes a straight scalpel, and introduces it on the right side of the perineum, ten or eleven lines from the raphe, and directs it to the left side, describing a slight curve. The centre of the incision should be from six to seven lines in front of the anus in children, and nine to twelve in adults. The parts divided in the first steps of the operation, are thin subcutaneous cellular tissue, perineal fascia, ejaculatores urinæ, and a few fibres of the transverse muscles.

The first part of the operation being finished, the surgeon introduces into the centre of the wound the extremity of the left forefinger, for the purpose of feeling the membranous portion of the urethra and the staff. Then the nail introduced into the groove of the instrument, serves as a guide to the scalpel, the cutting edge of which directed at first backwards, divides this portion of the canal, and then returned, prolongs the incision towards the superior part. As in the motion thus communicated to the knife, its escape from the groove of the staff might take place, Baron Dupuytren advises a double-edged scalpel to be used, although he generally employs a common scalpel himself.

When the urethra has been divided to the extent of six or eight lines, the nail being still kept in its situation, the lithotome is introduced. The operator then takes with the left hand the staff, and slightly depresses the handle, whilst the lithotome is pushed forwards, the concavity being turned upwards. When the lithotome reaches the extremity of the groove, it must then be disengaged from the groove, and the staff withdrawn. It is then turned, so that its concavity looks downwards. This turning of the instrument is necessary; for if it were opened whilst its concavity was turned upwards, the two blades would be perpendicular to the direction of the vessels, and these would then inevitably be wounded, an accident which will not occur if the concavity be downwards, as both blades will be then parallel to the rami of the ischia. The lithotome being in this situation is opened at No. 14 for children, and No. 15 for adults, and is then withdrawn with great precaution. When the operator finds, from the resistance being overcome, that the neck of the bladder and prostate are divided, he ceases to press on the part which communicates with the blades, by which they return into their sheath. It is the double incision which constitutes the great advantage of the bilateral over the internal operation, as performed in England.

The forefinger of the left hand is then introduced into the wound, and a blunt gorget, with the concavity upwards, is passed into the bladder when necessary, by means of which the forceps are introduced into the bladder, and the stone withdrawn.

Baron Dupuytren has performed this operation twenty-six times, with the loss of one case

From the Medico-Chirurgical Review.

#### SENSATION—MOTION—VOLITION.

From a little memoir lately published by Magendie, (and translated into English) on some recent discoveries relative to the functions of the nervous system, we have selected the following extract, as both curious and interesting.

“Undoubtedly, it would be of the utmost importance to ascertain how sensation and motion,—which, as it has just been stated, have their seat in the spinal marrow,—are propagated to the head, and extended to the brain and cerebellum; or, to be more explicit, how the impressions perceived by the senses and the determinations of the will are transmitted to the spinal marrow. Here experimental difficulties become almost insuperable, and I must confess that up to this day I have reaped nothing satisfactory on so delicate a question, and one which appears intimately connected with the most important secret of life.

“The innumerable experiments I have unsuccessfully tried hitherto, have, however, enabled me to ascertain one fact, which appears to me worthy of being noticed by physiologists, and respecting which, as far as my information goes, nothing yet has been advanced.

“If in a living animal you deprive the cerebral hemispheres of the power of acting, the animal will run straight forward with astonishing rapidity, as if propelled by some invisible and irresistible hand. If, on the contrary, the cerebellum be deprived of the the power of acting, locomotion assumes quite an opposite course—the animal recedes. It is a most remarkable phenomenon, for instance, to witness a bird, slightly wounded in the cerebellum, effect, for several successive days together, no other motions, either for walking, swimming, or flying, except in a retrograde direction.

“There should appear to result from these experiments, that any animal, otherwise enjoying its natural state of health, is placed, as it were, between two powers, which counteract and balance each other—the one impelling forward, the other backward; and that these two powers are completely under the influence of the will.

“A disease of the horse, and which is not generally known, seems perfectly calculated to elucidate these last results. Farriers term it *immobility*; and, in fact, if you attempt to drive the animal backward, let the means and strength you resort to be what they may, it remains rivetted to the spot,—the motions forward, on the contrary, are remarkably easy, and at times seem to be effected independent of the will.

“If the consequences I have just drawn be correct, the disease must consist of some physical alteration of the brain, or in some obstruction or other in the action of that organ.

“A few days ago I had two horses afflicted with immobility examined, and my conjecture was perfectly correct. In both, the brain was evidently diseased, and the cerebellum perfectly sound.

“It then appears demonstrated, that these

two opposite powers of the brain and of the cerebellum exist in animals; and that in some peculiar cases these powers may resist the influence of the will.

"Is this the case with man? Can our motions, which execute with so much precision the dictates of the will, cease to obey their commands, and be, as it were, in a state of rebellion? Finally, is the faculty of volition distinct from that by which our motions are regulated? Such are delicate questions we hardly dare to venture upon—they seem to lead to arduous abstractions, the insuperable limits of human understanding; but, however, I have witnessed myself, and I have had an opportunity of studying for several successive weeks, in a well-informed man, perfectly qualified for self-observation, a complete distinction between the will and that power by which our movements are regulated.

"Subsequent to violent grief, the person I allude to, to his utmost surprise, was suddenly deprived of the influence of his will over his motions; in spite of himself, he was compelled to assume the most ridiculous attitudes, and to make the most extravagant contortions. The eccentricity of his actions and postures baffle all description; in certain cases his motions would be natural: thus, without the slightest intervention of his will, he was seen to rise and walk precipitately forward, until he came in contact with some solid body that impeded his course;—at other times he would recede backward with equal promptitude, until he was checked by some similar cause. In other instances he was observed to recover the use of certain motions, and to remain incapable of directing others. It was thus that his hands and arms frequently obeyed the dictates of his will, and more frequently again could he regulate the muscles of his features, and those connected with the organs of speech. At times he was allowed to walk backward, whilst his progress forward became totally impossible; he then would resort to this mode of progression to reach the objects he had in view. This state lasted four calendar months, and terminated most successfully. A few grains of a substance which chemistry has lately discovered, (the sulphate of quinine,) sufficed to confine his motions to the immediate dictates of his will.

"It may then be correct to admit, that the faculty of volition is perfectly distinct from that faculty by which our motions are directed and classified into regular acts. Such is the only consequence I wished to infer from the fact I have just stated. Several others rush to the mind,—to follow them up would make a metaphysician of me, and I mean to remain a physiologist."

From the Medico-Chirurgical Review.

A PRACTICAL TREATISE ON THE TY-PHUS OR ADYNAMIC FEVER. By JOHN BURNES, M. D. &c. &c. Octavo, pp. 248.

Perhaps there is no class of diseases which has attracted more attention, or given rise to a greater variety of sentiment and discussion,

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than fevers. The frequency of their occurrence, the fatality of their character, and the obscurity in which they are involved, are inducements sufficient to elicit the most patient investigation. But, as in almost every other subject of difficulty, the progress made has not been in proportion to the amount of labour; and, whether we examine their etiology, pathology, or treatment, it is much to be feared that few of our modern writers are more orthodox in their views than the Sage of Cos. The constant bias that exists in our profession to study Nature through the medium of prejudice, to extract general inferences from premises that are particular, and to invest effects with the importance of causes, has cruelly retarded the march of its improvement. It is a science of itself to know how to distinguish facts from fancies, and to ascribe to phenomena, as they rise, the rank and value which Nature has assigned them. Because intestinal disease is a frequent result of fever, it has been inferred by many that fever is its consequence; and, because the brain and its appendages are often found vascular or inflamed, it has been hastily assumed, that the *sedes et causa morbi* are to be found in the brain, and in the brain only. Such exclusive doctrines are the product of partial and undigested views, and the practice they recommend is, consequently, confined to the relief of a few out of many symptoms.

We are, therefore, pleased to find that our present author has taken a more *systematic* view of this disease, and that, while Broussais and Clutterbuck insist upon its *organic* character, we have Burne to add to the number of those, who are not less successful in their practice, while they are more extended in their etiology.

But, while we hail Dr. Burne's escape from the schools of the localists, we fear that he has received a lesson out of that of Brown; for, although he refuses to confine fever to a certain seat, he hesitates not to confine it to a certain character. After dividing fevers into such as are inflammatory and adynamic; and subdividing these into such as are purely inflammatory and purely adynamic, and those accompanied by local inflammation, he rejects the generic term *typhus*, to which we have been hitherto accustomed, and adopts the word *adynamic* as a more eligible title, within which he includes "the putrid or malignant fever of Sydenham; the slow nervous fever of Huxham; the nervous fever of common language; the synochus, typhus mitior and gravior, of Cullen; the jail and hospital fever; the *fièvres essentielles* of the French; the epidemic of the Irish writers; the contagious of Bateman; the typhus of Dr. Armstrong; and the proper, idiopathic, or essential fever of Dr. Clutterbuck." P. 8.

Had Dr. Burne confined his epithet *adynamic* to the lowest and most atonic forms of fever, perhaps the change, which he wishes to introduce into our nomenclature, would have been useful, and might have been adopted; but we cannot silently subscribe to an

alteration, which is founded upon, and inculcates the old doctrine of debility; a doctrine, which, however ornamented by the ingenuity of Brown, and however countenanced by vulgar prejudices, has been productive of infinite mischief to men and to medicine. Holding, as we do, fever, in the abstract, without relation to peculiar epidemics, constitutions, and circumstances, to be more or less a tonic disease, where the whole system is under excitement, and where every texture and organ are stimulated to an inordinate degree, we cannot sanction an epithet which gives the reader, *in limine*, what we deem an erroneous view of its real nature. To designate by the same title the synochus and typhus gravior of Cullen, the putrid fever of Sydenham, and the brain fever of Clutterbuck, is, to say the least of it, indiscriminating and unsafe; not that we imagine they are different in essence, (with Dr. B. we believe them to be the same, "differing only in degree," as he rightly observes, "and modified by circumstances;") but, because the symptoms *are* different, and the treatment *ought* to be different. Our author admits that the debility, which exists in fever, is what Brown would have called *indirect*, and not "ordinary debility," p. 11; but the term *adynamic* (which, in truth, explains itself) signifies not *depression*, but *want* of power, or absolute debility; and, consequently, conveys a meaning very different from that which the writer's own admission required.

We have already stated that Dr. B. following the example of Pinel, divides all fevers into two great classes, viz. inflammatory and adynamic. These he subdivides into simple inflammatory and simple adynamic fever, and inflammatory and adynamic fever complicated with local inflammation: but, as his work is confined to the two varieties of adynamic fever, the observations which follow will, of course, be restricted to them.

"In order to the production (says the Dr.) of the adynamic fever, it is necessary that there be a certain state of system, which state depends, in most instances, on a continued exposure to a poisoned or contaminated atmosphere. This state then existing, it will be found that the adynamic fever attacks in two ways; and, it is of great importance, that these should be clearly understood, because they account for the presence or supervention of inflammation in some cases, and the absence of it in others, and thus afford evidence that certain modern theories are not founded on correct principles. In the one way, then, it attacks through the intervention of an accidental cause; in the other without such intervention. When the attack is without the intervention of an accidental cause, the condition of the body is, of itself, sufficient to stir up and give rise to all those actions and phenomena which constitute the adynamic fever. But, when the attack is with the intervention of an accidental cause, the condition of the body, though not sufficient of itself to produce the fever, is yet sufficient to give to the fever thus accidentally produced, the peculiar adynamic

type. In the one case, the development is slow and progressive, requiring many days or even weeks; in the other, it is fully formed in a few hours." P. 14.

He then details the symptoms of attack in both ways, merely differing from each other in the rapidity of their progress, and then remarks, "When it attacks without the intervention of an accidental cause, so far as I have seen, the attack is not accompanied with any organic inflammation;" it being only in such cases as occur through some accidental cause, as a severe cold, exposure to rain, wind, &c. "that organic inflammations are apt to accompany it from the beginning." 17, 18.

The accuracy of these observations we are somewhat disposed to question.\* That the febrile poison often lies latent in the constitution for a long time before its existence is announced by appropriate symptoms, is a fact as well established as it is important; and, that the development of these symptoms, in different cases, occupies very different periods of time, is not to be disputed; and, moreover, that there are instances, in which the pathognomonic signs of this disease suddenly and unexpectedly supervene to a perfect state of health, is certain; but, that fever is never accompanied by inflammation, when it attacks without the intervention of an accidental cause, is a position to which we cannot assent. Had Dr. B. required us to believe that inflammation occurred *less frequently* in such cases, his requisition would have been more reasonable; for, it is unquestionable, that the more sudden the excitement is, the less time and power have the weaker organs to resist, or adapt themselves to an increase of action, and the fainter is their chance of escape. We believe there are few examples of what our author calls *simple adynamic fever*, or fever unaccompanied by any organic inflammation. We have inspected many bodies after death, and seldom have we failed in detecting the effects of phlogistic action; and if symptoms during life be minutely watched, we hesitate not to aver, that the experienced observer will generally be able to point out the organ, on which the circulation preys with predominating force. Organic inflammations are not the essentials but accidents of fever, the frequency of whose occurrence is more in proportion to the susceptibility of individual texture, than to the peculiar nature of the exciting cause; and, whether the attacks arise from exposure to cold, or exposure to contagion, the number and nature of the organs affected will mainly

\* We imagine that every cause of fever is *accidental*, and that, although some may operate more actively and suddenly than others, fever cannot arise without some *accidental* cause. Respiring a contaminated atmosphere is as much an *accident* as respiring a cold atmosphere; and we are not aware, that any one is exposed, by a *fatal necessity*, to be assailed by this or any other disease. The above division is, therefore, built upon a distinction without a difference.

depend upon their state of health before the excitement has commenced. Very rarely, we fear, does it happen, that all the parts of our complicated system are equally healthy, and can equally resist the inroads of disease; and we are taught by the simplest law in mechanical philosophy, that the weakest portion of a structure, equally assailed, will be the first to strike to the assailing power.

"The adynamic fever, then, being produced in either of the two ways of attack just described, will be found to differ very much in severity, for which reason and for practical purposes it is expedient to divide it into degrees; and these may, with great propriety, be limited to four." 19. In the first occur slight headach, impaired appetite, tongue moist but white, except at the tip, which is red, flushed cheeks, suffused eyes, slight duskiness, and increased heat of skin, red and scanty urine, slow bowels, prostration of strength, and blunted senses. In the second degree the pulse is frequent, "*rather full and rather strong*," the headach increases, the tongue is more coated, the skin is hot, (shivering or sense of cold, although one of the most frequent and familiar febrile symptoms, is never mentioned,) the bowels are more inactive, and, in short, the symptoms of the first degree are much aggravated. In the third grade, the strength is much prostrated, the patient is averse to the slightest exertion, the respiration labours, the muscles twitch and tremble, the senses are more torpid, the headach becomes more dull, and is often accompanied by a sense of weight or sound, the thirst is excessive, (this is the first time that the symptom is noticed,) the countenance is inexpressive, the flush upon the cheek is now more purple than red, the eyes are suffused, glassy, and vacant, half closed by a relaxation of the upper eyelids, and besmeared with shreds of mucus, the lips are either blue, or with the teeth covered with black sordes, the breath is very offensive, (there is no notice taken of that peculiar odour which is exhaled from the surface of fever-patients, and than which nothing is more characteristic of this disease,) the tongue is thickly coated, brown and dry in the middle, red and dry at the tip, and whitish and moist at the edges, the "*pulse seldom exceeds 90, it is fullish, and sometimes rather firm*," the skin is dry and variable in temperature, the urine is turbid when cold, the abdomen is full and tender on heavy pressure, (this is the first notice of a symptom which, we imagine, will be much more frequently found in the two first than the last degrees of fever; for, it is a very important fact, that intestinal disease may go on to a destructive extent, when the sensibility is blunted by an oppressed cerebrum, without betraying even its existence under the very firmest pressure, and this is a circumstance which ought to be largely insisted on, and carefully remembered,) the bowels are generally slow, but sometimes lax, and the stools are very offensive; there is much restlessness and delirium, but especially during night. The pa-

tient does not, in general, remain long in this state. If he recover, his symptoms either gradually decline, or more rapidly disappear upon the supervention of some critical discharge, as epistaxis, diarrhœa, or a profuse sweat; but, if he sink, his delirium becomes constant, he talks incoherently, or screams, or mutters; his jacitation is incessant, or his prostration so great, that he lies prone and motionless, his lethargy increases, his skin becomes cold and purple, and, the vital function ceasing, the curtain drops. In the fourth and last degree, all the symptoms already specified, become very severe, immediately the fever is formed, and run hastily on to a fatal termination; but, as there is nothing peculiar in them, it is unnecessary to follow the author's delineations any further. In a malady so interesting as fever, our investigations cannot be too minute, so long as they are instructive; but, we had rather Dr. B. had traced its symptoms through four different stages than four different degrees, in as far as all these degrees frequently occur during one and the same attack, equally requiring the same variety of treatment.

The second chapter is devoted to some peculiarities from the ordinary combinations of symptoms, for which the author furnishes us with a rationale, by referring to the character of the exciting cause; how, in one case, in which head symptoms had been the most severe, the patient had replaced upon his head a cap that had fallen into the water; and, in another, how the abdomen was principally involved, by the patient having been previously subjected to hard labour. But, why hard labour should not injure the lungs as much as the abdominal viscera, the author leaves us to explain. The truth is, that he might have furnished many such instances of febrile *particularities*, which admit of no elucidation by a reference to the exciting causes, and are only to be accounted for on the principle already stated. The following remarks upon Dr. Armstrong's variety of congestive fever we leave to the consideration of our readers:—

"There is a congestive state also, which, now and then, happens at the beginning, or during the course of the adynamic fever. In this state, there is great and often dangerous depression of the vital powers; so that, instead of hot skin and accelerated pulse, the temperature of the surface is below the natural standard, and the pulse slow and feeble. The skin of the whole body is very dusky, and the hands, feet, and lips of a leaden hue; all, evidently, arising from a congestion of the venous system, the result of feeble and slow circulation; now to this state has been given the term congestive fever, but this is a nosological solecism, for that condition cannot be called fever, in which the signs of fever do not exist; and there is here neither heat of skin nor accelerated pulse, both which are necessary to constitute fever. The appellation is not consistent with the condition specified, and, therefore, is not correct." P. 37.

That the pulse is, in general, the safest in-

dex of the activity of the heart, and that the use of the lancet in disease ought generally to be indicated by the energy of this organ, are points in therapeutics which few can question; but, that there may be instances, even in adynamic fever, in which other symptoms than the pulse form more useful, because more practical guides, is an important fact, of which our author seems not to be sufficiently aware. After very considerable depletion, without relief, when the pulse had become *apparently* weak and incompressible, we have seen a few more ounces entirely remove the disagreeable symptoms, and the patient get rapidly well; and, in several instances, we have found, that taking away six or ten ounces, has changed the character of the pulse from that of *masked* debility to strength, requiring further depletion to control it. Our experience, therefore, in such cases, prevents us from adopting any universal rule; and, when we find our author asserting, that in fever "the pulse is never full and strong, and is not firm," (p. 54) we are still less inclined to receive, without some qualification, his remarks upon a point of so much importance. We are not advocates for bleeding largely in the last stages of fever, nor do we wish to advocate the practice of treating fever, in the first stage, as a *purely* inflammatory disease; but, we maintain that copious and repeated depletions are often safe and even necessary, and, that, while the pulse is sometimes restrained, and simulates weakness through want of bleeding, rising in force and frequency under the use of the lancet, it will frequently be found *full, strong, and firm, not retreating from the finger, nor leaving upon it a slight and transient impression*. Our author will, perhaps, permit us to observe, *en passant*, that his theory of the state of the circulation during fever is built upon several physiological points, which themselves require a foundation. The tonicity of arteries, their power of contracting upon their contents, the assistance given to the venous circulation by muscular contraction, and the necessity of a large column of blood in the arteries to preserve them from collapsing, are positions which have not been proved, and are no more than probable.

In fever, throughout all its degrees and stages, the tongue is a matter of much importance, and merits the most particular and constant attention. If carefully watched, it will often indicate, with accuracy, the mucous condition of the stomach and intestines. With this view, the following remarks are very valuable.

"These different states of the tongue correspond with the different states of the alimentary canal. When the tongue is moist, the coating of a light shade and not thick, and the redness of the edges and point not deep, the belly is in a natural state, and so it remains when this tongue becomes clean and of a natural appearance. Where the tongue is dry or parched, the coating dark or black, and the edges and point of a deep red, the belly is flatulent or tympanitic, and tender, in a cor-

responding degree: the bowels are relaxed, and the dejections dark or black, and highly offensive, constituting the 'dark or black offensive diarrhœa' to be spoken of hereafter. When the tongue has cleaned, and is left smooth, moist, of a raw red, and tender, sensible and sore, this state is accompanied with subsidence of the tympanitic belly and with relaxed bowels; but the dejections are now ochre-coloured, and much less offensive, constituting the 'ochre-coloured diarrhœa' to be spoken of hereafter. In those instances where, at the decline of the fever, the tongue is left preternaturally clean, but instead of being moist, is dry and shining in the middle and at the point, as if polished, the belly remains tympanitic, more or less; and although the diarrhœa is ochre-coloured, the dejections continue highly offensive, which will be explained when the 'ochre-coloured diarrhœa' is considered. When the adynamic fever is combined with rheumatism, the character of the tongue is modified, and the foul surface is made up of a mixture of the dirty coating, peculiar to the adynamic fever, and the white fur peculiar to rheumatism; and when it becomes clean, the surface will be whiter, and the substance paler, than when rheumatism has not existed." P. 61.

It frequently happens in those cases of fever, which have arisen from, or have been accompanied by neglected bowels, that the abdomen is more or less tympanitic, and betrays considerable tenderness under firm pressure. In such instances, there can be no doubt, that this accumulation of gas is often the product of retained fæces, and will generally disappear with their removal; but, that this tympanitic condition does not *always* arise from constipation is certain, since we daily see febrile patients, after a tardy course of illness, with distended bellies, yet, whose primæ viæ have been carefully cleared out at the commencement of the disease, and preserved regular throughout it by mild aperients. Indeed, the most marked and obstinate cases of tympanitis, which we have witnessed, occurred during convalescence, and, probably, depended upon the debilitated state of the alimentary canal, from a continued course of purging.

Dr. B. in this symptomatology enumerates diarrhœa, which he distinguishes into two kinds, "the black offensive diarrhœa," and "the ochre-coloured diarrhœa;" the former accompanying the most aggravated periods, and the latter the wane of the disease. But we much question whether the regularity of the phenomenon entitles it to rank among the common symptoms of fever, and also, whether, when it does appear, its character sanctions such a distinction. Except in the enteritic forms of typhus, where the mucous lining of the bowels is the principal domicile of diseased action, and when its irritability is easily excited by the gentlest stimuli, constipation, we apprehend, is a more regular attendant than diarrhœa. If the brain or lungs be chiefly affected, the bowels are generally confined and dull, and seldom, in such cases, do they act without the aid of

medicine, unless Nature has chosen them as the outlet, through which, by a critical discharge, she intends expelling the disease. Besides, the external character of the dejections vary daily with the symptoms and progress of the disease; sometimes assuming a more natural, at others a more diseased aspect. During the first stage, while we are unloading the intestines of their accumulated contents, the stools are, in general, both dark and offensive; but, so soon as the discharge has been removed, they put on many varieties of appearance, according to the medicines given, to the inveteracy or mildness of the existing symptoms. Dr. Bright believes that ochre-coloured dejections indicate, either the presence, or approach of ulceration of the bowels; and, although this remark may be disputed as a general observation, we believe experience will warrant the assertion, that, when the intestines are nearly empty, as they are throughout the greater part of fever, treated as it now is with so much opening medicine, and the mucous membrane is excited to an increased secretion, the dejections will be found either yellow, or of a light colour.

The following observations, upon the connexion between ventricular effusion and retention of urine, are, we fear, more ingenious than just.

"There would be little difficulty or attendant danger, were the patient able to direct the attention of the physician to his local distress; but this he is rendered incapable of, by his general insensibility and confused perceptions. The distention of the bladder, therefore, goes on, and with it, distention of the ureters, infundibula, and pelves of the kidneys. The great pressure of this distention *resists* the distillation of the urine from the mammary processes and uriniferous tubules: the secretion itself is also *thereby* diminished; and then *supervene* the phenomena and effects of suppression of urine; *namely, effusion into the ventricles with its concomitant signs*. The secretion of urine being only diminished, not altogether suppressed, the bladder would inevitably burst, was it not that the pressure eventually overcomes the resistance of the sphincter, and the urine drips away, forming also incontinence. Yet notwithstanding this overflowing, the bladder is only secured from bursting, for the great distention and pressure continue, and oppose the secretion of urine, and thereby encourage and augment the ventricular effusion." P. 72.

We believe we may safely assert that the prevailing opinion is the very opposite of that contained in this extract. That deficiency of secretion, or retention of urine may *augment* effusion within the head, it would be unphysiological to deny; but, that this effusion is the *sole result* of such non-secretion or retention, it is not easy to credit; and it would be more difficult to prove. In every instance, where the urinary apparatus fails to discharge its accustomed duties, the energy and functions of the brain will be found to have been *previously* affected. The mind is either muddled and delirious, or torpid and insensible;

the external senses are either blunted and dull, or cease entirely to acknowledge their appropriate stimuli; deglutition is impaired, or gone; the sphincter of the rectum has lost its retentive power; the patient lies powerless and prostrate, with his head down off the pillow, and his feet drawn up in the bed, and his entire system is under the paralyzing influence of an oppressed, or exhausted sensorium. We do not remember an instance of retention, suppression, or incontinence of urine during fever, in which the cerebral functions were undisturbed; and we are ignorant of any case, in which either a deficiency, retention, or incontinence of urine *preceded* their disturbance. Derangement of the urinary organ is, therefore, *posterior to, and the effect of* derangement of the brain; and this rationale of the symptom is confirmed by dissection, when it frequently occurs, that water is found *in large quantities* upon the brain and within the ventricles, while *none lies within the bladder*, and, yet, in such cases no accumulation of urine has been permitted during life, and no difficulty has been experienced in passing it. The reverse of this is likewise no unusual occurrence, the bladder being found *full of urine, and no effusion within the head*. Moreover, admitting the fact, we would ask our author why the cavities of the brain are those *always* chosen by Nature, into which she may pour the retained fluid? Why do we not have other forms of dropsy, as hydrothorax, ascites, or anasarca? In ordinary cases of retained urine, from stricture or diseased prostate, we find no such partiality; on the contrary, we believe that ventricular effusion is a comparatively rare occurrence.

We have no experience of the state portrayed by the following sentence.

"Sometimes, as the adynamic fever is on the decline, there takes place watching, with a peculiar, staring, rather brilliant, and observant eye, and frequent lifting up of the head, as it were to listen, and mild delirium. The pulse is frequent, generally 120 in the minute; its stroke is rather open and vibrating, but is short, and leaves no impression on the finger; and the artery is very compressible. These signs must not be mistaken for irritation, or sub-acute inflammation of the brain: they occur in patients who have lost much blood; they are the result of that loss, and depend immediately on a defective arterial impulse, and defective supply of blood to the brain." P. 84.

Speaking of the causes which retard convalescence from fever or occasion a relapse, he furnishes us with these important observations:

"The re-establishment of the health of patients, after the adynamic fever has terminated, is, perhaps, more rapid and complete, under favourable circumstances, than after any other disease whatsoever. I have known a convalescent gain flesh after the rate of two pounds in three days, for three weeks successively. Under less favourable circumstances the period of convalescence is uncertain. At one time it may be early, at another protract-

ed, and is influenced by many causes, such as any unsound condition in which the body may be left, the quantity and quality of the food, the purity or impurity of the air from locality, or the ventilation of the room or ward in which is the convalescent. Whenever an organ has suffered much during the course of an adynamic fever, convalescence is very apt to be protracted, that organ requiring time to resume its healthy functions, or to adapt itself to the new circumstances which may arise out of its diseased condition. Thus, the functions of the brain may remain disturbed for weeks, or those of the lungs be imperfectly performed. Diet, also, influences the period of convalescence; for the weak and susceptible state of the stomach and bowels, together with an eager appetite, render it exceedingly difficult to arrive at health without frequent checks; every little indulgence or excess being immediately followed by general disorder. The continuing to breathe the impure air of a bad locality, or of a close room, or ill ventilated ward (surely these are all localities, and require not specification, after being preceded by a term so general) retards convalescence; and, from this cause, one sometimes sees patients quite at a stand-still, (we know of no variety of standing from that of *standing-still*; standing excludes motion) for many days; convalescents are extremely susceptible of cold, and consequent organic inflammation. I knew a young woman, who lost her life from rheumatic inflammation of the lining membrane of the heart, brought on by exposure to currents of air, and to a damp atmosphere; convalescents are always importunate to be allowed to sit up; but their request must not be readily acceded to." P. 90.

The diagnosis of fever is easy to an experienced observer, but, to those who have witnessed only a few cases, it is occasionally attended with considerable difficulty.

"There are only two affections," says our author, "with which the adynamic fever is likely to be confounded, namely, delirium tremens, and a febrile state which accompanies the latter stages of some diseases of the urinary organs, and there is so much similarity in the disordered condition of the nervous system in all these diseases, that I think it would not be difficult to show that they are only modifications of the same thing, produced by different causes. They may, nevertheless, be easily distinguished. Delirium tremens is known by the peculiar and excessive tremor of the muscles, from which its name is, in part, derived; by the very short and breathless respiration, caused by the tremor affecting the diaphragm; and, also, by the vacant, unsteady, and staring roll of the eye; by the constant and extreme agitation, and by the history of the patient's habits. The febrile state, which accompanies affections of the urinary organs, is at once distinguished by the presence of the local disease." P. 102.

There is a circumstance of some importance, however, which is overlooked by Dr. B. in his observation on diagnosis. The physi-

cian is not always called in at the commencement of the disease, and often cannot procure such information respecting its incipient symptoms as can be depended on; and, therefore, it is not a rare occurrence that, mistaking the effect for the cause, he pronounces his patient to be labouring under fever, while he is only labouring under the effects of some primary organic inflammation. In the last stages of pneumonia and bronchitis, we have seen symptoms developed, as similar to those of idiopathic fever as it was possible to conceive; and, believing, as we do, that organic inflammation is a much more frequent attendant upon, and consequence of fever than Dr. B. imagines, it is sometimes a task of difficulty to rank cause and effect in their natural relative position, in the absence of a satisfactory history of the case.

In treating of the pathology of fever, Dr. B. divides the morbid changes observable after death, into such as are proper, and such as are accidental, limiting his first class to those of the brain and fecal tube; and then gives directions by which we ought to be guided in conducting our post-mortem researches. They are so correct and important that we will not mutilate them by an extract.

"In all dissections, in which it is desirable to ascertain the exact condition of the vascular system of the brain, the examination should commence with the head; for, if the chest is inspected first, and, as is generally the case, the superior cava or the subclavian veins are divided, the gorged veins and sinuses of the brain will empty themselves through the jugulars into the chest, and so modify very much the appearance. Hence, it is common, under these circumstances, to find the larger veins of the pia mater empty and flaccid, while the smaller are gorged. This emptying of the veins of the pia mater, and of the sinuses, is very much brought about by the great pressure the brain sustains from the force employed to tear off the cranium; a force often sufficient to diminish the conjugate diameter of the brain one inch during the separation of the dura mater. The pressure from this forcible separation acts, in a degree, on the principle of an exhausting pump: it forces the blood out of the veins of the sinuses; and when the skull-cap is removed, and the pressure acts no longer, air will not unfrequently find its way through the divided vessels in the chest into the larger veins of the pia mater (?) to supply the place of the blood which has been forced out of them. On the same principle, air will sometimes get into one of the larger veins of the pia mater through a wound in the dura mater, the chest not having been opened." (This is, we think, the true rationale of the appearance of air in the vessels of the head in all cases.) "So great an influence has the division of the veins in the chest, in allowing the escape of blood from the venous system of the brain, that I have seen all the blood disgorged from the posterior part of the plexus choroides, leaving its vessel flaccid and empty, while the anterior part remained excessively

gorged; the evacuation of the posterior part of the plexus being favoured by gravitation, the body lying on its back, while gravitation opposed the disgorge of the anterior part of the plexus choroides, on account of its peculiar situation and inclination. The morbid appearances depend much on the period at which the dissection is made, and on the cause from which the patient (proximately, we presume, is meant) died; dissection, therefore, should be performed as soon after death as practicable. The bodies of adynamic fever patients seldom grow stiff after death, as the bodies of those who have died from other diseases. The excessive prostration of the muscular powers peculiar to this fever, prevents the last act of life, the contraction of all the muscles, taking place to the same degree as in other cases." P. 113.

In the head the most ordinary appearances are a sero-gelatinous effusion between the arachnoid and pia mater, opacity and thickening of the arachnoid membrane, (the opacity Dr. B. considers as partly the effect of inflammation, and partly the result of maceration in the fluid lying between it and the pia; but it can only be in such bodies as have been inspected long after death, that we can ascribe much to this latter cause) turgidity of the larger veins and sinuses, except when the serosity is copious,—vascularity of the brain, displayed by making transverse incisions through its substance, when numerous bleeding puncta will appear,—turgescence of the plexus choroides, (this vascular tissue is very seldom highly injected, and never when there is much water within the ventricle) and a variable increase of serum in the ventricles. In some cases the arachnoid is rendered thick by an adventitious deposit, and the fluid beneath it is semi-opaque; but he has never seen fibrine effused between the membranes, and although the brain is generally firmer than natural, when the arachnoid is thickened, in common cases it is of the ordinary consistence.

"When a patient dies from the urgency of the adynamic fever itself, there will be invariably found a greater or less effusion under the arachnoid, with a corresponding one in the ventricles, never exceeding about three drachms. When, therefore, a much larger quantity is discovered in the ventricles, it may be concluded, that it arises from some other cause than the fever, even though there be effusion under the arachnoid: and, if there is this greater effusion in the ventricles without any effusion under the arachnoid, and an effusion under the arachnoid is invariably found in cases fatal from the urgency of the fever, it follows, that death, as also the ventricular effusion, has been produced by some other cause than the fever, which cause may justly be concluded to be a retention or suppression of urine; seeing that, in every case of copious ventricular effusion, death has been preceded by one or other of these affections of the urinary organs." P. 121.

The sentiments contained in this passage we dissent from *in toto*, and we caution the inexperienced from adopting them without

very mature deliberation. In many cases of the *purest adynamic fever* (and the observations now advanced are the result of multiplied experience) we have found water under the arachnoid *without any effusion* into the ventricles, and, when effusion co-existed with the sub-arachnoid deposit, we have found the quantity to vary from 3ss. to ʒiv. or more; and, that this plus-quantity of ventricular fluid could not have been the product of suppressed or retained urine is rendered indisputable by the *fact*, that, in many instances, there was *no derangement of the urinary organs*. It is incorrect, therefore, to assert, in terms so unqualified, that, whenever the case proves fatal, through the malignancy of the fever itself, both ventricular and sub-arachnoid effusion will be found, and that the amount of the former will never exceed ʒij. Besides, admitting that this increased quantity of serum arose from a diminished quantity of urine; will any one be disposed to believe with Dr. B. that death, as well as this ventricular effusion, has been produced by either a retention or suppression of urine? This is certainly "riding a hobby too far." We are not among the number of those, who can find the cause of death *in any case of fever*, an effusion within the head. Such a phenomenon only ranks with us as one termination of a preceding action, to which we ascribe much more importance; and we hold it to be a confounding of causes with effect, and our error not confined to our pathology, but extending to our therapeutics, to maintain, that a non-secretion or retention of urine, arising from a palsied condition of the kidneys or bladder, which condition itself arises from a prior derangement of the brain, can, in any case of fever, be the cause of dissolution. Were Dr. B.'s views correct, very few, indeed, are destroyed immediately by fever. The profession have hitherto nearly overlooked a point involving the most important consequences, and the value of the catheter has not been sufficiently appreciated in its treatment.

Passing over our author's morbid anatomy of the abdomen, which we will refer to in our review of the next article,\* we are informed that the bronchial lining is, in general, preternaturally vascular, and has often adhering to it much inspissated mucus, which the patient has not had strength to expectorate. This mucus is sometimes mixed with blood, and sometimes with pus, when the mucous membrane is found thickened and soft, as well as vascular (and may be easily abraded by the nail,)—the lungs are, in general, too heavy, and do not completely collapse, their natural structure is not, however, necessarily altered, their inferior and most depending portions are gorged with blood, and are either of a livid or purple colour;—sometimes large portions of them are hepatized and irrespirable, at others, circumscribed livid patches, resembling the spleen in structure, will be discovered in the middle of

\* Review of Dr. Bright's work, which we have been obliged to defer till the next Number.—Ed.

their substance, and which are considered to have been formed by the effusion of blood before death. The heart is often relaxed and soft, and, in one case, where the symptoms resembled those of delirium tremens, it was found pale, flabby, and easily torn. The inner surface of the aorta will, on some occasions, be seen "of a dark scarlet colour, which may be attributed to staining from the presence of blood in the vessel." The same appearance we have often met in the interior of the heart, especially in its valves and the mouths of the large vessels. P. 139.

Before leaving this department of the subject we wish to observe, that Dr. B. constructs his rationale of the morbid anatomy of fever upon one and the same principle, "debility." If effusion exist within the head it is in consequence of impaired nervous energy; or, if the brain be turgid with blood, it is because its vessels are labouring under a congestion, from deficiency of power to carry forward their contents. If the intestines be found tympanitic, and their mucous tissue inflamed or ulcerated, it is because their muscular powers being prostrated, and their peristaltic action weaker in effect, that their accumulating and putrefying contents extricate thin gases, and irritate the canal to inflammation and disease; or, if the chest betray effects of disorder, whether they be injection or thickening of the bronchial lining, engorgement or consolidation of the lungs, they result, either from inability to expectorate the ordinary secretion, which becomes viscid when retained and irritates the mucous membrane to inflammatory action, or from a dissolved condition of the blood, which, assisted by gravitation and enfeebled respiration, loads the parenchyma of the lungs to a degree incompatible with life.

This principle (debility) is, therefore, one of great use and applicability with our author; but, while such illustrations of symptoms and disease establish his consistency in adopting the epithet *adynamic*, we question very much, whether their simplicity is a sufficient test of their correctness, or their consistency with the title a satisfactory guarantee of their fidelity to nature. We are strongly inclined to suspect, that effects are sometimes mistaken for causes, and it were as difficult to convince us, that it is viscid mucus which inflames the bronchia, and putrid fæces which ulcerate the intestines, as it were to persuade us that it was the pus expectorated which generated tubercles, or that mucous stools were the cause of diarrhoea.

In another part he observes, that—

"The atonic character of inflammation accompanying the adynamic fever is, moreover, shown in the products of that inflammation, all of which are nearly destitute of fibrine, the characteristic of tonic or *healthy inflammation*. Where the serous membranes are the seat of inflammation, the effusion, instead of being fibrine, is sero-purulent, with merely shreds or flakes of albumen floating in it; so that there are either no adhesions between these membranes, or the adhesions are partial and

slight. Where the mucous membranes are the seat, the product is merely mucus, and seldom or never mucus combined with pus. Where the inflammation is in the parenchyma of any organ, as of the lungs, constituting pneumonia typhodes, the consolidation is in a degree trifling compared with ordinary cases of pneumonic inflammation." P. 42.

Now we can safely assert, that we have seen as active signs of inflammation during life, and as varied results of inflammation after death, in cases of fever, as in any individuals of the order phlegmasiæ. We have seen the pleuræ adhering, nay incorporated as it were; the parenchyma of the lungs fleshy, firm, and ir-  
respirable; the surface of the heart studded with white specks of coagulated lymph; flakes of fibrine floating both in the pleural cavities, and that of the pericardium; the intestines agglutinated so firmly that they were torn by effecting a separation, and several other of the abdominal viscera, as the liver to the diaphragm, the omentum to the spleen, and the fundus of the bladder to the ileum, united by adventitious attachment. Then as to inflammation of the mucous membrane, we have witnessed cases, during which many pints of nearly unmixed pus were expectorated. One we may specify. A woman, of middle age, was seized with the ordinary symptoms of continued fever; but the cavity mainly affected was the chest. She first had a teasing cough, accompanied with mucous sputa; her breathing was hurried, her cheeks gradually became dusky, and then of a leaden hue in the centre; and her dyspnœa increased, and she began to expectorate large quantities of almost pure pus. The purulent expectoration became, at last, so copious, that confirmed consumption was apprehended, and it was only the stethoscope which rectified our diagnosis. She died, and upon dissection, ulceration of the bowels, inflammation, thickening and softening of the mucous membrane of the air tubes, without any change of parenchymatous structure, were discovered.

After controverting, in a very ingenious and able manner, the doctrines of Clutterbuck and Broussais, and arguing for the general or systematic nature of fever, the following propositions are laid down:—

"That the adynamic fever has no local seat: that its nature is a morbid condition of the blood, produced by the operation of the primary cause, the respiration of a contaminated or poisoned atmosphere: that the morbid blood, acting on the brain and nervous system, is, of itself sufficient in very many instances, to bring about the very great derangement and imperfect performance of all the functions of the organic and of the animal life; which great derangement and imperfect performance of all the functions constitute the phenomena of the adynamic fever." P. 161.

The first proposition we deem incontrovertible by those who have had an extensive experience of fever, and whose minds have neither been warped by theory, nor forestalled by prejudice. The opinions of Broussais, al-

though followed almost exclusively in France, are quite heterodox and untenable; and, although those of Clutterbuck are more plausible in theory, and more countenanced in practice, they are neither established by symptoms, nor can they explain appearances. But while we concur with our author in rejecting the doctrines of the Localists, as unnatural in not accounting for appearances, as illogical in ranking effects into causes, and as unpractical in fixing the eye of the practitioner upon one symptom and one organ, while it ought to comprehend within its glance every symptom and every organ; we have not yet determined on adopting his view of the disease, though recommended by its antiquity and the probabilities attending it. In reviving the humoral pathology of fever, Dr. B. ought to have called in the aid of chemistry, as the surest, if not the only means of establishing his doctrine. For, although every symptom and phenomenon receive a satisfactory explanation, by supposing the blood to be diseased, without a confirmation of this supposition, by an analysis of that fluid at the onset and during the progress of fever, such a view could only be regarded as an hypothesis. Had it been proved, that the blood of a fever-patient differs, in its internal constitution and external character, from that of a healthy person,—that this difference is discoverable prior to any other palpable febrile symptom,—that the symptoms proceed in intensity, “*pari passu*,” with this alteration, and that the fever disappears with the disappearance of this difference,—Dr. B. would have had a strong claim upon the faith of his readers, a claim not to be resisted; but, when no such, and, indeed, no analysis is furnished, when the only observation, made upon the chemistry of febrile blood in the entire volume, disagrees with the results of those who have examined it,\* and when we believe, that all the phenomena of fever might be otherwise accounted for, we must for the present, at least, withhold our assent. That the external character of the blood, during the *advanced stages* of this disease, is changed, there is no doubt; and, that a diseased state of this fluid might and would produce *symptoms of fever*, we feel no scruple in believing; but, that fever is *in all cases* produced by such an alteration, and that such an alteration exists *before* every other febrile phenomenon, we have several reasons for doubting. At the commencement of the excitement, when the lancet is generally employed, the blood drawn does not differ very materially from that taken out of a healthy system, or one affected by organic inflammation. It separates freely into its constituent parts, and presents frequently, we might say generally, an inflamed surface; but, it is principally as the fever advances, as the whole body becomes more relaxed and

disposed to putridity, and as the energies of the nervous system sink, that this fluid assumes a loose, dissolved, and peculiar aspect.

But, having already devoted much space to this review, we must *at present*, abstain from dilating on a subject involved in so much darkness, and which yet requires the most patient and minute investigation, and follow our author to what is equally interesting and not less useful, the treatment of the adynamic fever.

“The treatment of the adynamic fever resolves itself into four principal objects: namely, to arrest the progress of its development; to cure the disease when fully established; to subdue any organic inflammation which may accompany it; to conduct the patient from convalescence to health.

“And, the means by which these objects are to be accomplished, are emetics, aperients, bark, cold-affusion, ventilation, febrifuges, blood-letting, mercury, hyosciamus and opium, stimulants and regimen.” P. 166.

“Emetics,” he observes, “are *never required*, except when the attack was without the intervention of an accidental cause.” P. 166. Now, we really see no good reason for such an exclusive dictum respecting this class of medicines; and, were we friendly to such sweeping generalities, we would have no hesitation in preferring one which conveyed a sentiment the reverse of that contained in this extract. When the fever creeps slowly over the constitution, and gradually takes possession of every organ and function, we believe that neither emetics nor blood-letting will make much, if any impression upon the progress, or character of the disease. In such cases mischief has been imperceptibly accumulating, the functions of life are slowly but surely undermined, indifference gives way to languor, languor to prostration, and prostration to pain. The poison is insinuated, unfelt and by degrees, into the fountain of life; and it is not until its stream be thickly charged, that the unwary patient perceives his preceding disorder was only the commencement of disease. The enemy has gained too firm a grasp of his victim to be surprised and vanquished by a single stroke, and, although an emetic may be occasionally necessary, it will be seldom found to ameliorate the symptoms, or abridge the attack. Not so, however, in cases arising from cold, disordered stomach and bowels, or other accidental causes. The boundaries between health and sickness are here well defined; the moment when the poison began to operate is easily ascertained, and the patient is not surprised into fever by the stealthy mode in which he has been involved. Under such circumstances, if an emetic be *early* employed, it will often arrest the progress of disease, or impart to it a mild and gentle type. We cannot, therefore, sanction the following passage,—when the fever attacks through the intervention of an exciting cause.—

“The development is so sudden, as not to allow the practitioner an opportunity of attempting to arrest it. The development of the fever, therefore, from an accidental cause

\* “Blood drawn while the adynamic fever is urgent, is surcharged with carbon;” p. 142. According to Dr. Sunderland, there is no free carbonic acid in typhous blood. The latter grounds his opinion on his own experiments.

cannot be prevented. But, it is far otherwise, when the way of attack is spontaneous, and the development slow and progressive; and if the aid of the practitioner is sought during this period, it will, in very many instances, stifle the fever in its birth, and save the patient from a long and serious illness." P. 200.

A certain time is necessary to habituate the system to a morbid change, and if any thing be done before the formation of this habit, which will impart a sudden and forcible shock, the tendency to disease is frequently destroyed, the chain of morbid action broken, and a new and healthy impulse given to the powers of life. Now, an emetic, or blood-letting, well-timed, acts as alteratives, we believe, in some such way; and, having seen many cases prevented and others shortened by such treatment, we consider it necessary to hold forth our veto against a sentiment, in itself, too exclusive, and which, when practically acted on, is calculated to deceive.

Speaking of aperients, he observes, that—

"The best are rhubarb, castor oil, and senna, combined with manna. The dose always to be moderate; that of rhubarb varying from five to fifteen grains; of castor oil, from one drachm to half an ounce; of infusion of senna, from one drachm to an ounce, with a proportionate quantity of manna. The larger of these doses are (is) to be prescribed only at the commencement of the disease; and, as a general rule, the longer the disease has existed, the less is the dose required. Although these aperients are all eligible, I have found rhubarb the most so; and so effectually does it answer the purpose, in all states and stages of the disease, that I do not hesitate to recommend its use, to the almost entire exclusion of the castor oil and of the senna." P. 167.

The following passage, we acknowledge, startled us:—

"*Bark is as serviceable in arresting the formation of the adynamic fever, as it is in arresting the progress of an ague; and it may be most advantageously employed when the disease has been for some time on the decline, and distinct remissions occur; and, also, during the early stages of convalescence, particularly in hospital patients, who have always to contend more or less, with an impure air.*" P. 170.

We have already quoted a passage which informs us, that the development of fever from an accidental cause cannot be prevented by any mode of treatment; and, in this extract, we are taught that bark is as effectual in preventing the formation of an adynamic, as it is in curing an intermittent fever; consequently, admitting that one-third of all our cases of continued fever arise from accidental causes, (we use the word *accidental*, of course, in the author's own meaning,) the bark must be supposed to fail in curing one-third of all the cases of ague in which it is tried. Whereas the fact is, that, with some obstinate exceptions, the sulphate of quinine will arrest the progress of ague, as certainly as mercury will arrest the progress of syphilis. This conclusion, however, although drawn from his own premises, we do

not imagine he will defend, and, therefore, upon it we will insist no further; but, if bark can cure continued fever as infallibly as intermittent, why is the mortality of typhus so sadly disproportioned to that of ague? There is some obscurity in the structure of this passage, and, therefore, we may be misinterpreting the author. We cannot see how a disease can be *arrested before it is formed*; its *formation* may be *prevented* and its *progress arrested*; yet, supposing the Dr. intended to convey this meaning, how can he add in the next clause that the bark "may be most advantageously employed when the disease has been for some time on the decline?" Surely, if the formation of the disease can be prevented, or even its progress arrested by bark, it would be most advantageously employed at its commencement, and not at its decline. If we can cut short, or prevent typhus, by the use of this medicine, as certainly as we can cut short or prevent a fit of the ague, why allow it to pursue its wonted course; why expose the patient to the pains and hazards of a protracted illness? and why administer at the close, in preference to the outset of the fever, a drug endowed with such therapeutic power? This is, at least, to trifle with sickness and to toy with death.

We attach no such value to bark in the treatment of *continued fever*. It is only when remissions are observable, that it can be employed with any propriety or prospect of success in the *early* stages; if exhibited during the period of excitement, when the skin is hot, the face flushed, the head disordered, the heart labouring, and all the functions disturbed, it will certainly do harm. But, if used in the decline or during convalescence, when the "heat of the battle" is over, and Nature, exhausted by the struggle, only requires strength to "gain the day," we then agree with our author in strongly recommending it, as, perhaps, the most effectual restorative we can have recourse to.

The undeserved neglect which cold affusion has lately suffered, renders the following judicious remarks peculiarly valuable.

"At any period during the course of the adynamic fever, and in all cases which are not accompanied with local inflammation, cold affusion may be advantageously employed, whenever there is a burning heat of the whole surface of the body; and, particularly, where the skin is very dry, harsh, and contracted, and the prostration of strength great. It diminishes the heat of the surface, saves the strength, disposes the skin to perspiration, and the patient to sleep. I have known delirium cease for several hours after the use of the cold affusion. When the preternatural temperature of the skin is only partial, affusion would be prejudicial; but ablution would be grateful and serviceable; and the hot surface may be sponged with vinegar and water frequently in the course of the day." 173.

We are sorry, after such sentiments, to find that cold lotions to the head are disapproved of.

"The utility of evaporating lotions is, in-

deed, very questionable, either in these or in more violent cases. I have never seen any decided benefit from them; and they often give cold, and excite a languid inflammation of the eyes, with puriform discharge; and excite or increase pulmonary catarrh." 209.

Perhaps, in one case out of a hundred, the consequences here stated may be produced by such applications, but we have not had experience of them even to that amount; and we are inclined to believe, that, when they do occur, they are not to be attributed to the use, but to the abuse of the remedy. If cold be applied *only* when the temperature of the head is *above* that of the body, (and it is in such cases only that it will do good,) we are convinced that it will seldom or ever cause any disagreeable result. Evaporation will abstract the excess of heat and not sink the natural temperature, unless too long applied; but if used when the scalp is not warmer than the rest of the surface, the evaporation will, probably, do more essential mischief by lowering the energy of the brain, than occasion such local inflammation. The consequences, then, are to be ascribed not to the application, but to the practitioner. Exposing the head to a cold atmosphere, after shaving the scalp, will often remove severe headach; and increasing the cold by the application of evaporating lotions, or an ice-cap, will frequently remove symptoms the most dangerous and distressing, without the aid of other treatment.

His views of ventilation are equally just and important.

"I have seen, over and over again, patients begin to improve, without the aid of medicine, the moment they have escaped the foul atmosphere of their own dwellings. And this is not to be wondered at, on the belief that the adynamic fever results from a certain condition of the blood, produced by the continued breathing a contaminated or poisoned atmosphere; for then the legitimate inference is, that the unhealthy condition being no longer kept up, but, on the contrary, diminished by the substitution of a pure for an impure air, the effects of that unhealthy condition diminish also, and thus the adynamic signs subside. The impracticability of good ventilation is the reason why it is difficult to cure the poor at their own home; where a whole family, perhaps, is cooped up in a small room filled with dirty, musty furniture, and in which all their little domestic operations are carried on. The adynamic fever, under these circumstances, is always protracted; the efforts of the physician are baffled, and all the remedies which he can administer are barely sufficient to prevent the patient losing ground; much less to conduct him to a safe and speedy convalescence. These are truths which plead in favour of those noble institutions, hospitals, and declare how necessary they are to the very salvation of the poor afflicted with the adynamic fever." 175.

We embrace this opportunity of observing that, although fever is a most prevalent and destructive disease in London, owing to its crowded population, the filthy state of many

of its districts, and the wretched habits of its poor; and, although there is *only one hospital* appropriated to fever patients, the support it receives is very limited, and its annual income insufficient to cover expenditure the most reasonable. When we consider the contagious nature of this disease, the ravages which it makes when it enters a family, and the difficulty that is encountered in circumscribing its influence, we cannot help being astonished that an enlightened public can so long overlook the merits of an establishment, to which the lord is equally indebted with the peasant, and which lays claim to a liberal support, as well from the judicious and efficient mode of its management, as from the amount of good which it effects with means so limited.

Had our author confined himself to one form of fever, and had that form been significantly expressed by the term *adynamic*, his remarks upon the treatment in general, and on blood-letting in particular, would have, in our apprehension, been appropriate. But, when we consider that under this epithet he groups every form and degree of this disease, we regard his practice throughout as timorous, and his recommendation of the lancet calculated to discountenance its use. After a few cautious observations in its favour, he winds up the subject by remarking, that "I regret to say, the information, which I have been able to obtain on this point, leads me to the melancholy belief, that, within the last few years, adynamic fever patients have sustained more injury than benefit from the abstraction of blood." 186.

We cannot determine how Dr. B. has arrived at this conclusion, because we have no knowledge of the cases from which it is drawn; but we can safely and certainly aver, that our experience has led us to a very opposite conclusion. We have no doubt but, that during some epidemics, when the type of the disease is characterized by debility, and, in some constitutions, depressed by misery, worn out by nature, or exhausted by fatigue, that the abstraction of blood must either be very limited, or entirely abstained from. But we are not aware that our author makes the above remarks with such peculiarities in view. His language is general and unrestricted, and as such we oppose the doctrine it conveys. Fever, in the abstract, as it has lately appeared and now exists, is not an adynamic affection, although it may become so by the peculiarities of the subject it invades, or of the circumstances attending its attack. Whenever it is seen in the low and *adynamic* type, it is seen modified by some contingent cause, and, therefore, a corresponding modification of treatment will be necessary. But such cases are exceptions to a general rule, variation from the natural portrait; and, although it may be safe and prudent to arrange them into one class, to designate them by a specific title, and to prescribe for them a certain plan of treatment, it is imprudent and unsafe to confuse the exception with the rule, the variation with the standard, and subject a whole genus of

disease to curative means only adapted to a species. At the commencement, and during the first stages of fever, blood may, in general, be safely and successfully drawn, apportioning the quantity to the power of the pulse, and the inveteracy of the other symptoms; and we fear that he, who will seal up his lancet-case, and feed his patient upon bark at the outset, will ere long have, at least, as many facts from which to draw a different conclusion, as Dr. B. can advance in support of the one we have extracted.

Our author is opposed to the internal use of mercury, believing it to irritate the mucous membrane of the alimentary canal, and thus dispose to or accelerate ulceration; but, while he prefers rhubarb alone, he admits that there are some instances in which it may be had recourse to with advantage, and strongly recommends its employment externally. As we concur entirely with his views on this point, we shall beg leave to give his sentiments in his own language.

"One of the characteristic conditions of the adynamic fever most difficult to combat, and most desirable to relieve, is the diminution or suspension of the secretions; and the power of the mercurial frictions to restore the secretions exceeds all other remedies, and is, indeed, very remarkable. I have witnessed such speedy convalescence from the operation of mercurial friction, that my mind is quite made up, as to the propriety and utility of this remedy. I believe it may be advantageously employed in all urgent cases of the adynamic fever; and, particularly, when the brain is much affected; as, where there is oppression or lethargy from fulness of blood about the head; where there is restlessness and noisy delirium; or, where inflammatory action is going on; and, indeed, in all cases, and at all times, when the return of the secretion is obstinately protracted. It has quite astonished me, on some occasions, to see how quickly the tongue will cast off its dry brown or black thick coating, the lips and teeth become moist, and the skin soft, and the brain resume its natural functions. Mercurial friction has produced more rapid and favourable changes in very severe cases of the adynamic fever, than any other remedy I have ever seen administered. The average quantity to be rubbed in is half a drachm of the ung. hyd. fort. twice a day." 192.

There is no point connected with the treatment of fever, which is involved in so much obscurity, and requires more judgment and experience, than the use of sedatives and stimuli; and we are not acquainted with one general rule by which to be guided in their administration. We have seen opiates exhibited in cases apparently the most favourable, where constant tremor, jactitation, and watching evinced much nervous irritation, but without any good, and, frequently, with a bad effect; and it has not seldom occurred, that we have witnessed their beneficial operation in instances where the untutored observer would have, *a priori*, denounced them as misapplied

and injurious. The same remark is equally applicable to the use of stimuli in the last stages of fever, and we would recommend the greatest caution in the employment of all such medicines, believing that they invariably do harm, if they fail producing the effect for which they were intended. In cases which seem to require them, they should be at first given in very small doses, increasing, diminishing, or entirely withholding them, according to the nature and amount of the effect produced; as, we are convinced, that the most experienced can seldom tell before-hand whether, by their employment, he is going to relieve his patient, or aggravate his symptoms.

With such views of these medicines we cannot adopt the following rules, that—

"When there is noisy delirium, wakefulness, and restlessness, with an accelerated and easily compressible pulse, hyosciamus or opium may be given, *whatever be the state of the other symptoms*, and will be found valuable remedies. When the adynamic fever has been unusually protracted, and the patient is much exhausted and emaciated, a small allowance of wine, as four ounces, will be serviceable, and accelerate convalescence." 193—196.

In some protracted cases wine may be and is useful, but we object to every thing in the shape of a general rule or principle upon a subject of such obscurity; and, still, of course, excepting peculiar cases and epidemics, we believe it will be found that he who knows most of the nature of fever, and has acquired the *tact* of treating it most successfully, will use the least wine and opium, and, when he does have recourse to them, will use them "with fear and trembling." In the language of our author, "*nomine mutato*, and if he is in doubt, let the doubt be opposed to the free administration of stimuli." P. 204.

We have now laid before our readers a tolerably minute account of the Doctor's leading views, as to the nature and treatment of fever; and having done so, it is not our intention to follow him through the minutiae of his practice, as we cannot, by such details, present either much novelty or information. His individual remedies we have individually considered, and the pages which follow he exclusively devotes in applying them to symptoms as they rise, in accordance with the doctrines already reviewed.

We, therefore, dismiss this very unique and ingenious performance with the belief, that the points, in which we differ, arise chiefly from the restricted views our author takes of the nature of fever, and, that could we adopt the doctrine of fever being "*in natura et semper*" *adynamic*, we could subscribe to many of the sentiments which we have considered it our duty to oppose. But, differing from him, as we do, on this fundamental point, and believing, as we must, from the facts we have seen, and the observations we have made, that fever presents as many varieties of symptoms as there are varieties of causes, constitutions, and circumstances, and that it may appear under every grade of inveteracy, from the most

putrid type, where life is seen sinking in a mass of rottenness, up to the most intense phlogistic character, where the lancet must be handled and handled freely; we have been compelled to impugn many positions that were perfectly consistent with the author's leading doctrine. Fever must not only be seen to be described, but it must be seen under every changing form which changing circumstances can attach to it. In one epidemic its prominent feature its debility, in another it is strength. In one, enteritic symptoms will be found to predominate, in another, disorder of the head. In one constitution every important organ will successively feel the influence of its power, in another, it distributes its excitement in equal proportion throughout the body, passing along its stages without characterizing its attacks or stopping its progress, by any local affection, or leading symptom.

All these varieties must be seen and studied, and every modification of feature must be marked and noted, before we can, either with safety or success, sit down to delineate a faithful portrait. Had Dr. Burne confined himself to a certain epidemic, or to a certain combination of febrile phenomena entitled to the epithet he employs, his work had not occasionally sacrificed its fidelity to Nature for the sake of consistence, and the claims which it certainly has upon public patronage, had not been deteriorated by a comparison of the limited applicability of the principles it inculcates with the extensive magnitude of the subject which it treats.

From the *Lancet*.

**AN ACCOUNT OF ANEURISMS FOLLOWING ARTERIOTOMY.** By **GEORGE BUSHE, M. D.** of the Royal College of Surgeons in Ireland, and Assistant Surgeon of the Forces.

In this communication, it is my object to describe the different species of aneurism which I have observed to follow arteriotomy; and, I purpose to illustrate the same with appropriate cases. The undertaking, it is hoped, will not prove unprofitable, since the subject has not yet met with that attention which it so justly merits; however, it may be necessary to apprise some of my readers, that M. Desruelles has lately written on this subject, in the *Transactions of the Medical Society of Emulation at Paris*; but his paper, though it contains much useful matter, can in no way detract from the advantages that may accrue from the publication of the following pages; for the reader will soon learn that I have described forms of the disease untouched by him, and illustrated the same with instructive cases; therefore, without further prefatory remarks, I shall lay before the public what information I possess on this subject.

In the second volume of Sir A. Cooper's *Lectures*, it is written, "I have seen several cases of temporal aneurisms from arteriotomy in that vessel, one in Mr. Heusleight, a me-

dical student. I opened the sac, secured the temporal artery at its lower part, and was there obliged to secure many others entering the circumference of the sac, which had been excessively dilated." No doubt this case, in the language of Mr. J. Bell, was an anastomosing aneurism; and one, in all probability, produced by the operation of arteriotomy.

Again, if the reader will look to Mr. A. Burns' work on the head and neck, page 342, he will observe a case in which the temporal artery was opened for an apoplectic affection, to which succeeded an aneurism by anastomosis, and which Mr. Burns believed existed beneath the temporal fascia before arteriotomy was performed, and was the cause of the cephalic symptoms. It is with great deference that I would oppose such high authority as Mr. Burns, but from the extract I have made from Sir A. Cooper's work, together with the following cases, I am led to suppose, that aneurism from anastomosis is not a very uncommon sequel of partial or total section of the temporal artery, and that, in all likelihood, Mr. Burns erred when he gave it as his opinion, that the aneurism existed beneath the temporal fascia before the performance of arteriotomy; and that the true nature of the disease only became manifest after the division of the membrane.

*Case 1.*—On the 30th of April, 1826, G. Graham, private in the 54th regiment, was admitted into Fort Pitt General Hospital, in consequence of concussion of the brain, produced by a fall, sustained a few hours previously, when intoxicated. In the second stage of the disease, (as mentioned by Mr. Abernethy,) blood was taken from his right temporal artery, but no bandage was subsequently employed, as its application produced an increase of headach and general fever. The wound in the temple did not unite, and a pulsating tumour gradually formed, which, from the 18th to the 22d of May, bled frequently and profusely, though firm pressure was steadily applied; indeed, the tumour appeared to extend rapidly under this treatment. On the latter date a vertical section was made of the aneurism, and pressure reapplied; but, much to my annoyance, the progress of the disease appeared to be increased by the means adopted, and on the 26th, in consultation with Dr. Skey, deputy inspector of hospitals, and Mr. Millar, surgeon to the forces, it was determined to secure the trunk of the vessel near the root of the ear; this was accordingly executed; but we were again disappointed to find, that the steps adopted were totally ineffectual, as neither the growth of the aneurism, nor the hæmorrhage from it, were suspended; the exterior of the swelling on the 15th of June, being about the size of a hen's egg, ulcerated in the centre, fungated more externally, and its circumference, where the skin was entire, it possessed a purple colour, irregular surface, and œdematous feel; the whole mass being simultaneously moved with the contractions of the left side of the heart;

moreover his countenance was pallid, and his frame debilitated and emaciated from the protracted stage of suffering, and repeated hemorrhages. A consultation was held, and the removal of the diseased mass recommended. Assisted by Dr. Skey, I proceeded as follows:—

Pressure being made on the carotid artery, an elliptical incision was carried round the base of the tumour, and its removal completed by dissecting from above downwards. In prosecuting this part of the operation, the knife had to be carried freely into the body of the temporal muscle, in consequence of the aneurism being lodged in it, particularly at its lower part behind the zygoma. But little blood was lost during the operation; but when the pressure was removed from the carotid, several large branches sprang in the body of the temporal muscle, one of which was secured by ligature, but the others being numerous and deep seated, the application of sponge and graduated pressure were employed to restrain the bleeding. On the third day the wound was dressed, after which it soon granulated, and rapidly healed. The morbid parts corresponded to the structure which Mr. J. Bell describes as peculiar to aneurism from anastomosis.

*Case 2.*—In June, 1826, — Salmon, publican, in Fort Pitt Barracks, had an apoplectic fit, for which he was blooded from the left temple, by assistant-surgeon Ford, to which, in the course of fourteen days, succeeded an aneurismal tumour, about the size of a filbert; the wound in the temple never having healed, it bled freely and repeatedly, was of a purple colour, and attended with the peculiar doughy feel so remarkable in Graham's case; pressure was applied, but Mr. Ford informed me, that it had most manifestly exasperated the disease. I removed the tumour (which laid above the temporal fascia) without difficulty, but to restrain the hemorrhage, I found it necessary to apply two ligatures, and fill the wound with sponge. The excised parts resembled those removed from Graham's temple.

*Case 3.*—In Jan. 1827, Captain M., of the 3d regt. of foot, was affected with inflammation of the iris of his right eye, for which the temporal artery was opened, but as he stated that the pain of his head was increased by the bandage, after a few hours its application was discontinued; the wound did not heal, and an aneurism formed, which at the end of eight days, had acquired the size of a hazel nut; pressure was applied, but repeated attacks of hemorrhage ensued, and the tumour, on the thirteenth day, was as large as the segment of a walnut. An incision was then made around its base, so as to divide the superficial vessels passing into it; and, after three trunks were secured, the pressure was reapplied; but this expedient did not answer, for, on the eighteenth day, the aneurism being rather larger than that of Graham's, its removal appeared imperative. Assisted by Mr. Fryer, assistant surgeon, 46th regt., I accomplished the desired object, which was rendered very difficult in consequence of the number of vessels en-

tering its base, and the depth at which it lay beneath the zygoma, in the substance of the temporal muscle. The bleeding vessels were so numerous, that to apply ligatures to them, at such a depth from the surface, in a narrow cavity, appeared impossible, therefore by plugging the wound with sponge, the hemorrhage was restrained; a healthy wound followed, and in a fortnight it had skinned over. This aneurism I injected and corroded, by which means its true structure was clearly exposed.

I have now mentioned three cases, in which anastomosing aneurisms followed the operation for arteriotomy, and from them may be deduced much useful matter. In the section of Sir A. Cooper's lectures before alluded to, it is stated,—"Aneurisms are to be prevented after arteriotomy, by the complete division of the vessel." This opinion being derived from such high authority, carries with it much weight, therefore it is with great deference that I would oppose it; but in two of the above cases the vessel was cut across, after the desired quantity of blood had been abstracted, viz. in those of Graham and Captain M. Hence, I am firmly convinced, that the means recommended by Sir A. Cooper will not prevent the formation of anastomosing aneurism, notwithstanding it will, undoubtedly, that of a species hereafter to be mentioned, viz. that of the trunk itself. I cannot comprehend how a complete division of the vessel could possibly operate against the development of a structure, consisting of a dilatation of the anastomosing vessels, though it is easy to understand the rationale of its operation, in the prevention of false aneurism of the temporal artery; and, moreover, I am convinced, that in the most careful attempts that can be made to complete the section of the vessel, the temporal fascia will be divided, by which a communication is opened between the superficial and deep seated vessels, an event that tends to increase the malady, and render the removal of the diseased parts much more difficult. In exemplification of this contrast the depth at which the aneurisms of Graham and Captain M. lay in the body of the temporal muscle behind the zygoma, and the supply of blood they received from the deep temporal arteries, with the superficial situation of the aneurism of Salmon, viz. external to the temporal fascia; the facility with which it was removed and the hemorrhage restrained, the deep temporal arteries being free from disease. From these facts, I cannot withhold recommending a discontinuance of the common practice of cutting across the vessel, when a sufficient quantity of blood has been abstracted; for though such a proceeding will prevent the formation of aneurism in the trunk of the vessel, still, if there is a disposition to the formation of anastomosing disease, it will, in my mind, increase it, and the establishment of dilatation of the trunk itself can be prevented by the application of pressure. In the cases of Graham and Captain M., pressure was not employed after the operation of arteriotomy; but in Salmon's case, it was applied in the usual way, still it did not prevent

the formation of anastomosing disease; therefore I am disposed to conclude, that in some individuals, there is, from causes yet unknown, a disposition to this peculiar morbid action which is called into existence, no matter how completely the vessel is divided, or how accurately pressure is employed.

In the three cases related after the appearance of the disease in the anastomosing vessels, pressure was applied, and in all it appeared to expedite the growth of the tumours, increase the central ulceration, and finally to produce much constitutional annoyance.—These effects I have observed to follow pressure when applied to anastomosing aneurisms of spontaneous origin; and the same results have been pretty strongly hinted at by John Bell, and some other authors. Therefore I would recommend, that as soon as the disease becomes manifest, pressure should be immediately discontinued.

In the case of Captain M., a circular incision was made around the base of the tumour, as recommended by Sir Astley Cooper, and afterwards firm pressure was applied; but I did not carry the incision to the bone as proposed by Sir Astley, as in the case alluded to by him, the aneurism was situated on the forehead, where he had not to contend with deep seated vessels, situated in a thick muscle. The result of the attempt in the case treated by me, the reader will recollect, proved abortive; but the failure can easily be explained in this way: by the incision, the superficial, temporal, and frontal arteries, were only divided; and the aneurism having its supply of blood principally from the deep temporal arteries, its growth continued, because they were left undivided, hence I shall not again adopt this proceeding. But it may be asked, if the incision was carried through the body of the muscle down to the bone, would it then not have answered? In reply, I would say, that we can never judge of the extent of the base of such aneurisms, by their external appearance; an opinion which the above cases will justify; and as, by external examination, we cannot gain knowledge of their inward extent; an incision, though carried widely round the base of such tumours, might, in all probability, pass into the substance, thereby give rise to troublesome hemorrhage, and an increased rate of their growth. And even if, in such cases, we could inclose the morbid parts with an incision, I should fear that the trunks of the deep temporals, would pour out blood very freely, and, in all probability, require dilatation of the wound, for the application of ligatures to stem the hemorrhage; and, above all, as the encircled mass could only receive blood from the bone, (a quantity small indeed,) sloughing of the diseased parts would be the consequence; a process, which, though radical in its effects, would be attended with so much irritation and delay, that I cannot look upon it as a desirable curative mean.

In Graham's case, the tumour was divided throughout its vertical extent, and afterwards plugged with sponge; but the reader will re-

collect, that after this plan of treatment, the disease extended more rapidly than heretofore, therefore I cannot agree to the following passage in Sir A. Cooper's lectures:—"The operation best calculated to cure aneurisms of the scalp, is to cut directly across them, and to make use of pressure to stop the bleeding, to prevent the course of the blood through the swelling, and to produce adhesion of the sides of the sac."

It was stated that, in Graham's case, a ligature was applied to the trunk of the vessel, and that it did not affect the growth of the aneurism; but when we recollect that the morbid mass was fed, not only by the superficial temporal, but also by the frontal and deep temporals, its failure can be easily explained. Hence it may be unnecessary to add, that such a proceeding is not at all adapted, even to suspend the progress of such diseases.

From a consideration of what I have now written, I have no hesitation in recommending excision, as soon after the formation of the disease as possible, as the means best calculated to establish a safe and radical cure.

The next species of aneurism, arising from arteriotomy, which I shall describe, is that of the trunk of the vessel itself; a form of disease of which I have seen many cases, particularly in the ophthalmic hospital of this establishment (which I had charge of, from September 1826 to May 1828.) This is the form of aneurism described by Desruelles; and I can safely say that I never saw a case of this disease, where pressure was properly applied, after the closing of the wound, though the vessel was left undivided. I am particular in this statement; because Desruelles has assigned imperfect division of the vessel, as a cause of the malady. However, I must admit, that I have never observed the disease after the complete division of the vessel; nor is it possible to conceive how it could exist, when such a measure is adopted; but though this may be a certain preventive, still I conceive it to be a bad practice, inasmuch as other means prove effectual, and are unattended with the danger that sometimes follows complete division, when there is a tendency to the formation of anastomosing aneurism. In all these cases, after the performance of arteriotomy, I have observed that the wound healed, and that it was only after the aneurism had acquired some size, that the integuments ulcerated in the site of the cicatrix, giving rise to troublesome hemorrhage.\* At one period, I was in the habit of securing the trunk of the vessel; again, of dividing the aneurism through its centre, and tying the extremities of the vessel; but more lately I have been uniformly successful by making firm pressure with a coin wrapped in old linen, and secured with a firm roller. Indeed, these aneurisms are very common, and very easily cured.

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\* It is to be recollected, that in none of the cases of anastomosing aneurism which I have described, did the wound heal after the performance of arteriotomy.

The last form of the disease, which I shall describe, is aneurismal varix, of which I have seen but the following case:—

General D., *et.* 78, for some years had laboured under carcinomatous ulceration of his left eye and temple. In August, 1826, he had an apoplectic seizure, for which his right temporal artery was opened near the ear, and in consequence of the diseased condition of the left side of his head, pressure was not subsequently employed. He gradually recovered from the apoplexy, and in three months afterwards, I visited him with Mr. Sproute, surgeon of the Royal Engineers, who was his medical attendant, when we found that a regularly formed aneurismal varix was established in the situation where the artery had been opened; but as it was not troublesome, no curative means were had recourse to. In May, 1827, he died; when, on examination, I found that a large dilated vein lay over the artery, adhered to it, and communicated with it, by a small, but well defined and direct opening.

As I stated before, this is the only case of this form of disease that I have observed to follow arteriotomy; therefore my judgment on it cannot be very perfect. But from a consideration of the structure of the parts, I conceive it may be avoided; by not opening the artery close to the ear; as in this situation the vessel has frequently in front of it large and contorted veins; and the parotid fascia, ascending over the zygoma in this situation, will bear off the pressure applied to restrain the bleeding. For I believe these two circumstances favour the formation of a disease, which may be avoided, by opening the vessel where it is only covered by the skin, and comparatively unaccompanied by veins. In the above case no curative means were adopted; but if analogical reasoning be allowed, it is fair to suppose that pressure would have effected a cure.

From the Medico-Chirurgical Review.

#### CLINICAL REPORT FROM THE HOTEL DIEU. By M. BRESCHET.

##### *M. Dupuytren's Treatment of Ranula.*

The subject of this report is the affection called ranula, which is generally imagined to consist in an accumulation of saliva in the sublingual or sub-maxillary ducts, in consequence of their orifice having been choked up. The disease appears to have been tolerably well known to the ancient writers, at least Hippocrates and Celsus make mention of a tumour occurring under the tongue, though they entertained erroneous notions of its pathology. It has been observed by some writers that ranula is more frequent in infants than adults, but M. Breschet is of opinion, from several dissections, that common serous cysts beneath the tongues of children have often been mistaken for this particular disease. The viscosity of the saliva which has been considered as the cause of the disease, M. Breschet looks on merely as the consequence of the plugging up of the salivary canal, which may arise either from inflammation of the mucous mem-

brane, aphthæ, or ulcerations in the canal itself. In dividing the frænum linguæ, some of the excretory ducts which open on its sides may be wounded by the knife, and afterwards become obliterated by the cicatrix; small calculi will also occasionally form within the ducts and check the flow of the saliva, a circumstance which happened to the late Mr. Cline, and gave him considerable inconvenience. The amount of these concretions is at times considerable, and the fluid, instead of resembling saliva, may be puriform, or even entirely purulent.

The symptoms of ranula are easily distinguished by a surgeon at all cognizant of the affection. The tumour is soft, whitish, regularly round or oblong, situated just beneath the tongue, having neither pain nor redness, nor, in fact, any of the characters of inflammation; elastic, fluctuating, at first so small as scarcely to be felt, and slowly acquiring size. In general, its volume is little larger than a nut or pigeon's egg, but in some instances it far exceeds this, and M. Breschet remembers having seen a man in whom the tumour appeared beneath the chin, and stretched from that in front of the neck, almost to the sternum, entirely preventing circulation. In a case which happened to Le Clerc, the tumour filled the mouth, thrust forward the teeth, and formed a prominence externally as large as a duck's egg. It may perhaps admit of doubt whether these large tumours are really dilatations of the sub-maxillary or sub-lingual ducts, but, whatever they may be, they cause extreme distortion, doubling back the tongue, displacing or pushing out the teeth, altering the voice or preventing articulation altogether, and hindering suction in children, mastication and deglutition in adults.

The treatment resolves itself into palliative, and that which aims at a radical cure. The first consists in merely making and opening into the tumour within the mouth, and evacuating the fluid which it contains. Some have made their incision beneath the angle of the jaw, instead of within the mouth; but this plan is attended with danger of a salivary fistula. Le Clerc, however, operated in this manner with complete success; but probably his case was one of those where a large cyst exists filled with serum, instead of the true ranular tumour. When a simple puncture has been made the orifice quickly heals, the fluid again collects, and a fresh operation is required: indeed Petit relates an instance, where it was repeated ten times, and was ineffectual after all. Some have practised large incisions in the sac, others have cut out portions of it, and introduced into the opening tents of various kinds, portions of bogie, &c.; while Sabatier employed a canula, which he left until the edges of the opening had become completely callous; but all these plans have failed in other hands. A large portion of the walls of the tumour has been removed, but though this operation may delay cicatrization, it does no more—the wound ultimately heals, and the malady returns. It has been

proposed to extirpate the tumour, but unless the gland which fed it were extirpated too, the operation would in all probability be ineffectual, and it would be a bold attempt indeed to perform so severe an operation for so harmless a disease. If, as is supposed, the disease consists in an obliteration or obstruction of the salivary duct, then the employment of stimulant injections, as has been recommended, must be worse than useless, because they tend, of course, to increase the agglutination and obstruction. This is not the case when it is a common cyst, containing serum or lymph, for here the puncture of the tumour and employment of irritating injections would probably be of service.

The use of the actual cautery as a mean of opening the tumour, and preventing the opening from closing afterwards, is of very ancient date, as ancient indeed as the times of Hippocrates, who employed it himself. Acids have also been had recourse to, and Camper employed the lapis infernalis, although he confesses that it was frequently ineffectual; in short, all the methods which have been proposed or executed have been more or less unsuccessful in the long run. This want of success depends upon the tendency of the artificial opening, whether it be made by incision, or by the cautery, to contract and close; but M. Breschet tells us that Baron Dupuytren has at length discovered a method by which the closure of the wound is entirely prevented. Having made an opening into the tumour, the Baron takes a small instrument invented for the purpose, introduces it, and allows it to remain, just as the stilette is allowed to lie in the lachrymal canal, after the operation for fistula lachrymalis. This instrument appears from the description to be very similar in principle to that invented by Mr. Weiss for perforation of the soft palate. It may be made of silver, gold, or platina, and consists of a hollow cylinder, about three lines in length, and one, or one and a half in diameter, with a small, elliptical plate, convex externally, attached to either extremity of the tube. The use of these two small plates is to prevent the tube from slipping either *into* the dilated duct, or *out* of that into the mouth, for the opening having been made, and the tube introduced, one plate of course lies on the *inside* of the wall of the tumour, and the other on the *outside*, retaining the instrument in its position. If the tumour be of great size, or its walls much thickened, a free incision should be had recourse to before the application of the tube; in some cases it may be even necessary to cut out a portion, and allow the wound to nearly close prior to the introduction, but with these precautions our author affirms that M. Dupuytren has experienced complete success. Five cases are given in illustration, all of them occurring at the Hôtel Dieu, and therefore above suspicion of inaccuracy or colouring.

*Case 1.* Bruno Duchâteau, æt 24, ex-tambour of the garde-impériale, was admitted, October 14th, 1807, with a small oblong tu-

mour beneath the tongue, appearing to be a dilatation of the excretory duct of the sub-maxillary gland. Every kind of treatment had been adopted—incision—excision—cauterization, but all with the same success, or rather want of it. The Baron having opened the tumour with curved scissors, and given vent to a quantity of limpid, inodorous fluid, took up the little silver tube with a pair of forceps, and introduced it into the opening, so that one plate lay within the tumour, and the other on its outside, in the mouth. The disease very quickly disappeared, and in fifteen days the patient left the hospital, being able to eat, speak &c. with the most perfect ease.

In the second case, the tumour had existed for several years, was about the size of a pullet's egg, and was perfectly cured by the above means. In the third case, there were tumours, one on each side. M. Dupuytren, for experiment's sake, introduced the instrument into one, but contented himself with merely making an incision into the other. The former was cured, the latter not, when the Baron treated it in the same way, with the same success. The two other cases were equally fortunate, save that in the last a tumour re-appeared, almost on the site of the one which had been treated. It was found, however, to exist in the dilatation of another duct, and after being punctured was dispersed.

The "sub-lingual and sub-maxillary tissues" are subject to an inflammatory swelling, which may be mistaken by an unpractised surgeon for common ranula, but which requires a very different treatment. The tumour in this case appears suddenly, increases quickly, and is accompanied by tension, pain, and redness. The following is a case of this kind.

Mary Eugrot, æt. 21, having irregular menstruation, entered the Hôtel Dieu, May 20th, 1821, with a hard tumour, beneath the lower jaw, formed by the sub-maxillary gland. The enlargement had been present for upwards of six years, was about the size of a pigeon's egg, painful to the touch, and, on the least pressure, there was forced into the mouth a fluid, made up of mingled pus and saliva. On the other side, there had formed beneath the tongue, within the last six weeks, a hard elastic tumour, which prevented speech, and caused some difficulty of respiration and deglutition. Its redness, pain, and hardness, induced M. Dupuytren to think it depended upon inflammation of the sub-maxillary duct, and accordingly he applied leeches, emollients, and derivatives. In four-and-twenty hours there was a marked melioration, and at the expiration of a week she left the hospital, without either pain or tumour.

We have thus given an exposé of the practice of that distinguished surgeon, M. Dupuytren, in a very troublesome affection. Whether the plan he has recommended and adopted will be equally successful in other hands we shall not pretend to say; but, at any rate, it is worth a trial, particularly at our hospitals. By-the-bye, we think our English reporters somewhat behind their French *confrères* as

yet; at least as far as the *utile* is concerned. In the *dulce*, i. e. the transmutation of certain sheets of letter-press into the much abused and much loved "filthy lucre," we verily believe that our native "gentlemen of the press" have fairly won the day. The fact is this, that the French reporter, for the most part, takes up a certain point of practice, and illustrates it, as far as possible, by a variety of cases; whereas, the Englishman is too much in the habit of giving a solitary case or so, because it is "curious" or out of the way; in other words—good for nothing.

From the London Medical and Physical Journal.

**CASE OF ARTIFICIAL ANUS, arising from Ulceration of the Transverse Arch of the Colon; which, after discharging Feces for eighty-one days, spontaneously closed.** By EDWARD SWARBRECK HALL, Member of the Royal College of Surgeons, London; Surgeon to the South Dispensary, Liverpool.

The subject of this case was an active, volatile girl, eleven years old. My attendance upon her commenced on the 13th October, 1827. She had then been ill six days. Her indisposition came on with pains in the belly, vomiting, loss of appetite, thirst, and fever. At the time of my visit, she was suffering from the usual symptoms of peritonitis: diffused pain over the abdomen, increased upon pressure; moist, furred tongue; dry skin; pulse 120, and weak. She was also considerably emaciated. The application of a few leeches, together with fomentations and a little diaphoretic medicine, relieved her very much. Two days afterwards, the symptoms recurred with additional violence, but were easily subdued by a small general bleeding, a blister over the abdomen, and small doses of tincture of colchicum with sweet spirits of nitre. On the 19th instant, I ceased attending my patient, as she was well nigh recovered, and her parents were afraid of incurring much expense.

On the 29th instant, I was again sent for. I found ascites had taken place; some pain of the belly had returned, with palpitation of the heart, and a pulse up to 140, but unresisting. Local blood-letting was again resorted to, and the tinctures of colchicum and digitalis, in small doses gradually increased, prescribed. By the 12th of November, the dropsical symptoms had all disappeared. For some days antecedent to that date, the bowels had not performed their functions with regularity, which I attributed at that time to their having been the outlet by which the greater part of the dropsical effusion had been carried off; neither the kidneys nor skin acting with more than their usual energy. At times, even after the dropsy of the belly was dissipated, the dejections were so numerous that my patient was unable to rest at night. They were exceedingly offensive, of a whitish colour, and sometimes had very much the appearance of yeast. I have now little doubt that the mucous glands of the in-

testines were diseased; and probably the enlargement of the mesenteric glands, which was subsequently developed, arose from that source. The pulse was never below 120 for three months, and the tongue was more or less furred for the same period.

At this time (12th November,) I commenced the use of small doses of blue-pill, which was persisted in until my patient became convalescent; sometimes combining it with narcotics, and at others with gentle purgatives, such as rhubarb. In fact, my treatment throughout this complaint was entirely on the principles of one of my much respected teachers, Mr. Abernethy, a gentleman who will ever be remembered with gratitude and respect by those who have had the honour of being his pupils. "You have no right to dictate to nature," he would say: "soothe irritation, aid her in her efforts, but do not attempt to control her."

On the 30th of November, my attention was directed to a red, prominent, and painful swelling, of about the size of a pigeon's egg, immediately below the umbilicus. It resembled a furuncle in some measure, wanting, however, the characteristic hardness of a swelling of that nature, and having rather an elastic feel, as if distended with air. The whole belly likewise was somewhat tumid and elastic; the legs were slightly œdematous, the pulse considerably accelerated, and thirst very much complained of. I ordered the tumour to be poulticed, and two table-spoonfuls of a saline julep to be taken every two hours.

At my next visit, the tumour presented a very singular appearance: the superincumbent cuticle was raised from the cutis, like a blister, but was quite elastic, and evidently distended with air. I made a very small puncture in it with my lancet: a pretty smart report followed, and very fetid air, having the odour of the gas usually discharged per anum, escaped. After this had ceased to issue, a small quantity of fecal matter exuded from the aperture. From the appearance of the fecal matter, odour of the liberated gas, and situation of the opening, I had no doubt it communicated with the transverse arch of the colon. It is most probable that, during the early part of the complaint, the colon was united to the parietes of the abdomen by adhesive inflammation, and that then ulceration commenced in one of the diseased mucous glands, and gradually extended through the coats of the intestine and walls of the abdomen. Indeed, I remember that at one time, during the existence of the diffused pain over the abdomen, she complained of it being more severe in the neighbourhood of the umbilicus than any where else, which induced me to examine that region very attentively; but I did not discover any hardness or tumefaction on that occasion. At a later inspection (13th January) I could trace very distinctly an irregular induration in that situation, which I believed to be in the meso-colon.

Next day (December 2d,) I found my patient something easier: her tongue was less furred, pulse not so rapid, and thirst abated.

She had passed several evacuations per anum since my last visit. Upon removing the dressings from the belly, gas again escaped. I found the cuticle which had been raised entirely detached, and beneath it in the cutis two points of ulceration, not exceeding the magnitude of pins' heads: feces slowly issued through them.

From this time until the artificial anus got well, poultices were applied every two hours. Various tonics were prescribed during the further progress of the disease, particularly sulphate of quinine; at the same time continuing the use of the blue-pill. Generous diet was allowed her, and wine, ale, and porter freely administered. Of course, particular symptoms were prescribed for as they arose.

It would swell my communication too much to relate the daily changes that took place from this time forwards: suffice it to give a condensation of the most important ones.—The apertures in the abdomen slowly enlarged as the disease advanced, until they would admit a moderate sized goosequill, but never extended beyond that magnitude. The surrounding integuments always preserved a healthy aspect. The discharge from the artificial anus varied exceedingly: sometimes none would take place for several days, and at others it would be very profuse, probably a quart or more in twenty-four hours. Its consistence, odour, and appearance, were also subject to variation: on one occasion, when my little patient was so bad that I apprehended her almost immediate dissolution, the smell was distinctly gangrenous; at other times it had a musty, disagreeable odour; but the highly feculent smell was most prevalent. She was frequently troubled with colicky pains, but never to any great extent, and generally previous to a dejection per anum. The anal evacuations were as mutable as those from the artificial openings, and pretty generally in an inverse ratio: that is, when the one was profuse and watery, the other would be trifling and consistent. The pulse sometimes rose as high as 160 beats in a minute, but was always weak and unresisting; and, as I have before stated, was never below 120, until the 30th January, at which time some amendment of my patient's health began to take place. The anasarca of the extremities became so great, that I was afraid the integuments would have sloughed; and it was not until her recovery was considerably advanced that it began to diminish. She was often affected with hysterical symptoms, inability to retain the urine, cough, and difficulty of deglutition. For several days, she was reduced to such an excessive state of debility that she could not articulate or move herself in bed. Her appetite was at all times exceedingly capricious, being at one period voracious, at another she subsisted for days together merely on fluids. She was likewise very often subject to vomiting, and for many days nothing would remain on her stomach but a little cold ale.

On the 30th January, a decided melioration of her symptoms took place, and from that

date she continued to improve. On the 13th February she was able to leave her bed, to which she had been confined for the protracted period of three months. The artificial anus ceased discharging on the 19th of the same month, and in eight days no aperture whatever could be detected; a puckered depression, that would admit the point of the finger, existing in its site. Her appetite had lost its fickle character, and any kind of food agreed with her. The bowels had completely resumed their healthy functions, and the evacuations were perfectly natural. Up to the present time she has remained quite well, with the exception of occasional slight pains in the belly. Her abdomen is still rather protuberant, and irregular indurations, as if of enlarged mesenteric glands, can be detected. Should she not have a daily evacuation from the bowels, which she commonly has, she feels some uneasiness, and the site of the artificial anus, instead of being depressed, becomes pouting.

Ulceration of the walls of the intestines, followed by the effusion of their contents into the cavity of the abdomen, and thereby destroying the patient, is not an uncommon consequence of disease in the mucous membrane of the bowels: but I am not aware that a case similar to that which I have related has ever been recorded. There are two cases in Mr. Howship's work on the Intestines which bear some relation to it, but they are by no means parallel.

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From the Medico-Chirurgical Review.

#### MEDICAL STATISTICS.

Modern records and researches would seem to hold out some prospect of immortality to man, even in this world. At all events, we may fairly hope that the "march of intellect" will slacken the pace of TIME, or so shorten the length of his scythe, that he will not mow down one half the number which he has been wont to do. Whether this change may be attended by a proportionate increase of happiness among mankind, is another question, and one which we cannot pretend to solve. Dr. Bisset Hawkins will certainly contribute to lower the premiums on life insurance, in this country, though peradventure his *assurances* may ruin some of the numerous companies which must start on the prospect of the increasing longevity of mankind. It is not our intention, however, to enter on the laborious researches of the learned Doctor, as detailed in his recent Lectures, at the College of Physicians. We shall, in this place, glance at some medical statistics which have been laid before the Royal Academy of Medicine, by Dr. Vilermé, respecting the connexion of births and deaths with rigorous and mild seasons—scarcity of provisions—healthy or unhealthy places of residence—political events—manners—industry—in a word, the entire state of civilization. The author comes to the

following conclusions, as far as Paris is concerned. The actual annual mortality of its inhabitants is 1 in 32. In the 17th century it was 1 in 25 or 26. In the 14th century it was 1 in 16 or 17.

Formerly the deaths in Paris considerably exceeded the births:—now, the *latter* predominate. There is one birth annually among every collection of 29 or 30 inhabitants. The maximum of mortality, at present, is in the Spring:—formerly, it was in the Autumn, especially in the months of August and September. There are more male than female still-born children—and there is greater mortality, during the first three months (after birth) among the former than the latter. “The month of June shows the maximum of conceptions and the maximum of births.” It is difficult to account for this fact; for if most conceptions take place in June, most births should occur in March or April. Indeed, we imagine that *maximum* in the latter part of the sentence should be *minimum*: for immediately afterwards we have the following statement.—“Le mois de Mars et d’Avril sont ceux qui présentent le plus grand nombre de naissances.”

The opulent and the indigent classes of society present great differences in respect to relative mortality. Thus, in the richer quarters of the capital, there is one annual birth to 41 inhabitants. In the poorer quarters there is one birth to 29 or 30 of the population. In this calculation, the children of the poor, born in hospitals, are not taken into account. These would, of course, greatly increase the number of births among them. Nevertheless, it is found, that among the opulent classes of society, there are more children living under the age of five years, than among the indigent:—the inevitable conclusion is, that, although the poor people beget more children than the rich, they do not preserve so many.

Of 100 children abandoned (this must allude to asyla for the illegitimate) 60 perish within the first year. This ratio of mortality, however, among “*les enfants trouvés*,” is annually diminishing. Scarcity or famine greatly diminishes the number of births. The fecundity attendant on marriages has been regularly diminishing during the last hundred years. The reasons assigned by the author for this decrement, we shall give in his own words. “C’est aux froids calculs de la fortune, c’est aux prévoyances extrêmes suscitées par le goût du luxe ou par l’amour de l’aisance qu’il faut sans doute en attribuer la cause.”

The talented author passes in review, year by year, since 1680, the great moral, political, and physical events which have occurred in Paris, and their influence on the population. The general conclusion is:—“that, each time the people suffer, whatever may be the cause, the number of deaths is increased—the number of births is diminished, and the mean duration of life is curtailed. On the other hand, whenever the people are happy, the reverse of the above obtains.”

Not being acquainted with all the data on

which Dr. Hawkins finds his cheering prospects of increased health and longevity, we shall not attempt to invalidate his positions; but we shall take the liberty of commenting on a few points, which happen to come within the scope of our own observation. The Doctor appears to us, to attribute a great deal more to medical art in the preservation of life, than he is justified in doing. Thus, he says, that in fevers, if left to themselves, there would probably be about one death in two; while treated according to modern scientific principles, *six out of seven*, or even *eleven out of twelve* will be saved! We are extremely sceptical as to the soundness of this position—nay, we verily believe, that taking all circumstances into account, nearly as many fever patients would recover on *whey*, as on the most costly and polypharmic treatment that modern medicine could devise. The Doctor evidently looks to the favourable side of the question on all occasions. We shall only be able to glance at one or two illustrations which he brings forward. “To mark the improvement of health in our navy,” says he, “we may compare the fate of Commodore Anson’s crew, with a ship placed in *similar circumstances* about fifty years afterwards. Anson passed 143 days at sea, without touching at any place of refreshment. On his arrival at Juan Fernandez, half of his companions alone survived; and of the remaining 200, only 8 were efficient. But, in 1794, the *Suffolk*, a 74 gun-ship, during 162 days, had no communication with land, and arrived in India without the loss of even one man.” But surely Dr. Hawkins is not in earnest when he makes the Anson, struggling round Cape Horn, against adverse winds, and among dreary, barren, and dangerous rocks, where hope was extinguished and human strength exhausted—to be *similarly circumstanced* with the *Suffolk*, sailing on velvet, before the balmy trade winds to the land of pagodas, at the beginning of the war, in which every common sailor expected to make a fortune! Perhaps, in the whole annals of navigation, from the *Periplus* of Hanno, down to Parry’s “*Noctes Atticæ*,” among the ice-bergs, so remarkable a contrast could not be found as that presented by the *circumstances*, moral and physical, in which the crews of the Anson and *Suffolk* were placed. But, let us come down nine or ten years later, and see what happened. A frigate, just fitted out with a wretched heterogeneous crew, was despatched without an hour’s preparation, for the East Indies, in order to give notice of the new war which broke out in 1803. That ship went to the East Indies, without losing a man, without presenting a single symptom of scurvy, though the crew lived on salt-junk during the whole of the voyage. Here, says Dr. H. is a fine illustration of my position. Softly kind Sir! Two other ships were fitted out for the East India station, immediately after the one alluded to; but they were stored with lime juice, and every thing that could be supposed contributing to health on a long voyage. They sailed, and they arrived at Madras complete

lazarettos, the crews being eaten up with scurvy and scorbutic ulcers! In short, they were pretty much in the condition of the Anson's crew. How is this to be accounted for? Readily enough. The first ship that sailed was peculiarly fortunate. She took prizes in every parallel of latitude, and the crew were elated beyond measure at the success already obtained, and the prospect of its continuance. The other two ships sailed with similar hopes; but they were cruelly disappointed. Not a Frenchman ever crossed their track—not a shilling of prize money did they make! The consequence of these mental depressions were corporeal diseases.

Dr. B. seems to pity the Roman legions, who had only one medical officer to three or four thousand men. They were still worse off, we should imagine, at the siege of Troy. But when we reflect on the powers of dame Nature, and the hardy constitutions of those times, we doubt whether the paucity of Doctors made much difference in the general average of mortality. In ordinary circumstances—in accidents—and in inflammatory diseases, medicine is very powerful; but when the epidemic influence has once gone abroad in the atmosphere, small is the share which the most consummate medical skill has in checking the mortality.—The plains of Bengal, the islands of the West, the swamps of Walcheren—nay, the very rock of Gibraltar, which is quoted by Dr. Hawkins, afford melancholy proofs of the position here mentioned. The increase of longevity, then, we are inclined to attribute to moral and physical causes which are little under the control of medical art. Indeed, we are forced to agree with M. Vilerme, that—when people are *happy* they will be healthy and long-lived, and *vice versa*.

From the Medico-Chirurgical Review.

#### EXTENSIVE SPINAL DISEASE.

A young man, 26 years of age, born of phthisical parents, and who lost two sisters by pulmonary disease, took to the occupation of chimney-sweeper, at the age of twelve, and led a drunken and irregular life—sometimes sleeping on wet straw, in barns and out-houses, Summer and Winter. After a night spent in this way, he experienced some stiffness in the lower extremities, accompanied by weakness. These spread to the arms—and, in the course of a year he became affected with vertigo in the head, and tremor of the limbs. These incapacitated him for his occupation. One day he fell some yards down a chimney, and cut his head against the iron scraper; but the wound healed, without much difficulty. Nevertheless, the trembling, and weakness of the limbs, increased very much after this accident, and he was obliged to seek refuge in an hospital. When received, he presented the following symptoms:—tremor, and even convulsions, (on making any exertions) in the upper extremities—inability to

stand or walk, without stumbling—heat and sensibility of the surface natural—no pain in any part of the spinal column—constant sense of vertigo—no headach—chest well formed—respiration easy and deep, with the power of lying in any position—all the secretions and excretions natural—appetite good—intellectual faculties weak.

The disease was considered to be, effusion into the vertebral canal, with some affection also of the head. A seton was made in the nucha, and various remedies were prescribed, which we shall not here enumerate. Moxas were also applied to the spine. A long, and pretty severe course of tartar-emetic, however, nearly removed the tremors and vertigo. The patient had got to the quantity of 24 grains of tartrate of antimony in the 24 hours. But the patient did not gain strength, and he left the hospital uncured. In July, 1826, he had another fall down a chimney—bruised his chest—and spat blood, with cough, pain in the thorax, and other symptoms which led the medical attendants to suspect serious mischief. A violent fever supervened, and again he entered the hospital. On examination, no disease of the lungs could be ascertained, but the heart was found pulsating in the right side of the chest. The expectoration indicated approaching phthisis, and was mixed with blood. After various vacillations, and a variety of remedies directed towards symptoms as they arose, anasarca came on, and the miserable patient sank exhausted, on the 16th November, 1826.

*Dissection.*—There were several vesicles filled with fluid between the dura mater and pia mater—the latter membrane was every where thickened and opaque—substance of the anterior lobes of the brain remarkably dense—six ounces of clear water in the ventricles—no disease of cerebellum. The spinal marrow was examined with great care. Nothing wrong appeared in its coverings. When these were slit open in front, the spinal marrow bulged out in five different places, giving the appearance of five salient points composed of whiter substance than the rest of the cord. On examining the two superior projections, the structure of the cord was found disorganized, and changed into a substance resembling thick pus. Beneath the third prominence there was found imbedded in the spinal cord, a solid body, of a kidney shape, and extremely vascular. A similar substance was discovered in the fourth projection. Between these salient parts, the spinal marrow presented a natural appearance. The lungs were disorganized—partly tuberculous, partly suppurated. The heart and pericardium were unaltered.—*Jour. Complem. Feb. 1828.*

We shall not enter into the numerous remarks and reflections which the author has appended to this curious case. It is evident that great disorganization both of the brain and spinal marrow must have obtained, at the time when this miserable man was pursuing his avocation of chimney-sweeping, four or five months before his death!

From the Medico-Chirurgical Review.

ON NERVOUS AFFECTIONS OF THE  
HEART AND VESSELS. By the late  
M. LAENNEC.

[Forbes's new Translation.]

As it is our intention shortly to dedicate an article to organic diseases of the heart, and as that article must necessarily be a very extensive one, we take this opportunity of touching on what the illustrious pathologist above mentioned has denominated "NERVOUS AFFECTIONS" of the Central Organs of the Circulation and its great Outlets. M. Laennec justly observes, that the study of pathological anatomy has not been unattended with the disadvantage of blinding a considerable proportion of students and practitioners to every thing but organic lesions—to all affections of the nerves—to all changes in the fluids. "Nevertheless, (says he,) we are bound to admit, that every disease in which we can discover no constant lesion of the solids, nor evident alteration in the fluids, must consist in some disorder of the nervous influence." Of this class are several cardiac and arterial affections, which we are now to notice.

I. NEURALGIA OF THE HEART.

It is by no means uncommon to hear people complain of pains in the region of the heart, resembling rheumatic or neuralgic affections, and which are too frequently set down by inattentive practitioners as organic diseases.

"Sometimes these pains are confined to this spot, but frequently they extend at the same time, or vicariously, over a greater or less portion of the lungs and stomach. Sometimes they exist simultaneously in the superficial cervical plexus, and extend along the tract of the branches supplied by this to the anterior parts of the thorax; still more frequently, at the very time they are felt most severely in the heart, they shoot with corresponding violence along the nerves of the axillary plexus, and more particularly along the brachial nerve to the elbow, and sometimes as far as the fingers. When this is the case, the affection is confounded with a nervous disease which, during the last twenty years, has been the subject of much discussion, and seems to me only a variety of the neuralgia in question. This disease is the *angina pectoris*, which is very remarkable, and very distressing, when it exists in a high degree, but which is far from possessing the degree of severity attributed to it by many authors."

Laennec first describes what has been called *angina pectoris*, before he discusses the English pathology of the disease—namely, change of structure in the heart. The following concise description of this dreadful disease is deserving of record.

"The attack commences with a sense of pain, pressure or constriction in the cardiac region or at the end of the sternum. There is at the same time a numbness, occasionally attended with pain in the left arm; rarely in both arms or in one half the body; more rarely still in

the right arm only; and sometimes in all the limbs. The painful sensation is particularly felt on the inner side of the arm, as low as the elbow; and sometimes, as already mentioned, it shoots still further down. It is not unusual for the patient to suffer, at the same time, from pains over the fore part of the left chest; and in the female, these sometimes so affect the mamma that the slightest pressure becomes painful. Sometimes, particularly when the paroxysm is severe but short, the patient feels as if the same parts were pierced by iron nails or the claws of an animal. There are also pains in different points of the chest, dyspnoea (in extreme cases suffocative orthopnoea,) violent palpitations, congestion of blood in the head, and sometimes syncope or convulsions. When the attack is over, the patient merely retains a slight feeling of these various symptoms, particularly the numbness of the limbs, the left more especially."

It is well known, that Heberden and Parry attributed this peculiar disease to ossification of the coronary arteries—and this opinion has been embraced by several others. Nothing, however, can be more erroneous than this doctrine. Not one case in ten will be found to present this alteration—and, what is more, the symptoms of *angina pectoris* are seldom present in those cases where the ossification is found. The general belief in England, Italy, and Germany, is, that the said train of symptoms is dependent on some organic lesion of the heart—that the disease is almost always fatal. Laennec is of a different opinion.

"*Angina pectoris*, in a slight or middling degree, is extremely common, and exists very frequently in persons who have no organic affection of the heart or large vessels. I have known many individuals who had suffered a few very severe but short attacks of it, and had had no further return of it. I am even of opinion that the prevalent type of disease influences its development, as I have some years met with it frequently, and hardly at all in others. On the other hand, it is certainly true that this affection frequently coincides with organic diseases of the heart; but nothing proves even then that it depends upon such diseases, inasmuch as they are of various kinds, and as the *angina* exists without any of them. I have examined several subjects who had laboured under this disease, and in whom there coexisted either hypertrophy or dilatation of the heart; and in none of these did I find the coronary arteries ossified. One of these died suddenly during an attack of *angina*; and such a result need not surprise us, when so severe a nervous affection coexists (as in this case) with extensive hypertrophy. Dr. Desportes, in a dissertation published some years since, has stated opinions very analogous to mine, respecting the nature and seat of this affection: he considers its site to be in the pneumogastric nerve. I conceive that the site of the disorder may vary, according to circumstances. For instance, when there exists, at the same time, pain in the heart and lungs, we may presume that the affection is principally seated in

the pneumo-gastric; on the other hand, when there is simply a sense of stricture of the heart, without pulmonary pain or much difficulty of breathing, we may consider its site to be in the nervous filaments which the heart receives from the grand sympathetic. Other nerves are also simultaneously affected, either by sympathy or from direct anastomosis; for example, the branches of the brachial plexus, particularly the cubital, are almost always so; the anterior thoracic originating in the superficial cervical plexus, are also frequently affected; and this is also sometimes the case with the branches derived from the lumbar and sacral plexuses, as we find the thigh and leg now and then participating in the pain and numbness. I have even seen the affection confined to the right side of the thorax. In this case the pain and numbness extended to the arm, thigh, and spermatic cord of the same side, and the testicle became swollen during the paroxysms. There was scarcely any pain in the region of the heart; but the attacks were attended by severe palpitation, without any sign of organic lesion of the heart."

The character of the symptoms, M. Laennec thinks, confirms this opinion. We know that neuralgia of the most unequivocal kind, as sciatica and tic douloureux, give rise to the same variety and species of effects as angina does—namely, acute pain, painful torpor, simple numbness along the tract of nerve, and sometimes spasm of the parts to which the nerves are distributed.

It would be useless to discuss the various opinions on this disease, which have been broached by different writers, since the time of Parry and Fothergill. Dr. Forbes is inclined to agree with Hosack, that the disease "most frequently arises from a plethoric state of the blood-vessels—more especially from a disproportionate accumulation of blood in the heart and large vessels." Dr. F. observes also that, "in persons subject to this complaint, in whom no severe organic disease of the heart existed, he has generally found, by auscultation, that the organ was possessed of thin parietes and feeble powers." It would require a very long life, and a very extensive experience, to speak generally, and with much confidence, on the pathology of angina pectoris. Not more than three or four opportunities have occurred in our own practice, of examining, post mortem, those who have fallen victims to the disease. In only two, was there ossification of the coronary arteries, and, in these, there were other organic lesions. In all the cases, there was a flabby, soft state of the muscular structure of the organ, whether or not accompanied by much fat. But we have seen several cases, unaccompanied by dissection, where there were strong reasons to believe that the disease could not be fairly attributable to ossification of the coronary arteries—and we have found this state of vessels in several subjects, where there was no symptom of angina pectoris before death. The impression on our own minds is, that the nerves of the heart are implicated in the pathology of

the disease. The wasting and flabby structure of the organ are, in themselves, rather favourable to this doctrine. We see the muscles of a limb waste and become flaccid, where neuralgia, for example, sciatica, has long obtained. In short, wherever PAIN is a prominent symptom in any complaint, we have a fair right to conclude, that the nervous system of the organ is implicated in the pathology. That the symptoms included under the term angina pectoris may proceed from other causes than affection of the nerves, we will not deny—or at least that various organic derangements may be found after death; but, as the paroxysms come on like those of apoplexy, at various intervals, the organic change necessarily remaining the same, it is reasonable to infer, that the ostensible change of structure detected by the scalpel is rather the predisposing, than the direct occasional cause of the paroxysm. A determination of blood to the head, where there is disease of structure in the brain, will bring on the attack of apoplexy—and so a neuralgia may induce a paroxysm of angina pectoris, where there is already some defective structure in the part. The following therapeutical extract is rather curious.

"The means which I (Laennec) have found most successful in relieving neuralgia of the heart, whether existing in so violent a degree as to be named angina pectoris, or only under the form of slight pains confined to the heart, are those formerly mentioned in the case of neuralgia of the lungs, and especially of the magnet. This I use in the following manner: I apply two strongly-magnetized steel plates, of a line in thickness; of an oval shape, and bent so as to fit the part,—one to the left precordial region and the other exactly opposite to the back, in such a manner that the magnetic current shall traverse the affected part. This method is not infallible, any more than others employed in nervous cases; but I must say that it has succeeded better in my hands in the case of angina than any other, as well in relieving the paroxysm, as in keeping it off. Magnetism was, perhaps, too much cried up by some medical men in the last century; but I am of opinion that it is too much neglected at present. That it acts on the animal system, is sufficiently proved by the fact of its giving rise not only to very obvious general effects, but even to local ones. In the case in question, after a certain time it most commonly produces an eruption of small pimples, which are sometimes so painful as to oblige us to interrupt the process for some days. This effect cannot be attributed to the action of the oxidized plates on the skin, as the eruption almost always takes place under the anterior one: and I have observed similar results from plates applied over the abdomen and loins. By means of these plates (applied to the epigastrium and spine) I stopped, at once, a hiccup which had lasted three years. At the end of six months, the patient having one morning neglected to put on the plates, the hiccup returned; but was removed upon their being replaced. In another case in a patient affected

with imperfect paraplegia, without any sign of structural lesion of the spine, and for which moxa had been used without success, I inserted, to the depth of half an inch, a needle near the vertebral column and another into the thigh near the external popliteal nerve, and connected these by means of magnetized rods; and at the very instant of contact, there occurred an involuntary dejection, which had never previously happened to the patient. In the angina when the magnet gives but little relief simply, this is sometimes found to be increased on applying a small blister under the anterior plate. During the paroxysm, if the oppression is considerable, we must bleed the patient, if he is at all plethoric. Leeches applied to the epigastrium or cardiac region sometimes give more relief than venesection; but sometimes their application is impracticable from the extreme agitation of the patient. Derivatives are also beneficial, particularly sinapisms to the lower extremities and blisters to the fore part of the chest; as are also antispasmodic medicines, with the infusion of cherry laurel or digitalis, and also the fetid gums. A mild regimen, with the use of the tepid or cold bath, according to the season, are among the best means for preventing a return of the paroxysm."

As to the magnetic treatment, we cannot say any thing from personal observation. The management of the paroxysm must be very different from the treatment during the intervals. Those who have witnessed a severe attack of this terrible disease, can never forget it. The sufferings of the patient must be dreadful. The respiration is sometimes threatened, and the rattling in the throat induces us to draw blood in order to prevent immediate suffocation. In other instances, the breathing appears but little affected, and a cessation of the circulation seems impending, and we are forced to administer cordials. In almost all cases, anodynes and ether, with camphor, are necessary. In the intervals, quietude and temperance, with tranquillity of mind, would be the surest prophylactics—where, alas! are these to be found in this world? Those who have not real woes are tormented with imaginary ones—or, at least, woes of their own creating. Within these very few weeks we witnessed a most distressing case of this disease, which has made a strong impression on our minds, and harrowed up the recollection of several other instances of this deadly malady.

The patient (General B—) was on the borders of 80 years; but remarkably healthy, hale, and vigorous for that advanced age. He was of a very florid complexion, and plethoric constitution—had resided long in a tropical climate—and was addicted to the pleasures of the table, not amounting perhaps to what might be called intemperance. Up to within a very few weeks of his death (February, 1828) he took active and passive exercise in the open air, and could walk from his residence in Portland Place to the Royal Exchange, as quickly as most men of 50 years of age, though now 80.

He had undergone the operation of lithotomy some 20 years ago, under Sir Astley Cooper, and had no return of stone. He never made any complaint of heart affection, and had a strong, equal, and excellent pulse. His only complaint was a sense of occasional fulness about the head, for which he often resorted to cupping. He had also some slight dyspeptic symptoms, in the shape of acidity—depression of spirits—irritability of temper. A very few weeks before his death he complained of shortness of breath, and pain darting from the region of the heart down along the left arm, when ascending the stairs of his own house—and, latterly, when walking in the street, especially if he went against a current of wind. The writer of this article was consulted, and on strict examination, could detect no change of structure or irregularity of action in the heart. He readily recognized, however, that the patient had symptoms of angina pectoris, and stated this to his friends. He advised quietude, temperance, and abstinence from all active exercise. But neither the General nor his friends would be quiet—and therefore they summoned a celebrated surgeon, whose knowledge of *anatomy* must, of course, enable him to detect the most obscure diseases of internal parts. He came—saw disorder of the "digestive organs"—and prescribed blue pill at night, and black draught in the morning. Six days after this treatment had been put in force, the writer was summoned, in the middle of the night, to the patient, who was said to be dying. When he arrived, the General was labouring under one of the most terrific paroxysms of angina pectoris which he ever witnessed. The face was pale, the lips blue, the countenance indicative of unutterable anguish, the pulse scarcely perceptible, the breathing laborious, with what has been not inaptly termed, the "dead rattles" in the throat. The unfortunate sufferer was propped up in bed—tossing from side to side—praying for relief from the horrible pain in the region of the heart and left arm. It was evident that the lungs were gorged with blood, and a lancet was immediately pushed into a vein in the arm. At first a little black blood trickled out—then it came more freely—and, at last, in a stream. When twelve or fifteen ounces of blood were abstracted, the "dead rattles" ceased—the pulse rose—and relief was considerable, though by no means complete. Thirty drops of Battley's liquor opii sedativus, which happened to be in the house, were given—and, in an hour, the patient fell asleep. Next morning, all urgent symptoms were gone. He was ordered to keep his bed for two or three days—and then only sit up in his bedroom. No symptom of angina pectoris returned—and on the sixth or seventh day, the General would no longer submit to restraint. He came down stairs—dined in the parlour on fish—took his first glass of wine in good spirits—and, while drinking the second glass, he died as instantaneously, as if a cannon ball had passed through his chest!

God forbid that we should attribute any

part of this tragic finale to the treatment on which the patient was placed for disorder of the "DIGESTIVE ORGANS;" but we can only say that, if General B. died of this said disorder, we are totally ignorant of the nature of the complaint.

From the Transactions of the Medical and Chirurgical Society of London.

**CASE OF RUPTURE OF THE STOMACH PRODUCED BY VOMITING; WITH SOME OBSERVATIONS.** By J. N. WEEKES, Esq. Member of the Royal College of Surgeons, and House-Surgeon at St. Bartholomew's Hospital.

The following case of rupture of the stomach, accompanied with some unusual circumstances, having lately fallen under my observation, I have thought it may not be unworthy of the attention of this Society.

George Andover, æt. 34, had been liable for about two years to paroxysms of pain in the stomach. The pain usually continued for several hours, and generally went off with vomiting, and it returned at uncertain intervals, frequently of many weeks. Between the attacks the patient enjoyed tolerably good health. About Christmas last he vomited a large quantity of blood, which rendered him so feeble, that he was confined to his bed for five weeks. Since that time, his health has been much impaired, and the attacks of pain followed by vomiting have been more frequent.

On the evening of April 13th, he was brought to St. Bartholomew's Hospital, where I first saw him. He was then suffering great pain, extending from the epigastric region over the whole abdomen, and accompanied by nausea; there was neither tenderness nor tension of the abdomen,—the pulse was frequent, tongue clean. He had shortly before his admission drunk some shrub and water, to which he in great measure attributed these symptoms, and told me he had had a similar attack a week ago, after indulging in spirituous liquors, and that it went off with vomiting. On the following day the pain had subsided, there had been no vomiting, but he complained of nausea; the abdomen was distended by flatus, and he had frequent eructations,—the pulse was weak, tongue natural.

At eleven o'clock, P. M. he had a sudden attack of most severe pain. I was called to him about an hour afterwards, and found him groaning with agony at the pit of the stomach,—the abdominal muscles were hard and contracted,—the belly was neither painful nor tender on pressure,—his pulse was small and feeble,—he was extremely restless, and his countenance expressive of the greatest suffering. I instantly gave him sixty drops of tincture of opium, and as he found no relief they were repeated, but without benefit. He continued to suffer most acute pain for about two hours, when he was suddenly seized with violent vomiting. After this the pain somewhat

abated; there was no return of vomiting,—but he sunk rapidly, and died at four o'clock in the morning.

*Examination.*—On opening the abdomen, the stomach was observed to be flaccid and empty, and its contents, which consisted of a large quantity of dark-brown fluid, were effused into the peritoneal cavity, through a ragged opening situated on its anterior surface, and near the œsophageal orifice. The rupture extended from below the lesser arch of the stomach to near its cardiac extremity, and was about four inches in length. The three membranes were not torn equally, the rupture of the peritoneal extending an inch farther than that of the muscular or mucous coat. On the posterior surface of the stomach was a laceration, measuring three inches in length; and there were two or three small ones, from an inch to an inch and a half in length, at its great arch. These lacerations extended only through the peritoneal coat of the stomach, the muscular and mucous tunics remaining perfectly whole. The mucous membrane of the stomach was lined with a great deal of dark-coloured secretion, beneath which the membrane itself was of a deep red colour throughout,—its texture was softened and partially emphysematous. The stomach in other respects appeared healthy,—the liver was pale and softened,—the gall-bladder contained a calculus,—the structure of the spleen was unusually soft,—the other viscera were healthy.

In the eighth volume of the Society's Transactions, Dr. Crampton and Mr. Travers have described some cases of rupture of the stomach; in all these cases there had been ulceration of the coats of the stomach at the part which had given way.

Dr. Abercrombie, in an interesting paper in the Edinburgh Medical and Surgical Journal, 1824, has published some cases of this kind. Many of these cases differed in their previous history, but presented similar symptoms in the fatal attack. In some there had been occasional vomiting, gradual wasting and other symptoms indicating serious disease of the stomach; but in others the symptoms were slight and obscure, and the health was not obviously impaired. The fatal symptoms commenced with a sudden attack of most violent pain, referred to the epigastric region, and extending over the abdomen; sometimes, though not invariably, accompanied by vomiting. The abdomen was in some cases hard and contracted, in others distended and tender. The pain continued unabated, with rapid sinking of the vital powers, and death took place in a few hours. The stomach was found penetrated by ulcerations of various extent, articles of food and drink were found in the abdomen, and in some cases peritoneal inflammation had supervened. The stomach generally presented marks of long standing disease, as induration and thickening of its coats, adhesion to the neighbouring parts, and organic disease at the part which had given way.

The most remarkable feature in the pre-

ceding case, is the extensive rupture of the stomach, with so little disease of its coats; and in this respect it forms a striking difference to those cases hitherto related. The stomach presented no thickening nor ulceration at the part which was ruptured; the disease was confined to its mucous tunic, and appeared to be recent inflammation and softening of its texture. It may also be remarked, that the symptoms in this case were not such as generally indicate the existence of organic disease; there were considerable intermissions of the symptoms, the patient had enjoyed tolerably good health, and there was no emaciation.

In the lacerations of the peritoneal coat of the stomach, without including the other tunics, this case forms a striking resemblance to some appearances which the uterus presented, in a case of sudden death during parturition, described by Mr. C. M. Clarke, in the third volume of the Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge. "In the fold of the peritoneum, which dips down into the pelvis between the uterus and the rectum, I observed about an ounce of blood; and upon that part of the peritoneum which covers the posterior surface of the uterus, there were between forty and fifty transverse lacerations, none of which were in depth above one-twentieth of an inch, and many were merely fissures in the membrane itself. They varied much in length, some measuring one inch, some two inches, whilst the length of others did not exceed the fourth part of an inch. The space upon which they were situated, extended from one side of the uterus to the other, and occupied the greater part of its whole posterior surface. The edges of these lacerations were thinly covered with flakes of coagulated blood, and there could be no doubt that the blood found in the fold of the peritoneum, had escaped from the lacerations. The muscular part of the uterus was perfectly whole."

The only case I have met with of rupture of the coats of the stomach produced by an act of vomiting, or rather, attempting to vomit, is recorded by Lallemand, and is described in the forty-ninth volume of the Dictionnaire des Sciences Médicales, Art. Rupture.

The patient had laboured under difficult digestion for five or six months, and had been much relieved by observing a strict regimen. After indulging her appetite to a greater extent than usual, she was attacked with uneasy feelings in the stomach, accompanied by nausea and inclination to vomit. She made violent but ineffectual efforts to discharge the contents of the stomach, and whilst suffering great agony, experienced intense pain, with a sense of tearing at the lower part of the belly; she uttered several screams, and fell down insensible. She sunk rapidly, and died in the night. On dissection, the cavity of the peritoneum was found full of half-digested food; the anterior and middle part of the stomach was torn obliquely from its small towards its great curvature, to the extent of five inches.

The edges of the rupture were thin, irregular, and presented no marks of disease. The three coats of the stomach were not torn to an equal extent, nor exactly in the same direction; the rupture of the peritoneal was larger than the muscular coat, and the mucous membrane was the least extensively lacerated. A mass of scirrhus, an inch and a half in extent, surrounded the pylorus. The other parts of the stomach were perfectly healthy.

From the Edinburgh Medical and Surgical Journal.

**CASE OF MOVEABLE CARTILAGES IN THE BURSA OF THE SARTORIUS.** By JAMES SYME, Esq. Lecturer on Surgery, &c.

I was lately requested by my friend Dr. Scott, to examine the knee of Master T. A., a stout very well formed boy, 15 years old.

The left tibia was considerably enlarged at its upper and inner part, over which the tendons of the flexor muscles pass to their insertion. This exostosis did not incommode the patient; but he complained of two moveable bodies, which shifted about under the tendons, and occasionally impeded the use of the limb. He stated that the enlargement of the bone had existed for several years, at least three, but that he had not noticed any mobility of the swelling until last winter.

In consideration of the inconvenience which the patient actually suffered, and the risk of farther mischief if the irritation were allowed to continue, we agreed that the bodies in question ought to be removed, especially as this could be done without any fear of those bad consequences which attend similar operations, when the large joints are concerned. I therefore desired the patient to bring them into the most favourable position for the purpose, which he did by completely bending the knee, and then pressing them from below upwards, so as to make them pass under the tendons and project distinctly on the fore part of the tibia, a little below the tuberosity. Having fixed them in this situation, and at the same time drawn the skin as much laterally as possible, I cut down upon the larger of the two, endeavouring to divide at once all the intervening parts, which was somewhat difficult, owing to unusual thickness of the fibrous textures. The pressure being continued, forced it to escape through the opening, with a little synovial fluid, and the smaller one immediately followed. The lips of the wound were retained in contact by the twisted suture, and healed without any trouble.

The larger of the two substances removed was about the size of a common marble, the other was smaller by two-thirds. Their surface was smooth, and possessed a pearly lustre. Their shape was approaching to round, but irregularly nodulated, so that they had a striking resemblance to the roots of the *Bunium bulbocastanum* (Pig-nut.) They had a tough closely adhering capsule, a fibro-cartilaginous consistence, and a bony nucleus.

I am inclined to publish this case by the

hope that it may assist to throw light on one of the most obscure subjects in surgical pathology, viz. the formation of moveable cartilages.

It seems to me particularly deserving of attention, that the exostosis existed long before the cartilages; which must therefore be regarded rather the effect than the cause of this effusion of the bone. If the very distinct account given by the patient could leave any doubts as to this point, they would, I think, be removed by the present state of the other knee, where a considerable exostosis can be felt at the insertion of the semi-membranous—though there is no vestige of a moveable cartilage.

Sir A. Cooper has observed, that exostosis occurs most frequently at the attachments of tendons, and attributes this to the irritation resulting from inordinate contraction of the muscles. This opinion derives support from the well-known anatomical fact, that the processes of bones always increase in strength along with the muscles attached to them. Another argument may be drawn from several cases which have come under my care of exfoliations from the ischium and pubes at the origin of the adductors of the thigh and flexors of the knee. I may take as a specimen of these, that of a herring-curer, who seven years ago worked a whole day, according to the custom of his employment, with his feet widely separated, alternately stretching himself to the utmost, and then stooping with his knees straight, a position which of course caused a violent extension of the flexors of the knee, viz. the *Biceps*, *Semi-tendinosus* and *Semi-membranosus*. In the evening he complained of an uneasy feeling of fatigue in the right hip. This gradually passed into pain, and after a time was succeeded by the formation of an abscess, which was treated as a *fistula in ano*. Many abscesses formed subsequently, and for the most part prevented him from following his employment. Some small pieces of bone were from time to time discharged; but his surgeons having contented themselves with simply extracting them as they appeared, never struck at the root of the evil. When he applied to me there was a large abscess in the upper and back part of the thigh, and an opening just below the edge of the *gluteus maximus*, at the bottom of which I felt a piece of loose bone. Having evacuated the abscess I introduced a long straight bistoury into the sinus, and dilated it so as to admit my finger to the bone and ascertain that it lay between the flexor tendons. I enlarged the orifice of its containing cavity and then extracted it. It was about the size of half a sixpence. The patient very soon regained the use of his limb, and also his general good health. But some months afterwards he told me that the sinus still discharged a drop or two of matter, and that he occasionally felt a pricking pain in the hip. I probed the sinus and felt a small bit of bone, which was removed as the former one, but with rather more difficulty, owing to the small size and very firm margin of the opening between the tendons. After this the wound

healed directly, and the patient has continued perfectly well ever since.

From the Edinburgh Medical and Surgical Journal.

DELINEATIONS OF THE ORIGIN AND PROGRESS OF THE VARIOUS CHANGES OF STRUCTURE WHICH OCCUR IN MAN AND SOME OF THE INFERIOR ANIMALS; *being the continuation of Works already published on this subject.* By JOHN BARON, M. D., F. R. S., Physician to the General Infirmary, and Consulting Physician to the Lunatic Asylum at Gloucester, &c. London, 1828. 4to. pp. 56. Four Coloured Engravings.

To all those who take interest in the progress of the pathological inquiry, the hypothesis which Dr. Baron some years ago proposed to explain the origin of tubercles in the tissues of the animal body must be well known. That hypothesis, it may be remembered, is distinguished by assuming the facts, that tubercles are at commencement small vesicular bodies with fluid and transparent contents, and that these vesicular bodies are the same as the parasitical animal known under the name of hydatid. We say "assuming the facts;" for we never could perceive that the ingenious author had succeeded in demonstrating that tubercles were vesicular at any period of their growth; much less could we admit that these vesicles were of the same nature with the hydatid. Bayle indeed described the miliary tubercle as translucent or semi-translucent; and Laennec showed that this is merely the incipient or nascent state of tubercles which are afterwards found to become opaque and grayish. But in no case were their contents said to be fluid; and in no respect could they be compared to the hydatid.

In the lungs of the sheep and other animals, in like manner, we have more than once seen minute granular bodies, opaline and semi-translucent, but hard and cartilaginous in consistence. What these bodies, so minute, so hard, and so numerous, might have become, we do not feel competent to say. But neither from their present characters, nor from any other circumstance, did it seem probable that they had been or were to terminate in hydatid cysts or vesicles. In short, whatever be the ingenuity with which Dr. Baron studied to uphold his opinions and persuade his readers of their truth, nothing like accurate evidence was adduced; and they continued in the shape of an unsupported hypothesis, not requiring refutation, because never founded on the basis of fact; and never generally admitted, because in direct opposition to any accurate knowledge on the development and growth of tubercular disorganization.

Of the weakness of his hypothesis Dr. Baron appears himself not to be unaware. He nowhere positively states that he has traced the transformation from the vesicular or hydatidiform condition to the opaque, firm, and tubercular structure in the tissue of the lungs;

and it is only by applying to these organs, what he recognises in the liver, that he ascribes to this source the formation of tubercles in the human lungs. The evidence which Dr. Baron adduced in favour of his opinions was derived almost exclusively from the liver of the sheep, the rabbit, and others of the inferior animals; and whatever be the accuracy with which he describes the appearance of these growths, the changes which they undergo, and the effects which they produce on the health of the animals, they admit of little direct or useful application to the development and growth of tubercles in the lungs, either of the human subject or of the lower animals.

In the present work, which forms a sort of practical illustration of the two preceding ones, Dr. Baron undertakes to illustrate still more fully and forcibly his peculiar opinions on the generation of tubercles in the textures of the animal body. We do not perceive, however, that the application of that part of his theory, which pertains to the growth of hydatids of the liver, is in the smallest degree more immediate than before; or that Dr. Baron has succeeded in making any decisive progress in establishing the general accuracy of his opinions.

Premising that the delicate texture of the lungs presents greater difficulty to the distinct investigation of the morbid changes which take place in their substance than the liver, in which the slightest change, he contends, is easily distinguished; and maintaining also, that it is an organ more frequently diseased, he adopts it as the source of the illustrations now to be adduced. Notwithstanding this admission, he denies that the facts thus established are applicable to this organ only; and he reminds the reader, that he does not relinquish those views which induced him to maintain that the description which is accurate in relation to one class of disorganizations is equally applicable, *mutatis mutandis*, to others.

The first traces of tubercular deposition in the liver become visible, according to Dr. Baron, by two circumstances,—the unusual enlargement of the lymphatics, and the formation of minute vesicles,—generally conjointly, sometimes separately.

In a liver becoming diseased, the first thing which meets the eye is a manifest enlargement of the lymphatics, which occupy a larger proportion of the liver than natural, and which, though still transparent, soon become opaque white, and pearly coloured. A section of the organ at this period gives a mottled appearance, not unlike that of the section of a nutmeg, the ramifications of the lymphatics being opaque blue over the brown ground of the hepatic substance. As the disease advances, a great disproportion takes place between the new white deposit and the sound parts. The former greatly predominates; and ultimately the brown remains of the glandular texture of the liver are entirely removed, and an opaque whitish hard mass, which Dr. Baron terms

scirrhus, is left in its place. This is the course in the first kind of change.

The second is represented by Dr. Baron as originating in two modes; one consisting in the expansion of the point of intersection where different branches meet; the other in the formation of a vesicle in the course of a lymphatic trunk,—in other words, the expansion of a trunk into a vesicle. The vesicle thus developed in either situation is at first small, but gradually enlarges.

“As it increases in size in every direction, a small portion of its circumference is seen like a transparent speck around the dark surrounding texture. This part becomes more visible as the body grows; and as its magnitude increases, the manner in which it dispossesses the true texture of the part where it is formed is beautifully seen. In the liver, where the glandular texture is divided and subdivided, these partitions may be perceived gradually expanding like a thin film or curtain over the increasing growth of the vesicle. After a time they are completely absorbed, and the boundaries of the disorganization become clearly defined.”

Of these two modes of disorganization described by Dr. Baron, the difference is that the former, that of the lymphatics, leads to a dense scirrhus texture without appearance of tuberculous disorganization; while the latter, that commencing by vesicles, forms circumscribed tubercles or tumours. When the two species of morbid action are combined, which is not uncommon, a compound structure partaking of the characters of both, is the result. The general accuracy of these statements is illustrated by the first, second, fourth, and fifth figures of the first engraving, as seen in the liver of the hog, and in the second, as seen in the human liver, and in the first figure of the second engraving as seen in the liver of the sheep. In the second figure of the same is seen this change of structure in the lung of the sheep, and in the third, in that of the horse. The description of the state of the lungs in the latter deserves particular attention.

“On examining the lungs (of a glandered horse) their healthy texture seemed at first sight but little changed. They retained their purplish hue, and their light elastic feel. On closer examination, a number of circular vesicles were seen; some being transparent, and others of an opaque yellow colour. Others had an oblong figure, and seemed clearly to have been formed in the course of transparent vessels, which, from their character and appearance, I took to be lymphatics. I endeavoured to ascertain this point by attempting to inject them with quicksilver: but I failed in my design.” In this account an omission, not altogether unimportant, is committed, in not stating whether the vesicles contained fluid, and in not ascertaining whether they were not mere ruptured or enlarged cells,—a change of structure not uncommon in the lungs of the glandered horse.

With the view of illustrating more forcibly

the formation of tubercles, Dr. Baron narrates the particulars of an experimental investigation which he performed on a family of young rabbits, under the influence of confinement and improper food. These animals when first shut up, about the end of April 1825, were perfectly healthy in appearance. One died on the 3d June: and on the external surface of the liver were found many transparent vesicles about the size of a pin's head, with distinct enlargement of the superficial and deep-seated lymphatics. On the 12th, died another rabbit, in which the liver was light coloured, enlarged, and studded with tubercles semitransparent and opaque, firm and yellow, with similar disease of the lymphatics. In a third rabbit, which died on the 16th, the morbid process was still farther advanced. One lobe was universally occupied by miliary tubercles, projecting from the outer surface, so as to give it a granular appearance, and so numerous as to disguise the hepatic structure in an irregular straw-coloured mass. To the lower surface of the liver was attached an hydatid about the size of a hazel-nut. In a fourth rabbit, which died on the 18th, the liver contained many large tubercles, though fewer than in the last case, and more detached, the interstitial substance being sound. To the lower surface of the organ were attached several hydatids not quite globular, becoming opaque, and with contents like calves' foot jelly.

At this period Dr. Baron removed from the place in which their companions died, three survivors, with all the external signs of labouring under the same disease which had already been the cause of the mortality. They were emaciated, with tumid belly, and scaly, harsh, unhealthy skin. Their new abode was kept dry and clean; and they were fed chiefly on bran and oats, with a moderate proportion of clover and dandelion.

"About ten days after the change had taken place, I killed one of them. The animal had increased a little in flesh, but the other external signs of disease existed, and the progress of the internal disorganization had not been arrested. The liver was very much enlarged, and there was a greater development of the whole of the lymphatic system of the organ than in any other of the former instances. There was also a very great number of tubercles of different sizes and different stages of their progress, so that scarcely any part of the liver remained in a sound state. In Plate III. Fig. 2, will be found a representation of this diseased part. I present it to the reader, to show how far change may take place, and yet the healthy condition of the part be restored. In order to put this point to the test, I killed the fellow of this last mentioned rabbit on the 5th August. It had been fed exactly as I have already described. It was very much improved in health; and on examining the liver it was found comparatively in a sound state. Its colour was natural; it was not much enlarged; and there were only a very few tubercles discernible in it.

"So far as the foregoing facts lead us, they

are interesting both in illustrating the origin and progress of the disease in question, and in giving us some hints respecting their treatment. The evidence that all the rabbits were diseased, is as strong almost as it can be. They were all affected with the same external signs; and till their diet was changed were all falling under the influence of the disorder. The lives of the three that remained were obviously saved by this change. In the first that was killed after it had taken place, the health was improved, but the local disease had not been arrested. In the second, not only was the general health improved, but there was also great reason to believe that a considerable removal of the external disease had been effected.

"I trust it will not be thought that this inference is overstrained; the traces of the disorganization of the liver being discernible, and all the other appearances of the animal denoting that it was diseased like its companions."

These morbid changes, which Dr. Baron afterwards traced in the lungs of the sheep, those of the horse, and in the liver of the hog, he ascribes originally to the agency of the lymphatics, which he maintains may be seen effecting in the bodies of the lower animals that transformation, the result of which only can be recognised in the human subject. Having established this point in relation to the liver in these animals, he applies it to the lungs of the human subject, and adduces in confirmation the case of a man cut off under diabetic symptoms, in whose organs tubercles were found extensively in various stages of development. "The tubercles varied in size as well as in condition; some were perfectly transparent and not larger than the head of a pin; others were consolidated and about as large as a garden-pea. In the lungs, liver, and spleen, irregularly shaped scirrhous masses were formed by the aggregation of minute tubercles and by the enlargement of the lymphatics of the organ."

A still stronger confirmation of his opinions Dr. Baron endeavours to derive from the successive changes which take place in the progress of ovarian disease, two cases of which he narrates. In these, indeed, some of the vesicles attached to the Fallopian tube were transparent, and others opaque, or approaching to opaque and solid. In this it is well known there is nothing uncommon. The application of the fact is, however, both different from what is commonly received, and perhaps may in some cases be the true one. The following statement may be given as the general inference which Dr. Baron derives from his several facts and arguments:—

"In the lungs, the primary condition of the disorganizations is not less illustrative of the above description. The clear and untransformed vesicles may be seen here and there interspersed through its substance, or studded over its surface. In this state, unless they happen to be formed in great numbers, and at no great distance from each other, they cause but little interruption to the function

of the part; but things do not remain long in this condition. A change, both in the diseased structure and the portion of lung connected with it, soon takes place; and, subsequently, all those progressive alterations occur which I have elsewhere endeavoured to describe."

Very much in the same manner he represents the occurrence of scirrhus degeneration to take place. One of the simplest varieties of this kind of morbid structure he describes as taking place in the following manner.

"The lymphatics of the first part show signs of disease. They enlarge and become opaque. The organ in which the disease occurs, if examined at the time, will exhibit a reticulated appearance. The size of the interstices depends of course upon the original distribution of the lymphatics. But as the disease advances, there is a corresponding diminution of the interstitial matter, arising from the approximation of the morbid parts. Ultimately they seem to coalesce, and thereby to form the dense, firm organization called scirrhus. This is perhaps the simplest form of the disease. One of the most obvious and common modifications of its character arises from the development of tubercles in connexion with the changes described."

In these views we willingly admit, as we have before done, there is much ingenuity. They tend to simplify, in a remarkable degree, whatever ideas we have entertained on the formation and progress of morbid structure. They enable the pathologist to form a distinct and tangible idea of that which was formerly confused, vague, and impalpable. They present him, in short, a sure resting-place, on which he may fix his wandering thoughts in an inquiry arduous, complicated, and obscure; and they encourage him to think that he at length sees clearly explained what was before dark, confused, and unintelligible. More than this, however, they do not accomplish. The premises, when carefully traced, do not legitimately and directly lead to the conclusion. When divested of every bias, and of all unnecessary circumstances, we examine the relation between the vesicular or hydatid bodies found in the livers of the lower animals, and the tubercles which in the lung of the human subject constitute the pathological cause of consumption, it is impossible to perceive that the relation is that of sequence, or of cause and effect. Conceding to Dr. Baron for the sake of hypothesis the assumption, that what is true of the former must be true of all other forms of disorganization, which, nevertheless, is a principle greatly too general for the present state of science, it is impossible to allow that the application holds good in the case of pulmonary tubercle.

On a former occasion we took the liberty of adducing such facts and arguments as the question then seemed to require, in order to place its actual merits in the true light.\* With

this subject, therefore, it is unnecessary again to occupy the time of the reader. But it is surely manifest, from the fair account which it has been our study to give of the inquiries and arguments of Dr. Baron, that between the fact with which he sets out generally,—the conversion of the vesicular cyst of a sheep's liver into a purulent one, or afterwards a solid body, and the conclusion at which he arrives, that the pulmonary tubercle is originally a vesicular cyst of the same kind,—a chasm abrupt, immense, and impassable is left. Even the circumstance of tubercles being found in their origin transparent, a point never doubted, and explained, as we conceive, by Laennec, is not applicable to the argument. These bodies, instead of being vesicular and containing fluid at this period, are solid, firm, and cartilaginous in all the instances hitherto observed accurately. Nor has Dr. Baron yet adduced positive or satisfactory evidence to the contrary.

The train of argument employed by Dr. Baron is further corrupted by another fallacy, in the circumstance of regarding the hydatid as susceptible of the changes which he describes. If the cysts which Dr. Baron finds in the liver of the rabbit, the sheep, and the hog, be really hydatids, that is, animals propagated according to a certain law, so far as we are at present aware, no well authenticated facts lead to the inference, that these fluid contents undergo the changes which he represents to take place.

On the influence which Dr. Baron ascribes to the agency of the lymphatics in effecting these changes, we do not feel competent to offer any opinion. In its present state the matter rests very much on his testimony as an accurate observer. To every one, however, who knows anything of the nature of evidence required in support of pathological opinions such as the present, it is obvious that nothing short of injection should satisfy us as to the fact, that the morbid change actually originates in that system of vessels. The reasonableness of this condition we trust Dr. Baron will not himself dispute; and if he establishes this point it certainly must go so far in demonstrating the truth of one part of his hypothesis.

The objections now adduced apply solely to the inferences which Dr. Baron causes to flow from the facts which he advances. The latter possess much actual merit, whatever be the ultimate fate of the hypothetical opinions which they are made to support. To this merit we have more pleasure in bearing testimony, in so far as we feel ourselves compelled to withhold our assent from the conclusions derived by the author. Had he divested his mind of the hypothetical prepossessions by which it has been biassed,—had he confined his inquiries to the simple phenomena observed in the rise and progress of the organic changes which form the subject of investigation, Dr. Baron would have formed a more valuable accession to the stock of facts accumulated on this head than his work in its

\* Medical and Surgical Journal, Vol. xix. pp. 298, 299, &c.

present form does. With this deduction even, it cannot be doubted that the facts which he establishes throw some interesting illustrations on the effects which diet and regimen exercise on the health of animals, and on the intimate atomic structure of their organs.

From the London Medical and Surgical Journal.

# PRACTICAL OBSERVATIONS ON THE USE OF THE COLCHICUM AUTUMNALE.

The following communication was sent to us by a very intelligent and diligent medical practitioner. As the remarks are the result of the personal observation of a gentleman of high respectability, who has been upwards of twenty years actively engaged in the profession, they form a valuable testimony in favour of this potent remedy, and cannot fail to be acceptable to our readers:—

I will endeavour to answer your queries, respecting the use of colchicum; and whatever I say will be from my own experience and observation; but you must bear in mind that I am writing about a favourite remedy.

1st. What is the best form of administration?

The form which I have used for some years past is a tincture, in the proportion of ℥iij. of the dried seeds (not bruised) to ℔j. of proof spirit; digest for fourteen days and strain. In this preparation of colchicum, I feel the most confidence.

2d.—To what extent have you ever carried it in a constitution of ordinary powers?

Fifty minims of the above tincture every four hours, and that continued for three days. My general dose is gutt. xxx. every five hours.

3d. Have you ever depended upon it solely in cases of severe inflammation of any important viscus or tissue?

Yes—I have administered it in many cases of severe pleuritis, and it generally succeeds in removing the disease without any other aid. Should the symptoms run very high, I have recourse to one large bleeding. In other cases, when there is pain remaining after the acute symptoms are over, I apply a blister over the part affected; but either bleeding or blistering is so seldom necessary, that I should find some difficulty in calling to mind a case where I have thought it necessary to employ them. If success in practice be any criterion of the efficacy of remedies, my plan may lay some claim to notice, for I have not lost a single patient from pleuritis for many years past, and, in a practice averaging nearly 3,000 cases annually, including all kinds, there must be many of that disease.

4th. What cases is it most suited to?

Pleuritis, particularly of the sub-acute form—acute rheumatism—gout—in the early stage of inflammatory fever, and in the commencement of inflammatory diseases in general, with the exception of gastritis and enteritis.

5th and 6th. What are the chief cautions required in its use, as respects the patient, or in its action? and what are the symptoms of an over dose, and how are they to be treated?

The unpleasant effects which occasionally follow the exhibition of colchicum, are, nausea and purging, with a feeling of general debility or sinking which the patient complains of, accompanied by a peculiar, moist, whitish fur upon the tongue, which I can scarcely describe; and a disagreeable taste in the mouth, also peculiar.

I have never carried its exhibition further than the production of these symptoms; when they arise, I direct the patient to abstain from the medicine until they have subsided, and then, if the disease be not removed, its use is resumed cautiously, and in smaller doses. During the presence of the above named symptoms, the disease, for which the colchicum was given, generally gives way, and I have never had occasion to have recourse to antidotes. After its use, tonics are very grateful to the patient.

Colchicum produces very different effects in different constitutions; in some it purges severely; in others, the bowels become confined during its use. Our friend R., had acute rheumatism affecting the diaphragm. I ordered him gutt. xxx. of the tincture every five hours, in a little water; after three doses the pain abated materially; three doses more produced nausea, purging, and the other etceteras which I have mentioned; the medicine was left off for a time. When the effects had disappeared, a little uneasiness and pain being still felt, a few doses more, given cautiously at longer intervals, accomplished his cure. At the same time, a female patient was suffering under a severe attack of lumbago; she took the tincture in the same doses and intervals; the pain was relieved by a few doses, and she got well; but in this instance the bowels became confined under its use, and it was found necessary to administer a purgative to procure alvine evacuations. In both the above instances, at the time the patients commenced taking the medicine, the bowels were regular.

As a general rule, I wish to have the bowels acting freely during the exhibition of colchicum, and if the remedy be not sufficient in itself to produce that effect, I unite it with the sulphate of magnesia.

T. G.

From the London Medical Gazette.

## CYSTS WITH OSSEOUS PARIETES, DEVELOPED IN THE SUBSTANCE OF BONES.

M. Dupuytren has more than once called our attention to these tumours, which he was the first to describe accurately. If, he observes, fibro-cellular tumours often develop themselves in the substance of the soft parts, and more especially in the uterus, similar tumours may also be found occasionally in the substance of bones. A contusion, or some other accident, may form the germ of such affections, and when once they have commenced, their increase is easily to be conceived. They are united to the neighbouring parts by a pedicle, which transmits their nourishment and life; and

their growth is at once the consequence of this connexion, and the cause of the separation of the osseous laminae. But, although it is easy to conceive that a solid matter interposed between the bony cells may, by its progressive growth, separate and distend them to a considerable extent, it is more difficult to comprehend how such cavities can be developed, containing only a fluid, and how this fluid can act with so much power as to separate and distend the cellular structure of a bone: nevertheless, the fact is so. M. Dupuytren has observed several examples of this kind of tumour, either in the extremities of the long bones, in the bodies of the vertebrae, or still more frequently in the bones of the face, in the upper or lower jaw.

These cysts contain either a solid or a fluid matter. The following case is a remarkable instance of the former.

**CASE I.**—It is now about twenty years ago that a young man presented himself at the Hotel Dieu, on account of a large tumour which swelled up his cheek, and occupied the right horizontal portion of the lower maxillary bone. This young man had been destined for the church, but had been refused admittance into the seminary, in consequence of the above tumour. M. Dupuytren examined it with attention, and was convinced that it was seated in the bone itself. When pressure was made upon the parietes of the cyst, which was of an oval form, he felt a slight crepitation, similar to that which is experienced in rubbing a piece of dry parchment between the fingers. The knowledge which the professor had acquired of the existence of these tumours with bony parietes, the absence of any fungous growth, or lancinating pain, together with the excellent state of health and the youth of the patient, joined to his ardent desire to get rid of the disease—all these circumstances determined M. Dupuytren to attempt its removal, and to induce him to believe that this was not a case of osteo-sarcoma.

He therefore made a large incision at the labial angle,\* which was prolonged in the direction of the jaw, and carried within the mouth. The bony cyst was divided, a small quantity of reddish serosity escaped, and a fibro-cellular mass was perceived, which was partly extracted with a pair of pincers; suppuration destroyed the rest; and by means of repeated injections, the cure was completed, the edges of the cyst approaching each other little by little, so that the patient retained but a very trifling deformity.

**CASE II.**—About three months ago, the sister of a physician inhabiting the neighbourhood of Tours, a young woman of from 20 to 30 years of age, handsome and robust, came to consult M. Dupuytren, on account of a tumour, the size of a hen's egg, which was situated on the right horizontal branch of the lower jaw. M. Dupuytren having examined it, and finding that there was neither lancinating pain nor

varicose degeneration, and also remarking the feeling of crepitation on pressing the parietes of the cyst, assured the patient that it was not a case of osteo-sarcoma, an opinion which had been previously entertained. Delighted with a prospect of a cure, she entreated M. Dupuytren to perform the operation which he had declared to be necessary. The tumour projected more within the mouth than exteriorly; it pushed the tongue out of its situation, and its growth appeared to have been determined by the incomplete extraction of a carious tooth. An incision was made within the mouth, upon the surface of the cyst; and upon opening into it, a great quantity of bloody serum escaped, but at the bottom, a solid mass was perceived, which was extracted, and found to be perfectly analogous to adipocere: it was so in fact—probably arising from the change produced on the animal matter of the food, which had penetrated the cyst through the alveolus of the tooth, and which had become so metamorphosed during its long stay within the cavity. A few injections, and poultices to the cheek, a bleeding, and a rigid diet for some days, were alone necessary to effect the cure. This patient is perfectly free from every vestige of tumour or deformity.

**CASE III.**—The report of the above successful case brought another young woman to the Hotel Dieu, a short time ago, affected with a disease, to all appearance similar, and who therefore hoped for a cure. In this girl the tumour was also oval, and about the size of a hen's egg; it was situated in the ascending branch of the lower jaw, on the left side. Its growth had been slow, without any lancinating pain, or change of colour in the skin. The tumour was most prominent outwardly, and its position rendered a different mode of operating necessary. The sense of crepitation was as distinctly felt in this, as in the two former cases, and several persons who had examined the tumour had felt it;—however, the number of persons who handled the tumour caused this crepitation to disappear; but M. Dupuytren, being convinced that he had felt it, attributed its disappearance to the constant and frequent application of the thin parietes of the cyst to the parts contained within it. On the 11th of July the operation was performed: the crepitation which had disappeared was again manifest, arising probably from the parietes of the cyst having reacquired their elasticity. An incision, about an inch in length, was made along the posterior edge of the masseter muscle, beginning some lines below its middle. In order to avoid wounding the vessels and the fascial nerve, the incision was continued down to the angle of the jaw; the edges were then separated, and the cyst was perceived, covered by a membrane which M. Dupuytren conceived to be serous, and which was soft and velvet-like to the touch. The whole surface was smooth and even. A stroke of the knife was then made across the bony cyst: a reddish bloody serum immediately escaped in abundance; a plug was afterwards introduced between the lips of the wound to keep them

\* This is the only case in which Dupuytren has ever divided the labial angle.

apart, and emollient injections were made into the cavity, a poultice applied to the cheek, and the patient was ordered to be bled in the arm, if necessary. Up to the present time, every thing is going on well—the cyst is suppurating, and the patient is free from pain and fever.

This case has given M. Dupuytren an opportunity of explaining the diagnostic sign of these tumours, and to establish the marked distinction which exists between them and the osteo-sarcoma, with which they might be confounded upon a superficial examination. The osteo-sarcoma is announced, from its very commencement, by lancinating pains, by a varicose tumefaction, by the participation of the neighbouring soft and hard parts, by fungous growth, and by the inequality of its surface. In these tumours, on the contrary, the neighbouring parts do not partake of the disease; the surface of the cyst is smooth and equal, and its growth is without pain; the osteo-sarcoma grows rapidly; the tumour above mentioned increases slowly. The osteo-sarcoma is internally mingled with bony fragments; which are never met with in these tumours. As to the crepitation, it is never observed in the manner above described in the osteo-sarcoma; whereas it is a pathognomonic sign in the cases alluded to: it resembles that which M. Dupuytren has remarked in those tumours situated half above and the other half below the ligament of the carpus; with this difference, that, in that case, the crepitation proceeds from the striking of one against the other—the upper one displacing the lower, or *vice versa*.—*La Clinique*.

From the London Medical and Surgical Journal.

#### REMARKS ON THE CIRCULATION OF THE BLOOD.\* By C. E. LUCAS, M. D.

The only mode of vital action of the moving fibre, with which we are acquainted, is that of contraction, derived, in some way, from the nerves. That the blood-vessels are endowed with this power, which, from its similarity to that of the muscles, has been called muscular power, has not only been generally admitted, but has recently been proved, by Dr. Hastings, in his work on Bronchial Inflammation, by a body of evidence perfectly irresistible. Assuming, then, the existence of this vital action, I will only observe, that, though possessed by all the vessels, it is more strongly evinced by the capillaries.

\* Most of our readers are, probably, acquainted with Dr. Lucas's very ingenious and useful work "On the Principles of Inflammation and Fever," wherein he discusses the subject of the present essay at some length, and applies his views of the circulation to the pathology of those diseases. Those who have not read that work, and who feel interest in these subjects, would find themselves well recompensed for the time devoted to its perusal.—*Editors*.

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But this vital action of the vessels receives the support of another power, of great importance in the maintenance of the circulation—namely, the mechanical one of elasticity, which is inherent in their structure. This, like the former, differs in its relative proportions in the different orders of vessels, being found to predominate greatly in the larger arteries. The importance of this power seems by no means to have been duly appreciated, and its office, even in the circulation, is but little understood. These two powers of the vessels acting in unison with, and in subservience to, the vital action of the heart, are mainly conducive to the support and regulation of the circulation, the varying conditions of which are only to be explained by this active cooperation; though the mere fact of their existence may be deemed sufficient evidence that the vessels cannot be passive instruments in that function.

It is then with surprise that I find your very able correspondent, Mr. Davies, in his "Principles of Physiology," contending for the exclusive action of the heart, and denying all active participation of the vessels in the circulation of the blood. This I understand him to do on the grounds, that, first, the heart is fully equal to the task, alone; and, secondly, that the exertion of a contractile power on the part of the vessels, beyond that of an uniform accommodation to their contents, would impede rather than promote the circulation through them. This, like every other question in physiology, is best answered by facts. For this purpose it might be sufficient to adduce the paramount one, that we find the blood in motion in the vessels, not only after the apparent death of the animal and cessation of the heart's action, but even after the absolute removal of this organ from the body. Dr. Philip (Vital Functions, second edition, Experiments, 24, 62, 63,) saw the circulation continue in the web of a frog's foot, and in the mesentery of rabbits, for a considerable time after the excision of the heart; and Dr. Hastings has observed the same in several instances. The circulation was, indeed, in these cases often irregular, "the blood oscillating in the arteries, and, in the veins, taking a retrograde course;" this, however, is so far from militating against the active influence of the vessels upon the motion of the blood, that it directly negatives the supposition that it might still be the effect of the last impulse of the heart, and is indeed precisely what must be expected to take place after the presiding influence of the heart is withdrawn. The familiar fact also of the larger arteries being found empty after death is a further proof of this power of the vessels over the circulation. But in the living body, also, we have proofs of the same. Mr. Hunter has observed an artery upon simple exposure to the air contract so as to become impervious; and Dr. C. Parry, though arguing on the opposite side, admits, that when a ligature is placed on an artery, or any other interruption to the circulation takes place, the blood moves in a retrograde current. In these instances

we find the vessels exerting a contractile power sufficient even to overcome that of the heart.

To disprove the second position, that the exertion of a contractile force on the part of the vessels would rather impede than promote the circulation, it will be sufficient to state, that as all reflux of the blood into the heart after it is thrust out by the ventricles is effectually precluded by the valves placed at the origin of the arteries, it is certain, that whatever pressure is made by the contractile powers of the vessels upon the column of blood within them must force it onward in the only direction upon it.

With facts then to show, that whatever force may be exerted by the vessels upon the blood must promote its circulation, and that they do exert a force, when deprived of all assistance from the heart, equal to the emptying of the larger arteries, can we doubt that these powers are applied to the support of the circulation; or, when we see them actually carrying on the motion of the blood after the cessation of the heart's action, can we believe that they do not assist it during its continuance? When indeed we consider the obstacles to the force of the heart, from the length and tortuousness of the vessels, the innumerable subdivisions, &c. we may well doubt whether the unassisted impulse of this organ alone would be equal to the propulsion of the blood through the whole course of the vessels. It is true, that, in the erect position of the body, little more would be required on the part of the heart than to throw the blood over the arch of the aorta, as the force of gravitation would be sufficient to carry it through the vessels of the trunk and lower limbs, and return it to its level at the right side of the heart. A greater power, however, would still be wanted to carry it through the upper parts of the body. But these advantages, would be lost where the body was recumbent; and therefore, where rest was required to restore the wasted powers, more labour would be thrown upon the heart. If the blood also were brought round to the right side of the heart by the action of the left ventricle alone, then a cessation of this, as in syncope, would prove fatal; but if its motion be carried on by the vessels, then the failure of a supply of blood to the heart is effectually provided against.

From what has been stated, a strong presumption at least will arise, that the task of maintaining the circulation cannot be left with the heart alone. A further consideration of the subject will show that the vessels, so far from being passive in it, are very principal agents in carrying it on, with the important advantage also, that the assistance derived from them is, in a great measure, obtained without expenditure of vital power. To show this, let us consider that the heart throws the blood with a given force into an elastic tube, diverging into numberless channels, which again converge into two at the opposite side of the heart, and which, for the present, we will suppose to be there closed. It is plain, that

this system of vessels being filled, *the receiving vessels will be brought to that point of distention at which the resistance of its elasticity will balance the force of the heart*, whatever that may be. If we now remove the obstruction, and suppose that the blood enters at one side of the heart as fast as it is thrown out at the other, the matter is only changed by the establishment of a circulation—the distention remains the same, and must do so, as long as the force of the heart on the one side, and the resistance of the capillaries on the other, also remain the same. Of course, any variation in either of these opposing forces will produce a corresponding variation in the distention of the elastic coats of the artery; but the vessel must needs be in a state of distention—it must, under the given resistance of the capillaries, be filled beyond the point at which the medium of its elasticity would keep it; unless *this* be such as the force of the heart is unable to overcome; in which case it would be *relatively* incompressible. Now, if the heart had not power to alter the diameter of the arteries, all power of accommodation to a varying plethora of blood would be lost; no dilatation of the vessels could take place, and the increased volume could only be circulated by increasing the velocity of the current. We know that this is not the case. We know, however, that a necessity exists for a contractile effort of the artery immediately to succeed that of the ventricle, for, without it, it would be impossible to shut the valves on the side of the heart. To do this by the *vital contraction* of the vessel would require the exertion of a force which we are now supposing the heart to be incapable of, and which we can, with little reason, therefore, attribute to the vessels; for the resistance of the elastic force must be equal on either side of the scale. But, under a diminished plethora, the difficulty would be still greater, as it would be impossible for the vessels to contract upon their contents without the permanent existence of such a force as we have been describing. Such exertion, were it possible, must soon terminate in fatal exhaustion of vital power; for it is to be observed, that the necessity for it would increase with the inability of the system to support it. It is indeed probable that it is in this way that hemorrhage proves fatal.

From the foregoing considerations, we might conclude that the circulation requires a settled preponderance of the power of the heart over the resistance of the elastic structure of the vessels. But we have surer grounds to go upon; we have facts to prove that such is actually the case, and that in a healthy state of the circulation the vessels are kept in a state of forced distention. In Dr. Parry's work on the "Arterial Pulse," it is abundantly proved by a number of experiments, that, in sheep and horses, the circumference of the arteries was diminished above one-fourth after death. As the exposure necessary to the measurement of the vessels was generally followed, after a time, by some contraction, the fallacy, if any, arising from this

cause, tends only the more strongly to confirm the result of these experiments. It is curious that, having ascertained the fact, Dr. Parry should not have deduced the important advantages derived from this distention of the arteries in the steady, permanent force thus brought to bear upon the circulation. Some idea of the amount of this force may be formed by the vehemence of the first gush of blood from a wounded artery, and which is only to be explained by the united pressure of the elastic force from all sides being directed to the point from whence the pressure is removed, and which obviously could not take place were not the arteries in a state of forced distention. The experiments upon which Mr. Davies has arrived at a contrary conclusion are, obviously, in error, by comparing the vessel in the living body with its measure when *injected* after death. Now, the question is not as to the distending power of the injecting syringe, but whether the measure of the vessel during the circulation exceeds that to which it is brought after death by its elasticity. By this arrangement, then, of the relative forces of the heart and vessels, the effectual support of the circulation, by a power not subject to exhaustion, is provided for, and which admits at the same time of the greatest latitude of fluctuation in the fulness of the vessels compatible with life, an arrangement perfectly consistent with the wise economy of nature.

Thus, the aorta and its branches being distended to the point at which their resistance balances the power of the heart, will react upon the column of blood, by their elasticity, (with a force equal in the first instance to that of the distending power, but declining with the diameter of the vessel) thereby at once shutting the valves behind, and propelling the blood forward in a continued current. And here we may ask, whether the equal flow of blood in the capillaries does not show that it is impressed with an equal force, or nearly so, during both the systole and diastole? The jerking stream from a divided artery will form no contradiction to this, as the first gush of blood will empty the vessel so far as to remove at once all propulsive influence of the elastic force upon the current, and make it dependent on the heart alone. We may conclude, then, that the blood flows through the arteries in a continued stream, and, consequently, that the quantities passing out during the systole and diastole of the heart will be determined by the time they respectively occupy. Thus, if the diastole occupy double the time of the systole, and the latter send forth two ounces of blood, it is plain that the diameter of the vessel during the former will be reduced by the displacement of two-thirds of two ounces of blood. The return of the systole will restore the vessel to its previous state of distention, one-third of the blood sent out by it passing on, and two-thirds restoring the lost diameter of the vessel. Now if we look at the capacity of the aorta, even in its undistended state after death, we shall at once

see to what small extent the quantity above stated can raise its diameter. How then shall such dilatation be visible in the distant arteries? We cannot expect to find it so beyond the root of the aorta, where we know it may be seen. But, although the pulse be not thus sensible *to the eye*, in the smaller arteries, yet the declension of impulse upon the blood, during the diastole, will make the succeeding increase of it, upon the return of the systole, sufficiently so *to the touch*, especially when the diameter of the vessel is narrowed by the pressure of the finger.

The question of the amount of the influence of comparative vacuum at the heart, over the venous circulation, will call but for little observation here. The supposition of Dr. Barry, that the blood returns into the auricles of the heart during inspiration only, is sufficiently refuted by the fact of the alternate contraction of the auricles and ventricles, and of the want of all concordance as to frequency and duration between the respective movements of the heart and lungs. When we are told that "the blood passes through the greater veins during inspiration only," we should also be told where the *vis a tergo*, which is so manifestly impelling it through the smaller, terminates; as its effects ought, on the contrary, to become more visible by the increasing velocity of the current as its channels become narrowed towards the heart. As, however, the resiliency of the lungs, whatever it be, must operate in relieving the internal parietes of the chest of so much of the pressure of the atmosphere, which must be extended also to the heart and great vessels as being within that cavity yet external to the lungs, it must, *pro tanto*, assist the return of the blood into the cavities of the heart; and if this advantage be further increased by mechanical enlargement of the sinus venosi during inspiration, as stated by Dr. Barry, the effect will be more considerable. But, although we shall not find in this arrangement the paramount influence ascribed to it over the venous circulation, we shall yet find an ample vindication of its utility, in the support it is calculated to afford to the important functions of the thoracic organs—to the circulation in the support it will yield to the heart at all times, and particularly under dangerous exhaustions of the vital power—and to respiration, in the complete relief of the moving powers of the chest from all exertion during the act of expiration.

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CLINIQUE DE L'HOPITAL DE TROYES  
—PERIPNEUMONIES GANGRENEUSES.—*Observations on Gangrenous Peripneumony.* By M. PIGEOTTE, Physician to the Civil Hospitals and Prisons of the Town of Troyes.

No subject of inquiry is more interesting to the medical practitioner than that which relates to the numerous variety of external causes contributing to give rise to disease. With few exceptions, medical topography, the influence

of the seasons on the human constitution, the state of the weather, the course of the wind, and other meteorological observations, are subjects which have been very much neglected by modern practitioners. These are causes which never cease to act on the human system, and whose influence tends continually to modify its functions. The diseases of every country have their peculiar seasons, and the characters of each vary considerably according to the different circumstances connected with the season in which it prevails. If we examine minutely the general causes of disease, we shall find that a very great majority of the maladies presented to our observation owe their origin to the numerous changes which take place in the state of the atmosphere, or to the certain conditions in which it exists at particular times and seasons. The experience of mankind generally, and the lessons conveyed from one generation to another, have taught them to choose those articles of food which have been found least pernicious to the system. Owing to this, internal causes of disease are, in some measure, avoided. But the action of external causes, physically allied to the animal body, never ceases to modify its functions and to bring about changes in its condition, varying in degrees from mere predisposition to disease, to the total destruction of life. If the peculiar characters of any malady depended entirely, or chiefly, upon circumstances connected with local situation, the profession, generally, might not find that degree of interest in devoting their attention to that malady, which those would who are likely to be called upon to apply their remedies to it; but it should not be forgotten that, however peculiar the circumstances attending a local situation may be, many other situations, attended by analogous circumstances, are to be found; and every practitioner is liable to be called upon to use his judgment founded upon this analogy. But various other external causes, not immediately dependent on local position, exert their influence on the system, and form very interesting subjects of medical inquiry. These, the author of the essay at present before us has not altogether neglected. He traces the origin of a very fatal disease of the pulmonary organs to a long succession of external causes acting on the system, and bringing it ultimately into that state most favourable for receiving the attack of the malady.

The wind continued to blow from the south and the south-east during all the summer, the heat was very powerful, and although rain fell in abundance, the atmosphere was not much refreshed thereby. The heat continued until the middle of September, when the wind became very variable, and the rain, which continued to fall, rendered the nights, the evenings, and the mornings very cool. The wind blew from the north on the first days of October, and the weather became dry without being cold. The season continued as fine as could be desired until the end of the month, when it became all at once quite autumnal;

the evenings and mornings became foggy, the wind blew with great impetuosity from the north-west, and frost commenced. During the month of December, the temperature was cold; the wind from the north, north-west, and west, blew constantly, attended with frost, with cold rain, and with very thick and fetid fog. The plain, on the middle of which stands the town of Troyes, was not the only place exposed to this weather; the same temperature was observed at the time by the physician to the hospital at Langres, and by the meteorologists at Paris.

The weather continued much the same throughout the winter. The cold was very intense, and the atmosphere never ceased to be humid; the wind blowing at different times from the north, north-west, and west, until the month of March, when it settled in the north. The humidity with which the air and the earth had been so long impregnated now gave way to sharp, dry, cold weather.

The diseases observed during the summer presented nothing remarkable in their appearance; they consisted chiefly of gastric bilious fevers, of double tertian, of erysipelas, of gastric pains, of bilious fluxes, and, towards the termination of the season, of adynamic dysentery among the military. Towards the middle of October, the air having become cold, and the fogs prevalent for some days, catarrhal affections of diverse forms made their appearance, such as pain in the ears, swelling of the glands of the neck, and rheumatic pains in the muscles of respiration and in the joints. But towards the end of November many cases of inflammation of the respiratory organs presented themselves at the civil hospital under M. Pigeotte's superintendence, and the characters of this inflammation were so unusually severe as to fix his particular attention. In addition to symptoms of inflammation of the pleura and lungs, often of both sides, which manifested themselves immediately after the invasion of the malady, there came on, on the second or third day, those "of a morbid state, known by the ancient physicians under the name of *putrid*, and of *adynamic* by the moderns." Soon after these made their appearance, the inflammatory symptoms disappeared, or became much less evident than at first. The adynamic symptoms increased very rapidly, and the patients generally died before or towards the end of the first week. No remedy which was tried appeared to make any impression on the disease, or do any thing to prevent, or even retard, the total extinction of the vital forces. On examining the bodies after death, the pleura and lungs were found in a sphacelated state, to an extent more or less considerable. Those parts of the lungs which were not gangrenous presented a very compact texture, and the red colour common to the first degree of inflammation of these organs; or they presented a state of very remarkable softness and flaccidity.

During the months of December, January, and February, M. Pigeotte continued to meet

with this remarkable species of pleuro-pneumony in the wards of the hospital, as well as in the town, among all classes of society. The disease prevailed also in the suburbs to the south and south-west of Troyes, as well as in a neighbouring village, which is situated amongst numerous stagnant pools, and which is enveloped during three-fourths of the year in constant humidity.

M. Pigeotte relates six cases as examples of the plan of treatment pursued in this epidemic, and of the morbid appearances discovered on dissection. We shall give an abridgment of two or three of these. That nearly all the patients should die under such inefficient treatment as was here adopted, is nothing wonderful; indeed it would have been much more wonderful had they recovered.

Pierre Nicolas Berthan, aged 44, was suddenly seized, on the morning of the 27th November, with a cold shivering, which lasted three quarters of an hour, attended with cephalalgia, particularly over the eye-brows, and general pain. These symptoms were succeeded by excessive heat, which terminated about eleven at night in very abundant perspiration. The fever soon increased, accompanied with cough, without any expectoration, and an acute pain in the left side of the chest, just under the breast. The patient continued in this state during the day and the night following, and he entered the hospital on the morning of the 29th. The disease then manifested the following symptoms:—Cheeks of a reddish violet colour; eyes shining and humid; restless looks; more heaviness than pain in the head; tongue charged with a thick coating of a lemon-yellow colour; bitter taste in the mouth; respiration short and laborious; pain in the left side of the chest during inspiration; pulse quick and frequent, but not hard. *Fifteen grains of ipecacuanha and one grain of tartar emetic for two doses. Infusion of elder flowers with oxymel; abstinence.* State of the bowels at this time not noticed. However, the emetic produced vomiting twice, and two intestinal evacuations. *Quieting potion in the evening.* He continued very restless during the night; the cough and pain in the side, which appeared to have ceased after the emetic, again returned; the patient expectorated a great quantity of aqueous, reddish phlegm; the pulse very quick, soft and feeble.

On the morning of the following day (fourth day from the first attack) the pain in the side much relieved, but respiration is laborious; pulse more regular and not so quick; cheeks highly coloured; expectoration very copious, and received on linen, it resembles the reddish serosity generally discharged from sanious ulcers. *At nine in the morning, a drachm of cinchona in wine and water: ether potion, of which a spoonful is to be given immediately after the bark: at eleven o'clock, a repetition of the same medicine: at noon, two grains of camphor and three grains nitrate of potass in pills: these pills were to be continued every four hours.* At five o'clock in the evening, the eyes ap-

peared more brilliant; expression of speech short; respiration more free; the pain in the side no longer felt. At nine o'clock, great restlessness; cough dry; pulse quick, frequent, and small; heat of skin much augmented.

At eight o'clock in the morning of the fifth day, pulse more developed than on the evening before; the matter expectorated was gray, but the spots produced by it on linen were of a reddish colour. At seven in the evening, the pulse quick, but very weak; prostration of strength very great; the intellectual faculties disordered; unpleasant dreams during the night. *The same treatment.*

On the morning of the sixth day, *rale*. Death at six in the evening.

*Examination of the body eighteen hours after death.*—Effusion of reddish serosity in the left thoracic cavity; adhesion of the base of the lung to the diaphragmatic pleura of that cavity; the upper part of the viscus compact, but presenting a red colour, and a friable texture; the lower portion was equally firm, but of a brown slate colour; the right lung and pleura were of a livid slate colour, and their texture was flaccid and putrefied; puriform serosity ran out of the pulmonary parenchyma, but no ulcerated cavity could be discovered which could have furnished it; right cavities of the heart full of black blood; the liver of a flabby texture, of a slate colour, and friable; gall-bladder of the same colour; the tissue of the duodenum was soft in some parts, and its aspect gangrenous; the vessels of the omentum gorged with black blood.

The second case was that of a man, aged 34, drummer to the national guards of Troyes. He entered the hospital on the fourth day after the attack. The symptoms were very similar to those attending the case already described. M. Pigeotte ordered him a drachm of cinchona every two hours, with a spoonful of ether potion immediately after each dose. At five in the evening, increase of fever. *A grain and a half of kermes added to the potion.* The symptoms much aggravated during the night; great fullness of the vessels of the conjunctiva, &c. About nine in the evening his chest was wrapped in flannels steeped in camphorated liniment. The patient perspired very copiously in the night, and towards morning the fever began to abate a little.

On the fifth and sixth days, the same remedies were continued; the disease appeared to gain ground. On the seventh day he had epistaxis, and that, with a copious secretion of urine which came on at this time, relieved him very considerably. The patient continued to mend, and was convalescent on the fourteenth day. We may truly say that Nature cured the patient in spite of the doctor in this case. The other four cases recited by M. Pigeotte terminated fatally. The bodies were examined in two of them; and the thoracic viscera presented appearances very similar to those described in the first case. It is sufficient to notice that the treatment adopted in

these cases did not differ much in principle from that already described.

M. Pigeotte informs us that blood-letting was had recourse to in some cases, but that it generally proved ineffectual. This is by no means improbable, if the remedy was employed in the same manner as it generally is by our continental neighbours. In this country, as well as in any other country, English practitioners endeavour to put a check to acute inflammation of any vital organ, and particularly of the lungs, at the very onset of the disease, by the abstraction of from twenty to forty ounces of blood at its very commencement. Having done so in as short a period as possible, they lose no time in using other remedies of an active nature, and of known antiphlogistic character. The merest tyro would never neglect to use some active cathartic, in order to clear out the contents of the intestinal canal. Bleeding and purging, at the commencement of acute inflammation of an internal organ, constitute such a straight-forward practice among us, that no one ever forgets these whatever he may do afterwards, or however confined his views may be respecting the general nature of disease.

The practice of a French physician is very different in this respect. Instead of abstracting thirty or forty ounces of blood the very first day from a robust man who has inflammation of the lungs, if he bleed at all, he will perhaps abstract six or ten ounces the first day, and repeat the same quantity the second and third day, &c. We have witnessed this plan followed by some practitioners in this country, and have always had reason to consider it worse than useless, even in purely phlegmous inflammation. When the quantity of blood taken away at a time is not sufficient to give a decided check to the disease, it serves only to reduce the strength of the patient, while the malady is gaining ground as rapidly as if no blood at all had been abstracted. The patient may be in this way drained, day after day, of almost all the blood in his system, and the fluid will constantly exhibit the buffy coat to the very last, but the disease will regularly gain ground, and produce disorganization of the tissue, unless arrested by some other means. In epidemic inflammations this is more particularly the case. It may, perhaps, be said, without much impropriety, that, in diseases having a putrid tendency, the greater the mass of blood on which the malady has to feed, the greater will be the putrescency. Whether this constitute an explanation of the fact or not, experience proves that the abstraction of blood at the very commencement of the disease, in epidemic inflammation of the internal organs, prepares the system for the reception of internal remedies; and that, even when bark and other tonics become necessary in the course of the malady, their effects are much more favourable than if no blood had been previously abstracted.

In the cases related by M. Pigeotte, in the essay before us, the state of the alimentary and biliary secretions appears to have been

entirely disregarded. Those on this side of the channel who have the greatest aversion to the use of mercury, would not hesitate to administer a few mercurial purgatives in a disease bearing the characters of that described by the author. Judging from analogy founded upon cases similar to those whose history we have before us, we do not hesitate to give an opinion that, after a copious abstraction of blood immediately, or as soon as possible, after the occurrence of the attack, a very free administration of mercury would have proved serviceable in this epidemic. These remedies ought to have been assisted by leeches applied to the chest, and by blisters. Blisters were, certainly, applied in one or two instances, not to the chest, but to the *legs*!

The epidemic continued to rage with great virulence until about the 20th of February, but after that period it sensibly diminished, and it ceased altogether about the beginning of March, when the northerly wind had blown for some days without rain, and the atmosphere had become dry and clear. The mortality caused by it was very great: of 2650 inhabitants, forming the population of the district in which it prevailed, 120 perished. M. Pigeotte remarks that subjects possessing strong constitutions were no more spared by the disease than those whose constitutions were of the reverse character, and that death did not appear to make particular choice of his victims from those who had been previously reduced by illness, more than from among the robust. M. Pigeotte is of opinion that the morbid characters of the disease in this epidemic, such as the inflammation, mortification, general fever, stupefaction, &c., were neither the effects nor the causes of each other, but were all the effect of one cause, namely, the impression of an atmosphere, impregnated with deleterious effluvia, on the whole organization, and principally on the pulmonary organs. In this opinion we perfectly agree with him. This is probably the case in every epidemic where symptoms of internal inflammation or congestion make their appearance during the progress of the malady. It does not follow, however, from this circumstance, that the local affection should be totally neglected, and allowed to go on to produce mortification of important organs. It may be often necessary to support the system by nutritious diet, or even by tonics and stimulants; but we ought, at the same time, to employ remedies for subduing the local malady, as well, indeed, as the general affection. We may ask, upon what principle abstinence from food is enjoined, while bark, wine, and other are given as fast as the patient can swallow them? If the system is to be supported, surely nutritious food forms the best support which it can have, and the least likely to aggravate the disease. By abstracting blood at the commencement of the attack, or as soon as the patient applies for relief, we certainly take away a portion of a diseased mass, and by then allowing a mild, nutritious diet, we do something towards supplying the remainder

of this mass with comparatively healthy materials. Experience has never yet favoured the supposition that bark and stimulants, in *acute diseases*, have the property of changing the whole mass of blood from the diseased into the healthy state.

Although we agree with M. Pigeotte, that all the anatomical characters of this malady were produced by the peculiar state of the atmosphere, and other circumstances connected with local situation and the season of the year, still we are not disposed to assent to the supposition, that all the structural changes observed on dissection took place simultaneously. The symptoms, as described by him, would lead us to infer that the primary affection of the pulmonary organs was *inflammation*, and that *gangrene* did not take place until a subsequent stage had occurred. In describing the symptoms, the author notices that the pain in the side of the chest was, at the *commencement* of the disease, acute. This acute pain subsided in the course of two or three days from the first attack, and expectoration of a sanious serosity came on. We should be inclined to attribute the acute pain, felt by the patient at the first onset, to inflammation, and the cessation of this pain to the termination of the inflammation in gangrene. The morbid condition of the lungs sufficiently supports this view; for, as much of the organ as was not in a gangrenous state was highly inflamed, firm, and friable. There is little doubt but that the mortified portions also had been in a similar state before gangrene took place. In fact, the morbid appearances described by the author, as forming the anatomical characters of this disease, appear to us to be perfectly analogous to those usually observed in gangrene of a limb, consequent on compound fracture, or any other local injury. The parts in which the inflammation first took place, or had been most severe, had run into mortification, and the rest were running their course towards that state, and some of them would, in all probability, have attained it, had the death of the patient not put a stop to the progress of the local disease.

Gangrene of the lungs is a disease so rarely met with, that scarcely any thing was known of its symptoms, or of its anatomical characters, before the celebrated and indefatigable Laennec gave a description of it, in his work on diseases of the chest. He informs us, that in the course of twenty-four years he only met with it twice; and that he only knew of five or six cases of it that had occurred in the Parisian hospitals during the same space of time. The disease described by Laennec under the term "uncircumcised gangrene," has nothing in common with that whose history M. Pigeotte gives us, but the gangrenous state of the lungs. In the cases related by the former, the local malady appeared to depend upon local or accidental causes; whereas, in the cases forming the foundation of the essay before us, the local disease was brought on by a general cause, and the malady pervaded the rest of the system as well as the pulmonary

organs. In M. Laennec's cases, the disease of the lungs made a gradual progress, and the patients survived under its influence for many days, or sometimes months; whereas, in the cases related by M. Pigeotte, the course of the malady was rapid in the extreme. To show the malignity of this epidemic in a still clearer light, we shall, in conclusion, translate one case more, in which the pleuro-pneumony terminated on the third day, in sphacelus of both lungs, and of some of the abdominal viscera.

A strong, robust, young man, was out at work on the 15th of February, during a very rainy day. In the evening, he was seized with a very violent shivering, to which succeeded great heat, intense cephalalgia over the eye-brows, pain in the throat, a suffocating pain in the side, and an inclination to vomit. On the morning of the second day, a neighbouring surgeon gave him an emetic, composed of two grains of tartarized antimony and twenty grains of ipecacuanha. The symptoms increased; the night was passed in low delirium and extreme agitation. *A large blister to the side, and to both legs.* The third day, the patient could scarcely breathe; the *r  le* had commenced; the pulse small and weak, very quick and irregular; the vital forces generally had sunk. Death took place at four o'clock on the following morning.

*Examination of the body ten hours after death.* The corpse exhaled a most fetid odour. Green and livid spots on the anterior and lateral parts of the neck, and under the hypochondria. A greenish tint of the skin covering the lateral parts of the thorax. Sores made by the blisters brown and black. The glottis and epiglottis presented gangrenous eschars, two or three lines broad. Both lungs and pleur   costales were of a slate colour, flabby, livid, and of a putrid appearance; the lungs were sunk in; their parenchyma was of a green colour, and they appeared as if they had been macerated for fifteen days. The fleshy fibres of the diaphragm were also brown and green; the peritoneum lining the abdominal muscles was of the same colour; those portions of it, also, covering the liver, stomach, and spleen, as well as the omentum, were in a similar state. The membrane was very soft and flabby. The gall bladder and stomach had lost their elasticity, and appeared sunk in, as in a putrefied body.

*Inoculation of Horses and a Cow with Small-pox Matter.*—M. Hamont has recently practised in France those experiments which have been repeated in this country within the last few weeks.\* His experiments were attended with no other result than those local effects which arise from punctures with any irritating matter: these always disappeared after a few days, and nothing whatever analogous to small-pox took place.—*Journal de M  decine.*

\* Vide Journal of Foreign Medicine, vol. ii. page 212.

## Medical and Philosophical Intelligence.

*Amputation during the progress of Mortification.*—A paper on this subject by Dr. Bushe, is contained in the present number of the Journal of Foreign Medicine. The following case, translated from the *Journal Général, &c.* may be adduced as confirmatory of his observations. It forms the subject of a memoir read to the *Académie Royale*, by the late Professor Chaussier, to whom was submitted the examination of the question relative to the propriety of the operation—doubts, it appears, were entertained on this subject, and the payment of the fees of the surgeon was contested, because he had operated contrary to the established rules. After referring to the experience of some of the most eminent surgeons in France, particularly M. Larrey; and of Messrs. Hutchinson, Lawrence, and S. Cooper, in England; M. Chaussier decides in favour of the necessity of the operation, in cases of gangrene arising from an external cause, and of its propriety in the present instance.

M. Prilleux, æt. 28, fractured his left leg about its inferior third, by a fall from a horse; the soft parts were lacerated, and the extremities of the bones protruded. A surgeon was called, and reduced the fracture after the necessary preliminaries. Some days afterwards, gangrene took place in the wound, and spread rapidly to the adjoining parts. Dr. Labesse being called in, explained to the parents of the patient his dangerous situation; his pulse was small, the leg almost completely sphacelated, and amputation of thigh affording the only prospect of preserving his existence, it was accordingly proposed, and acceded to. The mortification spread so rapidly, that, during the space of an hour and a half, while M. Labesse was making the preparations necessary for the operation, it had extended more than an inch and a half. The operation was attended with entire success,—cicatization being completed after the seventieth dressing.

*M. Lisfranc's mode of treating Chronic Abscesses.*—This gentleman has recently communicated to the *Académie Royale de Médecine*, a new method which he successfully employed in treating one of these abscesses of slow formation and great extent. It is well known that an opening made into such abscesses is almost always followed by an inexhaustible discharge of ill-digested matter, which eventually conducts the patient to marasmus and death. M. Lisfranc attributes these accidents to inflammation of the cavity of the abscess, which he resolved to avert, in the case under consideration, by the application of leeches. Immediately upon making an opening, twenty-five were applied above, and as many below, the abscess. The next day the pus was of good quality, and very small in quantity. The leeches were re-applied on the second, third and fourth days. On the fifth the parietes of the abscess were in great part united, the pus of good quality,

and the cure almost completed.—*Journal des Progres, &c.*

*Hydatids in the Palm of the Hand.* By Dr. MAUG, of Esslingen.—Wager, a vine-dresser, æt. 28, of a scrofulous constitution, had for a year past in the palm of his right hand, a tumour about six inches in length and one and a half in breadth. It began about the articulations of the ring and middle finger with the metacarpal bones, and soon extended into the palm of the hand, and subsequently as far as the middle of the forearm. There was no discoloration of the integuments, the tumour was entirely indolent, and gave a loud crackling noise upon pressure; its contents could be readily pushed either backwards or forwards, so that one half of the tumour remained empty. An incision was made about the middle of the tumour, and fifty hydatids were discharged, with about three ounces of inodorous serum. A probe readily penetrated as far as the articulations of the ring and middle finger, and posteriorly, beneath the annular ligament of the carpus.

At the same time that strong compression was made upon the extremities of the tumour, the orifice was maintained open, and during eight days more than a hundred hydatids were discharged. The secretion of serum having ceased, red wine was injected, and immediately afterwards strong pressure was made upon the whole surface of the tumour. In a little while, adhesion had taken place at its two extremities, but a discharge of a small quantity of serous matter still continuing, the injections were repeated, and after a lapse of three weeks the patient was able to resume his laborious occupation. Two months afterwards, during a catarrhal fever, the wound again opened, and about half an ounce of serum was discharged, unaccompanied with hydatids; the injection was repeated, with the same success as before.

The bodies discharged from the tumour were regular, and the greater number cylindrical in shape, the largest was about the size of a small haricot; one of their extremities was conical, the other compressed, so that they appeared to have been placed one against another, and to have formed a kind of chain; they were soft, elastic, of a milky white colour, and retained their size when compressed, permitting the escape of a small quantity of limpid serum which they contained in their cavity.—*Journal des Progres, &c.*

*On the effects of the vinous tincture of the seeds of Colchicum Autumnale.*—With many other Physicians, Professor Chelius bears testimony to the excellent effects resulting from this preparation in rheumatism and arthritis. Investigating the cause of these almost specific effects, he found, that during the use of the colchicum, the urine underwent a marked change, consisting in a striking augmentation

in the proportion of uric acid. The following is the result of the examination of the urine of a patient, affected with arthritic swellings of several of the joints, especially of the knees, which were almost immovable. Before the administration of the colchicum, the quantity of uric acid, both uncombined and in union with ammonia, amounted to 0.069. On the fourth, eighth, and twelfth days after the employment of this remedy, it had increased respectively to 0.076, 0.091 and 0.112, being almost doubled at the expiration of 12 days. Analogous results were obtained from the analysis of the urine of other arthritic patients.

Colchicum is successfully employed both in acute and chronic arthritis. Professor Chelius has never seen it produce any unpleasant effects; but caution is requisite in its administration. He commences with twenty or thirty drops in a little water, and gradually increases the dose till symptoms of gastric irritation are observable. This remedy is also productive of very salutary effects in different stages of prosopalgia, in sciatica, rheumatic ophthalmia, dropsies of the joints, and some species of paralysis of the lower extremities, not arising from an arthritic cause.—*Bull. des Sciences Médicales.*

*On a new Succedaneum for the Sulphate of Quinine.*—Bartolomeo Rigatelli, a chemist of Verona, states that he has discovered a preparation which will form an advantageous substitute for the sulphate of quinine; he has given it the title of *Salino Amarissimo Antifebrile*. A commission, appointed by the Academy of Verona, to investigate this article, has ascertained that it is derived from an indigenuous vegetable, spread over all Europe; that it is obtained in large quantity by a simple process; that it is composed of an acid in combination with a vegetable alkali; and that it contains nothing deleterious to health. The salt is friable, has an earthy aspect, and brick red colour, and is bitterer and more astringent than the sulphate of quinine; it has scarcely any odour, or only a slightly herbaceous one; pulverised, it becomes white, and is then very soluble in water. Multiplied observations have proved, that it may be advantageously substituted for the sulphate of quinine, in all cases where the latter is indicated.—*Bull. des Sciences Médicales.*

*On Valves in the Pulmonary Veins.*—In all systematic works on anatomy, we find it asserted that the pulmonary veins have no valves. It is unnecessary to prove this by multiplied citations—Waller among the ancients, and Meckel as the modern writer, will suffice. The former says, in his *Elementa Physiologiæ*, t. i. p. 145, "Sed etiam vera pulmonalis absque valvulis est;"—and Meckel, in his *Human Anatomy*, vol. iii. p. 368, remarks that the pulmonary veins are usually without valves, with some very rare exceptions.

Professor Mayer's attention was first called to the valves in these vessels, by finding them very numerous and very large in the

pulmonary veins of the cow, although, on looking for them in swine, he found them absent.

In man, however, he found them, on examination, both large and numerous; so that it is difficult to understand how they should have escaped observation. A valve is always met with at the place where a venous branch joins the larger trunks at an acute angle; and the more acute this is, so much more marked is the valve. But where the branches join at a right angle no valve exists; which is precisely what takes place in the other parts of the venous system, as in the extremities, where valves exist where a branch joins the larger trunks at an acute, but not where this happens at a right angle. From this we see why it happens that fewer valves are met with in the pulmonary than in other veins; because the ramifications of the pulmonary veins chiefly take place at a right angle. This form of distribution is particularly the case in swine—and hence in their pulmonary veins there are no valves.—*Mayer in Zeitschrift der Physiologie.*

*On the Employment of Phosphorus as a Caustic.*—Dr. Paillard has lately written an interesting article on this subject. Reflecting on the rapidity with which phosphorus destroys the tissues to which it is applied, the doctor conceived the idea of employing it as a revulsive upon the skin, to remove chronic inflammations of the viscera, of the muscles, or joints. It is more convenient and quicker in its operation than moxa. A piece of phosphorus, about half the size of a lentil, placed on the skin and set fire to, produces great pain, cauterizes deeply, and to as great an extent, as an ordinary cotton moxa. Twenty seconds suffice for this operation. These new moxas may be made of all sizes; they can be applied in a greater or less number, one at a time, or all at once, according to the case in which they are employed. The author has applied twenty-four at once upon the loins, for the cure of a lumbago that had resisted all ordinary means. In a case of neuralgia affecting the thigh and ham, Dr. Paillard placed thirty small moxas from the tuberosity of the ischium to the tendo achillis; they were all lighted at the same time, and extinguished in fifteen seconds, each producing an eschar as large as a *five-sous* piece. The patient (who had not been able to get relief from cupping the whole extent of the limb) was quickly cured. The phosphorus may be also employed to destroy a diseased tissue, or to change the character of a wound or ulcer. Dr. P. says, that he has cured a woman 65 years of age, who had suffered for eighteen months from a cancerous wart under the lobe of the left ear, of the size of a very small pea; upon which a piece of phosphorus of about twice that size was applied; an eschar covered the little tumour, which was detached in six days, and the patient speedily cured. This method is very useful in those timid patients who are alarmed by the preparations for the common moxa;

for scarcely does this caustic begin to act before its operation is over, and yet it has as great an effect as that produced by the long-continued pain of the ordinary moxa, which becomes insupportable from the time it occupies.—*La Clinique*.

*Historical Note on the Origin of Syphilis.*—By Dr. BEER.—An opinion very common among physicians respecting the origin of syphilis is, that the Jews, who were expelled from Spain under the reign of Ferdinand the Catholic and of Isabella, were affected by that disease, and that they conveyed it to the other countries of Europe. C. Sprengel appears to approve of this opinion in his History of Medicine. Dr. Beer adduces some historical points which render this improbable, and to support them he notices a passage from Isaac Abarbanel, born at Lisbon, in the year 1437, a man of great learning, holding some important posts at the Court of Alphonso V. of Portugal, and author of a Commentary, in Hebrew, on the prophets of the Old Testament. In this work, the author remarks on an account given in the prophet Zachariah, where it is mentioned that a certain malady affected all those who fought against Jerusalem. This disease was called *Zarfosim*; he considers it to be the same as syphilis, and that it did not exist among the Israelites, but among those who fought against them.

Very little reliance can be placed upon an inference like this. It is well known that the genital organs are subject to a variety of diseases, and as long as we have no regular description of the characters of those ancient maladies, we cannot with any propriety conclude that they were identical with those of the present day. There is even at the present period some difference of opinion respecting what is syphilis and what is not; what dependence can then be placed upon the result of a comparison drawn between modern syphilis and an undescribed disease which existed between two and three thousand years ago?—*Bull. des Sciences Medicales*.

*Nervous Delirium—Efficacy of Opiate Clysters.*—M. Dupuytren gives the name of *nervous delirium* to a condition which seems much to resemble the *delirium tremens* of practical writers. This form of delirium, which is unaccompanied by fever, often takes place without there being any wound or inflammation; so that it cannot be looked upon as always a traumatic affection. It likewise occasionally comes on after every different kind of wound, and at every period of inflammation—nay, even when the cicatrix is forming—so that it is difficult to point out any specific cause of the disease. Individuals of what is called a nervous temperament, however, are more obnoxious to it than others; and it would seem that those who are much afraid of any operation which they undergo, and still more those who are desponding, suffer from it more frequently than those of greater mental equanimity.

The attack is marked by restlessness; some degree of incoherence; then follows a singular confusion of things, persons, and places, and the patients are occupied day and night with some fancy generally connected with their previous habits and pursuits. They give themselves up to violent and constant movements, which have no object, and abundant perspiration covers the body; the eyes look brilliant and injected; the face becomes flushed, and the expression animated; and the individuals are very loquacious, or even vociferous. Sometimes they are merry, and sing aloud, manifesting no sign of pain.

Notwithstanding these symptoms, the pulse remains quiet; there is no fever; the natural evacuations take place as usual, but there is no appetite; and at the end of from two to five days the disease terminates, sometimes fatally, but more frequently in the recovery of the patients. When recovery takes place, the change is sudden; the patients falling into a profound sleep, as if exhausted by fatigue. From this they awake in ten or fifteen hours, sensible and alive to pain; the appetite returns, and the original disease, whatever it may have been, goes on as before the attack. Sometimes the delirium returns two or three times, and leaves the patient weaker after each accession.

Although the *nervous delirium* may become very dangerous of itself, and although M. Dupuytren has seen some patients (particularly one young man, of robust constitution, in whom it had supervened in consequence of a simple bruise on one of the toes) sink under it in forty-eight hours, without the affection which had given rise to it appearing to contribute towards the fatal event; still he regards its severity, in general, as proportioned to that of the disease which it accompanies. Thus it is much more to be dreaded when it supervenes upon a fracture of the extremities, or of the ribs, or after large wounds, than when it comes on after simple injuries, not in themselves dangerous. Post mortem examination discovers nothing which can explain the phenomena.

Sedatives of every kind, including large doses of laudanum, bleeding to syncope, and all the usual means, have appeared to M. Dupuytren altogether ineffectual—neither arresting the progress nor changing the course of the disease. But a remedy which has always succeeded in M. Dupuytren's hands, and to which he attaches an almost specific effect, is the injection every six hours, and repeated two, three, or four times, of enemata, containing eight or ten drops of tincture of opium in a small quantity of any convenient vehicle. These injections will generally remove the most furious delirium. M. Dupuytren attributes their efficacy to the opium being absorbed from the rectum without undergoing any digestive process; for, as already mentioned, laudanum given by the mouth fails to do any good.—*La Clinique*.

*Effects of Animal Charcoal in Indurations of the Lymphatic Glands.* By Dr. GUNPERT.

About a year ago it was stated in the public papers, that M. W. a physician, had discovered a method of dissipating engorgements of the glands attended with induration; to this announcement I gave no more attention than to the many others with which the public is continually deceived; such was not the case, however, with a man in my neighbourhood, who, seduced by the promises of Dr. W., confided himself to his care. For twenty years this man had had a swelling of the parotid, which had acquired the size of a child's head. He brought me a letter from Dr. W., in which this gentleman stated that his remedy was no other than animal charcoal, from the use of which he said he had derived unquestionable advantage. In preparing this charcoal, he uses common butcher's meat, but especially beef or veal, the fat is entirely removed, and about one-third weight of bone is added; this mixture, divided into small portions, is exposed to the fire in a suitable vessel until it is carbonised, when it is powdered and kept for use. In administering the remedy, Dr. W. mixes an ounce and a half with two drachms of sugar, and of this directs morning and evening, a quantity equal in size to a lentil. Persons in health subjected to the action of this remedy, are attacked with painful engorgements of the mammary and salivary glands, which soon disappear spontaneously; sometimes, also, an eruption of red pimples appears upon different parts of the face, the nose, forehead, &c.; the vascular system is equally stimulated, indicating the necessity of caution in its administration. This substance in no degree impairs the appetite, it even excites it when it is habitually wanting. Such is the information contained in the letter of M. W. Dr. Gumpert subjoins to what he has above stated, that the patient to whom he owes the communication of the letter, having been scrupulously subjected to the treatment of M. W. experienced a manifest improvement after the lapse of three months; the skin covering the tumour was not so tense as before, and had acquired a more natural colour. I cannot deny, observes M. Gumpert, that animal charcoal exerts a manifest action in cases of lymphatic engorgements; I have twice employed it with success in scrofulous swellings of the glands of the neck; it has been necessary, however, to raise the dose in adults to half a drachm (gros,) and even a drachm, two, three, and even four times a day, and it has been given in this quantity, till the increase of the frequency, and the fulness of the pulse, admonished me to discontinue it.--*Rust's Magazine*.

*Cold Affusions in Croup.*—This practice has lately been recommended, as highly useful in the last stage of croup, when the strength of the patient is exhausted; the following case recently published by Dr. Schmidt, is favourable to this method of treatment. A lad, æt. 10, living in great indigence, had a violent attack of croup on the 22d of November, 1826; medical assistance was not procured till the evening of the 24th. At this period he had a

croupal cough; wheezing respiration; pulse small and hard, beating 130 per minute; his countenance had a bluish aspect; a cold and profuse sweat covered his body, and the extreme anxiety which he experienced, indicated a very speedy termination of his sufferings. Ten leeches were directed to the neck, a blister plaster, mercurial frictions, and the fourth of a grain of tartarized antimony every eight hours; a pseudo-membrane of a cylindrical form was expelled by vomiting, and the patient had one alvine evacuation. On the following morning there was an aggravation of all the symptoms; imminent suffocation, absence of cough during the last nine hours, a sensation of pain in the middle of the thorax, the pulse extremely feeble, could not be counted, and the patient had fallen into a state of stupor. It was at this period that recourse was had to cold affusions. Two measures of water, at 10° Rheumer, were employed in the first instance, and sprinkled upon the whole dorsal part of the trunk, from the neck to the sacrum; during this operation the cough recommenced, and another cylindrical portion of membrane was expectorated, followed by some albuminous flocculi. From this moment there was a mitigation of all the symptoms, but this was of brief continuance, and after the lapse of an hour, they re-appeared with the same intensity as before. The affusion was again employed with the same success as in the first instance, and the melioration was almost equally transitory; it was necessary to recur three times to the same remedy, after which the condition of the patient progressively improved during some time, and then remained stationary; the mother of the child refusing to permit its further employment. An aggravation of the symptoms again took place, and it was nothing less than the return of the former danger, that could overcome the opposition of this woman to the use of cold water; but it was then too late; it could only defer, for a time, the fatal event, which took place on the second day of the treatment. Notwithstanding the unfortunate termination of the disease, we cannot overlook the remarkable influence exercised upon it by the cold affusion.\*—*Rust's Magazine*.

*Pregnancy.*—The increased activity of the circulation in the parturient state, has been long known, and the cause has been sought for in the necessities of the fœtus, while, at the same time, it has always been attempted to restrain this augmented activity within proper limits, by the evacuation of a fluid, in these cases, more fibrinous and more abundant than ordinary. No one has hitherto proved that there existed in this state, a modification of the circulatory organs themselves. From observations made by M. Larcher upon a great number of women who have died at different pe-

\* A case analogous to the above is contained in the Journal of Foreign Medicine, Vol. I. page 374.

riods of pregnancy, or shortly after delivery, it follows, that in almost all the subjects thus circumstanced, there was an evident hypertrophy of the left ventricle. According to Laennec, the parietes of this ventricle ought to have a thickness rather more than double that of the right. From the investigations of M. Larcher, it appears that this proportion is almost constantly augmented during the puerperal state, and that this augmentation varies from a fourth to a third of its diameter. The right ventricle and the auricles preserve their natural thickness; the left only, becomes thicker, firmer, and of a more vivid red. Whether the hypertrophy be viewed as the cause or the effect of the plethora, it is evident that it must impart to the circulatory movement, an energy which accounts for all the accidents of pregnancy. This assertion of M. Larcher is one of great interest, and calls for further researches on the part of those physicians, whose situation enables them to make post mortem examinations in puerperal cases.—*Archives Générales de Médecine*.

*Polypus of the Heart.* By M. RIGACCI.—A woman, labouring under disease of the heart, supposed to be an aneurismal dilatation of the left ventricle, after having been subjected to a variety of treatment, died on the 18th December, 1827.

On dissection, besides several unimportant peculiarities, a body of a fleshy appearance, and resembling what is known under the name of sarcoma, was found in the left ventricle. This cavity was much dilated, and its parietes uniformly thinned and distended. From the interventricular septum arose the root of the morbid production just mentioned, which was also attached to the columnæ carneæ of the heart, by means of other radicles. Another root arose by two peduncles from the valvular apparatus of the auriculo-ventricular opening. These two roots soon united, to form a rounded body, two inches, and as many lines in length, terminating in a fringed extremity; it appeared not to have any membranous envelope. On its exterior were observed three reddish lines, which arising from the columnæ carneæ, were continued upon the morbid production, and lost themselves in its substance. Examined with a good microscope, they were seen filled with a reddish fluid, and ascertained to be blood-vessels; their nature was placed beyond doubt by the injection of mercury into two of them; a rupture took place in one at the distance of an inch from the introduction of the tube; the other filled completely, and could be seen ramifying into the substance of the polypus. Upon close examination, the polypus was found to be composed of three or four fibrous layers, superposed upon each other, and intimately connected together. In this case it is evident, 1st, That the polypus was organized and endued with a proper life; 2d, That its formation was long anterior to death, of which it was the cause and not the effect.—*Antologia Firenze, Febbraio, 1828*.

*Perspiration after Death.* By Professor SPERANZA.—A lady, æt. 20, of a robust constitution, was attacked by acute encephalitis, which terminated fatally on the fourth day. The rapidity of her death, and the symptoms which had preceded it, having induced a belief, upon what foundation it is difficult to determine, that the disease was contagious, Dr. Speranza was directed by the civil authority to examine the body. Twelve hours had elapsed since her death, when the Professor made his visit. The whole surface of the skin, which was still warm, was covered by a profuse perspiration, particularly upon the face, where it formed inodorous, viscid, and limpid drops. It was also very apparent upon the neck, chest, and extremities, but less so, than upon the face. From the latter part it was wiped away several times by Dr. Speranza, with a fine linen cloth, and he plainly saw it reproduced, little by little, upon the parts which had been thus dried. The experiment was repeated upon different parts of the body, and the same phenomenon was observed. It was performed with a similar result by the assistants and by Dr. Negri, physician to the hospital of Parma.

Twenty-four hours after death, the body was entirely cold; the face and neck were still covered by a visible perspiration, but it was no longer observed upon the other parts of the body. A peculiar fætor began to be exhaled, and this sign of putrefaction left no doubt of the reality of death.

Several analogous cases are quoted by Dr. Speranza, from the *Ephémérides des Curieux de la Nature*. He supposes that this cutaneous exhalation, which has always been observed a few hours after death, is due to the action of the capillary system, which still subsists for a longer or shorter time, but that when it continues after the body has become cold, it may be attributed to the disengagement of gas, which takes place when the putrefactive process commences, and occurs in the fluids as well as in the solids. In proportion as these elastic fluids are disengaged, they act by their expansive property upon the large blood-vessels, propel their contents into the capillaries, and thus occasion the transudation upon the surface; so that a phenomenon, which in the first instance originated from a remnant of life existing in the circulatory organs, owes its continuance to the exclusive influence of chemical and physical forces. We may cite as examples of transudation after death, the numerous cases of the exhalation of blood, which have occurred one or two days subsequently to that event; in these instances the exudation is the sole effect of putrefaction.—*Opere Mediche Moderne Italiane, tome VI. Bolog. 1827*.

*New Method of Treating Idiopathic Dilatations of the Iris.*—M. Demours, in the name of a commission, read to the Académie Royale, a report upon a memoir, by M. Serres, of Uzes, entitled *De la cauterization de la Cornée, pour corriger d'une manière prompte et sûre les alterations de la vue avec dilatation des pupilles*. In this memoir, M. Serres pro-

poses to treat idiopathic paralysis of the iris, unaccompanied by a morbid condition of the retina or optic nerve, by the application of nitrate of silver to the cornea, near its junction with the sclerotica. In those cases where the retina preserves its sensibility, and injury of the ciliary nerves has diminished or annihilated the alternate motions of the iris, the preternatural dilatation thence arising, is ordinarily obviated by placing before the affected eye, a card, or some similar substance, having a minute perforation in its centre, by means of which, the light is rendered more supportable, and objects more distinctly visible, than without such assistance, especially when placed near the eye.

M. Serres is aware that irritants externally employed, have a special influence in these cases. The electric and galvanic spark, and frictions upon the globe of the eye with a small silver file, followed immediately by the introduction within the lower eyelid of an acrid liquid, a strong cold infusion of tobacco, for example, suddenly induce contraction in the pupil, accompanied with a copious flow of tears. The lachrymal nerve, and the filaments supplying the conjunctiva, being excited by these three means, employed almost simultaneously, the shock is communicated to the ciliary nerves; the pupil almost invariably contracts, so as to recover, in great measure, its natural diameter, and the patient is enabled to read for a few moments, after which, the pupil resumes its previous state of dilatation, preserving, however, some traces of its temporary contraction.

According to M. Serres, the nitrate cannot be applied indifferently to the cornea or sclerotica, experience having proved that the latter is less capable of communicating the irritation thereby excited, to the iris and retina; the cornea, on the contrary, has more numerous connexions with these parts, and it is usual to see increased sensibility of the retina, and contraction of the pupil, accompanying ulcerations of this membrane. The remedy should be applied near the circumference, rather than the centre of the cornea, for two reasons; in the first place, experience evinces that its effects are more prompt; and secondly, we thus avoid the production of a superficial albugo, which, in certain cases, continues for several days, to the disquietude of the patient. Four cases are related in attestation of the efficacy of the plan.

The committee appointed by the Academy have tried the remedy, and bear witness to its utility; three cases in which it was successfully employed, have occurred in their own practice. The application should not continue longer than a second, and the irritation should be carried to the extent of inducing lachrymation, followed towards evening, by a slight injection of the vessels of the conjunctiva. The light cloud which appears on the cornea rarely continues beyond a few days. We may assure ourselves of this fact, by destroying the transparency of the cornea of a living rabbit, by means of slight cauterization, so as to simu-

late a general albugo; on the next, or succeeding day, the membrane will be found nearly as transparent as before. The plan has been tried unsuccessfully in amaurosis, and M. Serres thinks that it can only be usefully employed in idiopathic paralysis of the iris; in those cases where the ciliary nerves, or some other ramifications of the third and fifth pair are alone affected.—*Journal Général de Médecine.*

*Sulphate of Quinine.*—M. Bally, physician to the Hôpital de la Pitié, has ascertained, from a long series of experiments and observations, relative to the sulphate of quinine, that this remedy independently of its anti-periodical effects, possesses a marked sedative property upon the circulatory system. He has been induced, therefore, to employ it not only in intermittent, but also in continued fevers, and even in gastro-enteric inflammations. In the case of a young man, labouring under acute gastro-enteritis, with tenderness of the epigastrium, intense redness of the tongue, continued fever, &c. &c. we saw him administer this remedy in quantity of a drachm, in the twenty-four hours, and effect in a few days a reduction of the pulse, from 75 to 36 pulsations in a minute.—*Nouv. Biblioth. Médicale.*

*Singular effect of Cantharides.* By J. DURET.—A blister was applied upon the thorax of a man, æt. 50, towards the conclusion of an attack of peripneumonia; some days afterwards, an ulcer made its appearance upon the corona glands, about six lines in breadth, accompanied by inflammation of the interior of the prepuce, some degree of phymosis, and by a plentiful secretion of purulent mucus, furnished by the ulcer, and the sebaceous follicles of the diseased organ. A cure was effected by the application of a dilute solution of the acetate of lead.

I should not have suspected any connexion between the application of a blister and the ulceration of the glans, had it not been for the case recently published by Dr. Ammon,\* which proves, from its conformity with the present, that they stand in the relation of cause and effect. The same result has been likewise observed in the horse.—*Journal Universel des Sciences Médicales.*

*Extirpation of the Right Labium.* By Mr. YOUNG.—A lady, æt. 39, married, and mother of several children, of a delicate habit, had suffered much inconvenience, during the last seven years, from an enlargement of the right labium pudendi. Upon examining the tumour, I found it as large as an ostrich's egg, in shape pyriform, of the hardness and weight of schirrus. There was no enlargement of the glands in the groin. Under these circumstances, I determined to remove it, and the operation, which was performed in the pre-

\* Journal of Foreign Medicine, Vol. I. page 283.

sence of Dr. Naylor, consisted in cutting through the base of the tumour, with a single stroke of a broad knife. One or two small branches of the arteria pudendi externa required ligature. The tumour was found to weigh two pounds; when cut into, it resisted the knife, and was almost cartilaginous. The wound has healed remarkably well, and the lady is now almost recovered.—*Lond. Med. Gaz.*

*Partial Hydrocephalus Externus cured by an Operation.* By Professor TEXTOR.—The patient, æt. ten months, had, situated upon the anterior fontanelle, a soft elastic tumour, which made its appearance seven weeks after birth, and from the size of a pea, had progressively attained that of a goose-egg. The tumour was shining, transparent, had a large base, could not be moved from side to side, and did not yield to pressure. Around its circumference, the osseous margin of the fontanelle could apparently be felt by pressing firmly with the fingers; the temperature and colour of the tumour were not different from the adjoining parts. The swelling had always been unaffected with inflammation, pulsation, or pain; the strongest pressure occasioned no uneasiness; the child was lively, in good health, and presented no symptom of imbecility. Some degree of constipation had existed for the last three weeks, and there had been several slight convulsions. The disease having been determined to be partial hydrocephalus externus, a puncture was made, on the 13th of March, with a long, lanciform cataract needle, from which issued a great quantity of perfectly limpid serum; when about half the contents of the tumour had escaped, the wound was closed by adhesive plaster. No unpleasant symptoms followed the operation. April 27, the tumour not having sensibly filled, a second puncture was made, and a great quantity of serum discharged, in every respect similar to the first. All the fluid was evacuated by a puncture made on the 14th May, and immediately afterwards a slight convulsion supervened, which, however, did not recur. The fourth and last puncture was made on the 14th July; the tumour, though the fluid was entirely evacuated, still appeared to contain something, and upon enlarging the opening and making slight pressure upon it, about a spoonful of a soft pulpy matter, not unlike overboiled rice, made its escape; a tent was inserted into the wound, and the whole dressed in the usual way. Without any evident cause, the child was attacked, on the first of August, with slight convulsions and vomiting, which soon yielded to an anodyne mixture; two days after, the wound, which had become fistulous, was opened throughout its whole extent, and the cavity filled with lint; the dressings were removed on the sixth, and gave exit to a considerable discharge of coagulated blood and pus. By the 17th, the cicatrization was completed.—*Journal des Progres, &c.*

*Medical Effects of Hydrochloruret of Lime.*—From the accounts recently published re-

specting the influence of this substance in destroying animal effluvia, Dr. Reid was induced to make trial of its efficacy in neutralizing the morbid poison generated in the fever which prevailed epidemically in Ireland in the year 1826, if such poison really had any existence. It was, however, in some bad cases of dysentery that he first prescribed the hydrochloruret. He directed ten grains to be added to the common enema, and to be administered to the patient night and morning. The fœtor was corrected, and the discharges became much more natural. Another form in which he frequently prescribed the hydrochloruret, with the very best effects, was in combination with tincture of columba, ten grains to two drachms, mixed with four ounces of water, sweetened with sirup, and exhibited in the proportion of half an ounce every hour. "From the observations which I have made," says Dr. Reid, "of the efficacy of this medicine, in cases which exhibited all the severe symptoms of that disease which medical writers have denominated yellow fever, I can with confidence recommend it as a valuable remedy. Indeed, I am induced to expect, that when properly employed, the hydrochloruret of lime will be found as valuable a remedy in the treatment of yellow fever, as mercury has proved in syphilitic disorders."—*Dublin Hospital Reports.*

*On the different Medicinal Properties of Peroxide and Protoxide of Iron.*—Iron is one of the most valuable articles of the materia medica. The PROTOXIDE acts as a genial stimulant and tonic in all cases of chronic debility not connected with organic congestion or inflammation. It is peculiarly efficacious in chlorosis. It appears to us that the PEROXIDE and its combinations are almost uniformly irritating, causing heartburn, febrile heat, and quickness of pulse. Many chalybeate mineral waters contain an exceedingly minute quantity of protocarbonate of iron, and yet exercise an astonishing power in recruiting the exhausted frame. We believe their virtue to be derived simply from the metal being oxydized to a *minimum*, and diffused by the agency of a mild acid through a great body of water, in which state it is rapidly taken up by the lacteals, and speedily imparts a ruddy hue to the wan countenance. We find that these qualities may be imitated exactly by dissolving three grains of the sulphate of iron and sixty-one of bicarbonate of potass in a quart of cool water, with agitation in a cool vessel.—*Lond. Med. and Surg. Jour.*

*Preparation and Properties of Aluminum.*—On these subjects the following statements are made by M. Woehler. The method of preparing aluminum is founded upon the inoxidability of this metal by water. When an attempt is made to heat chloride of aluminum with potassium in a tube, the action is so strong and the extrication of heat is so considerable, that the apparatus is instantly broken. I therefore employed a small platina crucible, the

cover of which was kept on by a wire of the same metal. At the moment of reduction, the crucible became intensely red-hot, both within and without, although it was but slightly heated; the metal of the crucible was not sensibly acted upon. The operation may also be effected in a porcelain crucible with a cover attached. Some small pieces of potassium of about the size of a pea, and not more than ten at once, are placed in the crucible, and upon them are put an equal number of pieces of chloride of aluminum of the same size; the crucible is to be heated with the spirit-lamp, at first gently, and afterwards more strongly, and until the spontaneous incandescence of the matter ceases. Excess of potassium is to be avoided; for after it was oxydized, it would dissolve a portion of the aluminum. The reduced mass is generally completely fused, and is of a blackish-gray colour. When all is cold, the crucible is to be thrown into a large vessel of water; a gray powder is soon deposited, which, when looked at in the sunshine, appears to be entirely composed of small metallic plates; the powder is to be washed with cold water and then dried: it is the metal of alumina.

Aluminum somewhat resembles platina in powder. I discovered some scaly coherent particles, which had the colour and splendour of tin. Under the burnisher it readily assumes the appearance of this metal; rubbed in an agate mortar, it seems to be a little compressible, and unites into larger scales, with a metallic lustre; and it leaves in the mortar traces of a metallic appearance. When heated in the air, until it is ignited, it inflames and burns with great rapidity; the product is the white oxide of aluminum in a hard mass. Reduced to powder and blown upon in the flame of a candle, each particle suddenly becomes an inflamed point, the splendour of which is not less than that of the sparks of iron burning in oxygen gas. In pure oxygen gas aluminum burns with so dazzling a light, that the eyes can scarcely bear it; the heat generated is so considerable, that the oxide produced is partly fused. The particles which have been fused are yellowish, and as hard as corundum; they do not merely scratch, but they cut glass. In order that aluminum may burn in oxygen gas it must be heated to redness.

Aluminum is not oxydized by water, and this fluid may spontaneously evaporate from the metal without its being in the least tarnished; when however the water is nearly at its boiling point, the metal is slowly oxydized, and hydrogen is liberated.

The sulphuric and nitric acids when cold do not act upon aluminum; when heated, concentrated sulphuric acid readily dissolves it, and without the evolution of sulphurous acid; the sulphuric solution did not by evaporation give the smallest crystal of alum.

Aluminum introduced into a solution of caustic potash, even when weak, dissolves readily, and with the evolution of hydrogen; the solution is perfectly clear; the same solution takes place in ammonia; and it is surprising to

observe how much of this earth the ammonia is capable of uniting with: the evolution of hydrogen is similar to that with potash. When aluminum is heated to dull redness, and exposed to a current of chlorine, it inflames and is converted into chloride, which sublimates as fast as it is formed.—*Hensman's Repertoire de Chimie.*

*Corydalin*.—*A New Vegetable Alkali.*—According to M. Wackenroder, this alkali is contained in the root of the fumitory (not the common fumitory, *fumaria officinalis*, but the *fumaria cava*, and *corydalis tuberosa* of Decandolle.) The dry root is to be coarsely powdered and digested for some days in water; filter the infusion, and precipitate with excess of potash; dry the precipitate and treat it with boiling alcohol, until it ceases to dissolve any thing. It sometimes happens that during the cooling of the alcohol, crystals of corydalin are deposited. The solution is to be evaporated to dryness, and the residuum is to be dissolved in weak sulphuric acid; this solution is then to be decomposed by an alkali either caustic or carbonated. A white deposit is formed, which by drying becomes of a light gray colour.

Dry corydalin soils the fingers very much; it is insipid and inodorous. It is soluble in alcohol; and this solution when hot and saturated deposits colourless prismatic crystals of a line in length. By slow spontaneous evaporation, fine laminæ are formed. The solution acts as an alkali upon vegetable blue colours. At a temperature below that of boiling water, it melts into a deep green-coloured fluid, which, when solidified, has a crystalline texture, and is transparent in thin laminæ. At a higher temperature it yields water and ammonia, and is converted into a transparent brown mass. Æther dissolves corydalin with the same facility as alcohol; caustic potash dissolves it in considerable quantity.

This alkali forms extremely bitter salts with acids; sulphuric acid forms two different salts; one which crystallizes is obtained when the acid is digested with excess of base; the solution is to be filtered and evaporated: the production is very slightly soluble in water. When a small quantity of sulphuric acid is added to a solution of corydalin in alcohol, so as not to saturate the base perfectly, a portion of crystalline matter is deposited; and there remains a stratum of a greenish transparent substance, which is unalterable by exposure to the air, and readily soluble in water: the solution reddens litmus paper slightly; an excess of acid renders it purple, and eventually blackens it. Nitric acid when diluted and cold dissolves and forms a colourless solution with corydalin; but when heated it becomes of a red colour, which, when the solution is concentrated, becomes of a blood-red colour. This action is so strong, that by the aid of heat the smallest quantity of corydalin may be discovered in a fluid. Muriatic acid forms with this alkali an uncrystallizable salt; acetic acid is still more difficult of combination with it than sulphuric

acid; but it forms a crystalline salt, which may be re-dissolved a second time in water and crystallized. Tannin is one of the most sensible tests of corydalin, as for all other vegetable bases. The precipitate is white when the solution is dilute, and grayish-yellow if concentrated.—*Hensman's Repertoire de Chimie.*

*Test for Nitric Acid and its combinations.*—Pour a solution of protomuriate of iron upon the surface of an amalgam of zinc, and then place a crystal of nitre upon the latter in the fluid; a dark band immediately forms around the crystals, sometimes extending over the whole surface of the mercury. All the nitrates, as well as nitric acid, act in this manner; but other salts, as the chlorate, produce no effects of the kind; so that a very sensible test of the presence of nitric acid is thus afforded. It is necessary that the solution employed be a protosalt of iron. If nitric acid is supposed to exist in a liquid, it should be saturated with potash, evaporated to dryness, and the dry mass tried. Of course, salts of copper or of silver must not be present.

When an amalgam of brass is used instead of zinc, those effects are not produced; which M. Runge considers as a proof that the zinc or brass is combined, *chemically*, with the copper.—*Annalen der Physik*, 1827, p. 479.

*Analysis of Cancerous Tissue.* By M. COL-LARD DE MARTIGNY.—The following are the results, obtained by this gentleman from the analysis of a gramme\* and ninety-five centigrammes, of cancerous tissue, transmitted to him for examination, by Professor Cruviellier. Albumen, 0.206, gelatine, 0.021, fatty matter, 0.020, traces of phosphorus and salts, water 1.700.

This result is considered by M. Martigny as confirmatory of the opinion which he promulgated about twelve years ago, that cancer was an hypertrophy of the cellular membrane.—*Jour. de Chimie Medicale, &c.*

*Principe immediate de la Dentelaire.*—M. Dulong announces that he has discovered a new crystalline principle in the dentelaire (leadwort), *plumbago Europæa*, Lin. It appears in the form of very minute acicular crystals, of a golden yellow colour; it is partly soluble in water, but more readily in alcohol and sulphuric ether; its colour is not affected by acids; by the alkalies and the subacetate of lead, it is changed to a crimson red. Applied to the tongue, it produces a burning taste, as acrid as that of the root from which it is derived, and which continues for a considerable time. It appears to be neither acid nor alkaline, but a principle sui generis, and much disposed to separate by crystallization from the black extractive matter with which it is mixed.—*Archives Générales de Médecine.*

\* A gramme is 15.4441 grains troy; a centigramme 0.15444.

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**PATHOLOGICAL AND SURGICAL OBSERVATIONS RELATING TO INJURIES OF THE BRAIN.** By B. C. BRODIE, F. R. S. and Surgeon to St. George's Hospital.

[Continued from page 397.]

**SECT. 7.—Treatment of Concussion of the Brain.**

Although the treatment which is required in the first period which elapses after an injury of the head is neither various nor complicated, yet, in order that it should be conducted with advantage, it is necessary that many circumstances should be taken into consideration. We are called upon not only to do that which is to contribute to the relief of the present symptoms, but to guard against future ill consequences, and where no symptoms actually exist we are to look to those which may occur hereafter, and which proper measures of precaution may enable us to prevent or mitigate.

It is commonly remarked that two opposite methods of treatment have been recommended in cases of concussion of the brain; the one consisting of the exhibition of stimulants and cordials: the other comprising blood-letting, and what are usually termed antiphlogistic remedies. Here, however, as on many other occasions, the opposition of opinion is probably greater in appearance than in reality; and I am inclined to believe that if the advocates of the respective systems were questioned on the subject, it would be found that the views which they entertain are not essentially dissimilar. I suppose that none of those who have suggested the exhibition of stimulants would actually be inclined to apply this practice to cases in which the pulse has regained its strength and regularity; and, on the other hand, I conclude that no one among those who have advised the use of the lancet would think of taking away blood when the patient lies with pale cheeks, and cold extremities, and a feeble and intermitting pulse, or would refuse to resort to the cautious exhibition of cordials and stimulants where these symptoms are so urgent that he is manifestly in danger of sinking, in consequence of the depressed

state of the circulation which has followed the first shock of the injury.

Cases of this last description are however in reality of rare occurrence: and there are indeed sufficient reasons why we should regard that condition of the system which approaches to syncope, as being, in the great majority of instances in which it exists, conducive to the patient's welfare, and why we should wish to prolong, rather than to abridge the period of its duration. The same blow which gives rise to symptoms of concussion frequently occasions the rupture of some small vessels within the cranium. The same state of the system which produces an enfeebled action of the heart is calculated to prevent the ruptured vessels from pouring out their contents; and the longer it continues, the less is the danger of internal hæmorrhage. If we artificially excite the action of the heart by the exhibition of wine and ammonia, we are in danger of inducing symptoms of pressure on the brain. If on the contrary we watch the gradual restoration of the pulse, and at the proper moment take from the arm a sufficient quantity of blood to prevent the heart resuming its wonted action, it is probable that we may often succeed in checking or arresting an extravasation of blood on the surface of the brain, or among its membranes, which might otherwise prove fatal. There is also the following very important circumstance which is not to be overlooked in this part of the inquiry. A state of depression is followed by a state of excitement. As the patient recovers from the former, the pulse, with respect to fulness and strength, becomes raised above the natural standard, and it is evident that this affords an additional argument in favour of the practice which is here recommended.

The same views respecting the prevention of internal hæmorrhage, which incline us to take blood from the arm in the first instance, cannot fail to influence our conduct afterwards. There is no evident reason why vessels, which have once bled, should not be liable to bleed again within the cranium, as well as in other situations. I have already mentioned a case in which a patient, who was apparently going on favourably, suddenly expired in consequence of such secondary hæmorrhage, on the fourth day after the occurrence of the in-

jury. If similar cases are rare, this may reasonably be attributed to the remedies which modern surgeons, with few exceptions, do not fail to employ. At any rate, where so much is at stake, we are called upon to neglect no measures of precaution; and however small the danger from this cause may really be, the surgeon should provide against it, by frequently inquiring into the state of his patient: by urging the necessity of continued repose of body and mind, by limiting him to a scanty vegetable diet, by the exhibition of laxative medicine, and by the abstraction of blood, whenever the state of the pulse indicates that this may be done with propriety.

Independently of the foregoing, there are other considerations which might of themselves lead us to adopt the same method of treatment. I believe that the patient in cases of concussion will generally spontaneously recover from that state of insensibility in which he remains after the vigour of the circulation is restored. But, nevertheless, from the best observations which I have made on the subject, I cannot doubt that his recovery is much assisted by repose and low diet, and depleting remedies. Often immediately after being bled, the patient, who before was in a state of stupor, exhibits manifest signs of returning sense. Further, it may be urged that concussion is liable to be followed by inflammation of the brain, or its membranes. Now I do not mean to say that such inflammation can always be prevented, or that the abstraction of very large quantities of blood will make the patient a better subject for it if it should occur; but it seems reasonable to suppose, and our experience of these cases, and other cases bearing an analogy to them, confirms the opinion, that there is less danger of inflammation, where the antiphlogistic treatment has been carried to a moderate extent, and where the patient has been kept in a state of perfect quiet, than where bleeding and laxative medicines have been neglected, and the patient has been allowed to exercise his body and mind, and to live on his usual diet.

The quantity of blood which the vessels of the brain contain depends very much on the position of the head with respect to the rest of the body. Not only in cases of concussion, but in all other cases where there has been an injury of the brain, or one likely to affect the brain, the head and shoulders should be raised by additional pillows, so that the blood may have an easy descent to the right side of the heart. In addition to this, in severe cases of concussion, the head should be shaved, and compresses should be applied constantly with a cold evaporating lotion. Opiates should be avoided. It is difficult to conceive what good purpose they can ever have been expected to answer; and, at any rate, they tend to constipate the bowels, and not unfrequently cause a confusion of symptoms, the patient complaining of headach, of which it is difficult to say whether it belongs to the injury itself or to the opium.

In taking a view of the various satisfactory

reasons which may be urged in favour of a particular plan of treatment in cases of concussion of the brain, we must not overlook the circumstance that this treatment may be carried too far: and we must endeavour to avoid the error which I have known some surgeons fall into, of resorting to a too free use of the lancet. At first when the reaction of the heart has taken place, it may be right that the patient should lose a considerable quantity of blood, so as completely to subdue the force of the circulation. Afterwards, for the most part, it is only an occasional blood-letting that is required, and that to a moderate extent. It has appeared to me that this mode of proceeding has usually done more, both towards relieving the present symptoms, and preventing subsequent inflammation, than a more active system of depletion: and where very large quantities of blood have been already taken away, if inflammation should show itself, our resources are comparatively limited, and we are not able to meet it with that energy and vigour which the circumstances of the case require.

Where bleeding has been carried to a great extent, symptoms frequently occur which in reality arise from the loss of blood; but which a superficial observer will be led to attribute to the injury itself, and concerning which indeed it is sometimes difficult, even for the most experienced surgeon, to pronounce in the first instance to which of these two causes they are to be referred. Repeated copious blood-letting is of itself adequate to produce a hardness of the pulse, which we shall in vain endeavour to subdue by persevering in the same system of treatment. In many individuals it will produce headach and confusion of mind, not very different from what the injury itself had previously occasioned. These things may be observed especially in young females who are disposed to hysteria, and whom I have often known to suffer from a continued aggravation of such symptoms as I have described, while the system of depletion has been continued, recovering immediately on the use of the lancet being laid aside, and on their being allowed to take solid nourishment, with occasional doses of carbonate of ammonia.\*

#### SECT. 8. *Treatment to be employed in cases of Compression of the Brain not complicated with wounds of the brain or its membranes.*

When we consider the variety of circumstances under which compression of the brain may follow an injury of the head, and the different effects which it produces in different instances, we cannot suppose that the same mode of treatment will be found applicable to all cases, or that any such simple rules can

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\* Dr. Marshall Hall has published, in the thirteenth volume of the *Medico-Chirurgical Transactions*, some excellent practical observations on the effects of copious blood-letting, many of which are applicable to the cases mentioned above.

be laid down for the conduct of the surgeon as those which we have to guide us in cases of concussion.

There is one most important complication which aggravates very much the ultimate danger, not only of these, but of all other cases of injury of the head; namely, the existence of a wound or laceration of the dura mater. This circumstance also tends to modify if not to alter the surgical treatment which is to be adopted. At present I suppose that such a complication does not exist; that the brain suffers from pressure, but that the dura mater is entire, and that there is no exposure of the important parts which are contained within it.

Where the symptoms of compression are such that the patient's life is manifestly in danger, there can be no question as to the propriety of removing the cause on which they depend, where that can be accomplished by means of a surgical operation.

In cases in which there is a fracture and depression of bone, it is generally in our power to remove or elevate the depression. If there be a wound of the scalp we may at once resort to the application of the trephine, or in some cases, where the cranium is not only fractured but splintered, we may do what is required by means of the forceps and elevator, without the aid of the saw. Where however the scalp remains entire, it will of course in the first instance be necessary to divide it, so that the bone may be completely exposed, and that the surgeon may be enabled to trace the extent of the mischief which has been inflicted on it.

An operation is also to be resorted to in those cases in which there are symptoms of pressure depending on hæmorrhage between the dura mater and the bone. But here another question arises: what is the evidence which is to enable us to detect a mass of extravasated blood in this situation, and how are we to determine what is the exact part of the cranium which should be perforated by the trephine? I must here refer to an observation which has been already made. Blood is seldom poured out in any considerable quantity between the dura mater and the bone, except in consequence of a laceration of the middle meningeal artery, or one of its principal branches, and it is very rare for this accident to occur except as a consequence of fracture. If therefore we find the patient lying in a state of stupor, and on examining the head we discover a fracture with or without depression, extending in the direction of the middle meningeal artery, although the existence of an extravasation on the surface of the dura mater is not thereby reduced to an absolute certainty, it is rendered highly probable, and the surgeon under these circumstances would neglect his duty if he omitted to apply the trephine. If it happens that no extravasation is discovered, the operation does not leave the patient in a worse condition than he was in before: but if there be an extravasation, although it does not place him in a state of absolute security, it relieves the present symp-

toms, and gives him a chance of recovery which he would not have had otherwise.

Where no fracture is discoverable, yet if there is other evidence of the injury having fallen on that part of the cranium in which the middle meningeal artery is situated, the use of the trephine may be resorted to on speculation, rather than that the patient should be left to die without an attempt being made for his preservation. I cannot indeed adduce any particular experience of my own in favour of what is here recommended; but I conceive that the instances which have been recorded, in which the middle meningeal artery has been ruptured without any fracture of the bone, and the known fact that there is sometimes a fracture of the inner table of the skull, while there is none of the outer table, sufficiently justify such an experiment in desperate cases, or even in those in which there is much danger. Our judgment may be assisted on those occasions by attending to the rule laid down by Mr. Abernethy: "If there be so much blood on the dura mater as materially to derange the functions of the brain, the bone to a certain extent will no longer receive blood from within; and by the operation performed for its exposure, the pericranium must have been separated from its outside. I believe that a bone so circumstanced will not be found to bleed, and I am certain that it cannot bleed with the same freedom and celerity as it does when the dura mater remains connected with it."\*

In applying the trephine on account of a fracture with depression, the removal of a small portion of bone is generally sufficient; and there is indeed no sufficient reason for removing any considerable portion of the cranium. But in resorting to the application of the trephine, on account of an extravasation of blood on the surface of the dura mater, our practice should be different. The bone should be removed extensively, so as to expose at any rate a large portion of the surface of the dura mater, in which the extravasation has taken place. The necessity of attending to this rule, was impressed on my mind by a case which came under my care in the hospital, in the year 1814. A man was admitted with a fracture of the parietal bone, and a large extravasation of blood, between the cranium and the dura mater. I removed two triangular pieces of bone with a straight saw, and a large quantity of blood, partly fluid, partly coagulated, escaped through the opening that was made. The symptoms under which the patient laboured, were immediately relieved, and for several days he appeared to be going on favourably. But suppuration ultimately took place on the surface of the dura mater, wherever the extravasation had separated it from the bone. The opening made by the saw being in a great measure occupied by granulations from the dura mater, afforded no opportunity for the free escape of the pus

\* Abernethy on Injuries of the Head. Edit. 1797. Pp. 33, 34.

which was formed in the neighbourhood, in consequence of which the abscess burrowed between the dura mater and the bone, separating them from each other, much farther than they had been separated originally. As soon as I had discovered what was taking place, I removed another portion of bone with the trephine; but the mischief had now become so extensive that the operation gave scarcely temporary relief, and the patient died. Reflecting on the case afterwards, I could not but acknowledge that if I had removed a larger portion of the bone in the first instance, so as to expose the extravasated blood more completely, the pus which was afterwards secreted could have been freely discharged, and the life of the patient would in all probability have been preserved.

But the most common cause of pressure on the brain is an extravasation of blood within the cavity of the dura mater. Here if there be any large collection of blood in one mass, it is generally in the basis of the cranium; sometimes in the substance of the brain, at other times in the cells between the tunica arachnoides and pia mater. In either of these cases it is beyond the reach of an operation. There may indeed be a large extravasation of blood on the superior surface of the cerebrum immediately beneath the dura mater: but if such an extravasation does exist, in what manner are we to become informed of its existence? We may regard it as a general rule, that an operation is not applicable to cases of compression of the brain from internal extravasation. But there are few general rules in surgery, to which some exceptions may not be made. Let us suppose a case in which a considerable portion of bone has been already removed; in which the dura mater is seen exposed, of a blue colour, lifted up by a collection of blood beneath it, and bulging as it were into the aperture, which has been made in the cranium. Are we not justified in puncturing the dura mater for the purpose of allowing the extravasation to escape? Every thing that we see of wounds of the dura mater tends to prove the very great danger of this kind of injury. The dura mater should never be wantonly punctured; but we cannot doubt that, in what may be regarded as desperate cases, it must be right to give the patient the chance, small as it may be, which the division of the dura mater affords him. The combination of circumstances which would lead to such an operation must be very rare, but it may occur nevertheless, and the surgeon should be prepared to meet it. The late Mr. Chevalier was called to a child a year and a half old, who had received a severe blow on the head. The child lay in a state of insensibility, and was affected with convulsions. There was no wound of the scalp, but on an attentive examination of the head the fontanel appeared to be somewhat elevated. Mr. Chevalier was led therefore to make a crucial incision of the scalp, by dissecting up the corners of which he exposed the fontanel. He then made an angular incision of the right side of the fonta-

nel, and raised the membrane forming it so as to expose the surface of the dura mater, beneath which the purple colour of extravasated blood was plainly to be seen. A puncture being made carefully with a lancet, the blood issued at first with considerable force, spouting to the distance of a foot. Three or four ounces of blood escaped; the symptoms were immediately relieved, and the child recovered without any further unfavourable symptoms.\*

The following case, which is still more remarkable, was communicated to me by Mr. Ogle of Great Russell Street, in whose practice it occurred some years ago.

A woman, who kept a cellar in Monmouth Street for the sale of second-hand linen, &c. fell from the street, head foremost, to the bottom of the cellar. When taken up she was in a state of total insensibility. Mr. Ogle being immediately sent for found her lying as if in a fit of apoplexy. He ordered her head to be shaved, and, on examining it afterwards, discovered no wound of the scalp, but observed that she flinched very much when pressure was made on one spot near the anterior and superior angle of one of the parietal bones. Having made an incision of the scalp at this part, he could perceive no appearance of fracture. Nevertheless as the woman was manifestly in imminent danger, he thought it expedient to remove a portion of the bone with the trephine. Immediately on the bone being removed, the dura mater of a dark colour rose into the opening nearly as high as the external surface of the cranium. Convinced from its appearance, and from the feeling of tension communicated to the fingers, that a fluid was interposed between it and the brain, and that that fluid was blood, Mr. Ogle ventured to puncture the dura mater with the point of a lancet. The puncture was instantly followed by a stream or jet of blood, which spirted out to the height of some feet. Immediately on the blood being discharged, the woman, who till that moment had continued totally insensible, opened her eyes. After looking about her, apparently amazed, she exclaimed, "What is the matter? what are you doing with me?" and was able to give a clear account of the manner in which the accident had occurred. From this time she recovered without any untoward symptoms. It was impossible to ascertain the precise quantity of blood which escaped through the opening of the dura mater, but Mr. Ogle supposes it to have been about three quarters of an ounce. But cases such as these are to be regarded as out of the common course of events. The ordinary cases of extravasation within the dura mater from injury are to be treated as we treat cases of apoplexy, or of paralytic seizure, in consequence of a blood-vessel within the head being ruptured from disease: on the same principle as that on which we treat other cases of internal hæmorrhage.

\* Medical and Physical Journal, Vol. VIII. p. 505.

Take blood from the arm so as to reduce the force of the heart's action. Repeat this, or take blood by cupping, as soon as the pulse has recovered from the effect of the former blood-letting: administer active saline purgatives; let the head be shaved and bathed with a cold lotion, being kept at the same time in an elevated position; and although such a plan of treatment will not effect the cure of a patient who lies with stertorous breathing in a state of perfect stupor, many will recover under it, in whom the symptoms of pressure have been very urgent. In some instances a slight improvement is perceptible from day to day, until at the end of two or three weeks the patient seems to be restored to his natural condition. In other instances his recovery is less complete, and a partial loss of nervous power may remain for many months; or such a memorial of the accident as, a dilated pupil, a benumbed hand, or a paralytic limb, may exist for a much longer period, for years, or even during the remainder of the patient's life.

The foregoing observations are intended to relate to those cases in which pressure operates on the brain in such a manner as considerably to impair its functions. There are many other cases in which there is reason to believe that there is extravasation of blood within the cranium, although not in sufficient quantity to produce any formidable symptoms. I have already observed that it is sometimes difficult to distinguish such cases from those of concussion of the brain; and it is therefore fortunate that, even where the distinction is plain, it leads to no difference of treatment.

It is also not uncommon for a fracture of the cranium to exist, with even a considerable depression of bone, and for the patient to suffer from it only in a very small degree, or to have no symptoms at all. Mr. Abernethy has published an account of several cases of this description, in which there were not only no symptoms at the time, but none at any subsequent period, although no attempt was ever made to restore the depressed bone to its natural situation, and I might add to the catalogue many similar cases which have fallen under my own observation; but the fact is now well known to every practical surgeon; and in doing so, I should unnecessarily occupy the time and attention of the Society. Here the condition of the patient immediately after the accident does not indicate the necessity of an immediate operation; and a very interesting and important question arises as to the course which the surgeon should pursue, and whether he should, or should not, under these circumstances, resort to an operation for the purpose of elevating or removing the depression?

The removal of a part of the cranium is not to be viewed as a trifling matter, or as an operation which we are warranted in performing without a very sufficient reason. 1st. The process, by which the aperture in the cranium is filled up with new bone, requires many

years for its completion, even where the aperture is small; and where it is large, that process is never completed at all. The deficiency of the cranium must render the patient much more liable to suffer from accidental injury than he would have been if the cranium had been perfect. The cicatrix must be more easily penetrated by a cutting instrument, and more likely to give way under the force of a severe contusion than the bone itself; and in the second volume of the Edinburgh Medical Essays, a case is recorded in which, during a violent fit of the whooping-cough, such a cicatrix was lacerated, the dura mater torn, and the brain made to protrude through the wound, the patient dying with paralysis of the limbs five days afterwards 2dly. Without referring to those remote consequences, or to cases in which it has been carelessly or improperly performed, the operation of the trephine is not to be regarded as one altogether free from danger. I saw a case in which a surgeon was induced to apply the trephine, although, as the event proved, there was no sufficient reason for so doing. The dura mater, at the time of the operation, was found adhering to the bone, and in a healthy state. Nevertheless, when the patient died some time afterwards, the body was examined, the external layer of the circular portion of the dura mater which had been exposed in consequence of the trephine being employed, was found in a state of slough, and it was a matter of doubt whether the sloughing did or did not extend through the whole thickness of the membrane. In another case, which occurred in St. George's Hospital, Mr. Gunning was induced to apply the trephine, in consequence of a suspicion that suppuration had taken place between the bone and the dura mater. The suspicion proved to be ill-founded: the dura mater was in a perfectly natural state, and there was bleeding from the small vessels on its surface after the renewal of the bone. The patient died afterwards in consequence of inflammation of the brain and pia mater. On dissection, besides the usual appearances produced by such inflammation, it was found that the circular portion of the dura mater which had been exposed in the operation was in a state of slough, the slough extending through its whole substance. Everywhere else the dura mater was in a natural state. It is reasonable to conclude that the sloughing of the dura mater in these cases was the consequence of its being deprived of its natural protection, and of the supply of blood which it receives through the vessels of the bone.

Now if the patient, whose case was mentioned last, had survived some time longer, what would have happened? The slough of the dura mater would have separated, and the brain losing the support which it derives from this firm membrane, and having its vessels loaded with blood, would, in all probability, have become protruded in the form of what is denominated a *hernia cerebri*. Such a protrusion would not indeed aggravate the dan-

ger of the case, where suppuration had already taken place within, but it might make the difference of life or death to the patient where the inflammation had not begun to terminate in this manner.

That the removal of a portion of the cranium may in itself be sufficient to make the patient liable to this formidable and dangerous disease of *hernia cerebri*, would appear sufficiently probable without any particular experience on the subject; and for evidence that this is actually the case, we need not go further than the Transactions of this Society. I allude to the very important paper by Mr. Stanley, published in the eighth volume of this work. In every one of four cases, which are here recorded, in which a portion of the bone of the cranium had been removed by the trephine or straight saw, the dura mater was found not to have suffered from the injury, yet a *hernia cerebri* presented itself some days afterwards. In one case it is distinctly stated that the dura mater was in a state of slough at the time of the protrusion beginning to take place; but it does not appear whether in the other cases it gave way in consequence of sloughing or ulceration.

Taking all these facts into consideration, we cannot refuse our assent to the proposition that the perforation of the skull, and the removal of a part of it, is attended with a certain degree of danger, and the evidence hitherto adduced is in favour of the opinion, that "it is most prudent to abstain from the use of the trephine, where there is a fracture with depression of the cranium producing at the time no unfavourable symptoms."

But much may be said on the other side of the question; and at any rate there are other points to be considered before we can arrive at a positive conclusion on the subject.

1st. Although in some cases sloughing of the dura mater and *hernia cerebri* may follow the operation of the trephine, there are many other cases in which this never happens, the dura mater granulating, and the wound cicatrizing favourably.

2dly. Notwithstanding that a depression of the cranium is allowed to remain in many instances without its being productive of any bad consequences, there are numerous examples of such an injury being followed by extensive mischief. Suppuration takes place on the surface of the dura mater, an abscess is formed between that membrane and the bone, and ultimately (as I shall endeavour to explain on a future occasion,) if the abscess has no opportunity of discharging itself externally, the inflammation extends to the parts below, and there is suppuration of the tunica arachnoides and pia mater, leading inevitably to the patient's destruction.

3dly. Where a depression of the cranium is allowed to remain, it sometimes happens that symptoms arise after a considerable lapse of time, which may even endanger the life of the patient, and which are to be attributed to the continuance of the depression, although it had occasioned no inconvenience in the first

instance. I saw a well-marked and very instructive case of this kind several years ago under the care of Sir Everard Home, of which Sir Everard has published some account in the Philosophical Transactions for the year 1814. A gentleman received a blow on his head in consequence of having fallen from his horse, which occasioned a fracture and depression of one parietal bone. The depression was two inches and a quarter in its longest, and an inch and a half in its shortest diameter, and in one part nearly three quarters of an inch below the natural level. At the end of six weeks the early symptoms had subsided, and the patient was considered well. As soon however as he returned to his usual occupations, various nervous symptoms began to show themselves, which manifestly depended on the continued pressure on the brain.—These symptoms, instead of diminishing, increased in severity, and on some occasions were such as to occasion serious alarm; in consequence of which, at the expiration of three years from the time of the accident, Sir Everard was induced to remove nearly the whole of the depressed bone with the trephine. The wound cicatrized readily. The symptoms which existed before the operation were immediately relieved, and, as I have been informed, never recurred.

In this case the fracture and depression were very extensive, and probably these ultimate ill consequences, or secondary effects of the injury, may be avoided, if we consider it as a general rule, that an extensive or deep depression should lead to the application of the trephine, although the same necessity does not exist where the depression is small.

This rule however affords us no assistance with respect to the greater danger arising from the chance of suppuration between the bone and the dura mater; this being as likely to occur where the depression is small as where it is large.

Sir Astley Cooper has stated in his Lectures on Surgery\* that there is a great difference as to the danger of inflammation and suppuration of the membranes of the brain, between those cases in which the fracture and depression is complicated with a wound of the scalp, and those in which the soft parts are uninjured; such mischief being much more liable to occur in cases of the first kind than in those of the second: and on these grounds he recommends that, where this complication exists, we should not hesitate to apply the trephine; and on the other hand, that, where it does not exist, we should carefully abstain from adding to the injury, by dividing the scalp and exposing the fracture. But many persons undoubtedly have recovered in whom there was at the same time a wound of the scalp, and a fracture and depression of the cranium, although no operation was resorted to. The cases to which I have before alluded as pub-

\* The Lectures of Sir Astley Cooper, Bart. by F. Tyrrell, &c. Vol. I.

lished by Mr. Abernethy, are all examples of this fact; and I recollect other similar cases which have fallen under my own observation. I have conversed also with several other surgeons whose experience on the subject has corresponded with my own, and all these circumstances led me in the first instance to doubt the accuracy of Sir Astley Cooper's conclusion.\*

The question however is not to be decided merely on these premises. Many persons may do well without an operation, who suffer from what Sir Astley Cooper denominates a compound fracture of the cranium, and yet it may remain to be determined what is the probability of suppuration taking place in these cases, as compared with those in which the scalp escapes uninjured?

For many years I have preserved notes of a large proportion of the cases of injury of the head, which it has fallen to my lot to witness. Among them, of course, are many in which

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\* The following statement was furnished to me by my friend and colleague, Mr. Rose, from notes which he made while surgeon to the Coldstream Regiment of Guards during the Peninsular war:—

“In the battle of Talavera de la Reyna, which was fought on the 27th and 28th of July 1809, the brigade of guards lost about 600 men in killed and wounded. Amongst the latter were a considerable number of cases of wounds in the head. There were a great many cases of fracture of the bones of the cranium with and without depression, and from the cause which produced them, these were, of course, in every instance complicated with wounds of the scalp.

“On the third of August, in consequence of some military movements, the town of Talavera, in which the hospital had been formed, became exposed, and an order was given for the wounded who could march, to leave it. This was so speedily obeyed that no time was afforded to make any selection. The worst cases necessarily remained, but among those who undertook the march there were twelve or fourteen with wounds in the head, accompanied with injuries of the bone, at least four or five of whom had both tables of the skull fractured, and two of them, along with fracture of the *os frontis*, had each the globe of one eye totally destroyed. In none of them had the trephine been applied, nor had any attempt been made to remove splinters of bone. After leaving Talavera, they were exposed to a burning sun, and to very severe fatigue. Every evening, after the day's march, Mr. Rose collected the wounded round him, examined and washed their wounds, dressing with care those that particularly required it. Cold water was the principal application employed. The retreat occupied sixteen days, in spite of which, and with no other treatment than that which has been described, every one of those who were wounded in the head recovered.”

there was fracture, with or without depression, followed by suppuration between the *dura mater* and the bone. On referring to these for further evidence on this interesting subject, I find that the cases in which suppuration takes place where the scalp is entire have been comparatively rare; bearing a very small proportion indeed to those cases in which suppuration has followed a fracture complicated with a wound of the scalp. Such is the result of my own experience, during a considerable period of time, and which I am enabled to give not merely from a general recollection of what I have seen, but on the authority of written notes, made at the bedside of the patients, and for the most part before the question which they illustrate had ever presented itself to my mind.

Taking all these facts into consideration, and endeavouring to give its proper value to what may be urged on either side of the question, I cannot but acknowledge, whatever may have been my first impression on the subject, that it appears to me at this moment that the views of Sir Astley Cooper are well-founded; and that, in those cases in which depression of bone exists without any symptoms, or with only trifling symptoms arising from it, the surgeon can follow no better general rule than this: if the depression be exposed in consequence of a wound of the scalp, let him apply the trephine, and elevate the depression: but if there is a depression without a wound of the scalp in consequence of the accident, let him not make such a wound by an operation. An exception may perhaps be properly made with respect to very extensive depressions of the cranium, which it may be prudent to expose and elevate at all events, not because there is a greater danger of suppuration from these than from smaller injuries, but on account of the ultimate ill consequences which the patient may experience if the brain be left permanently subjected to a very considerable pressure.

I have only two further observations to offer before I leave this part of the inquiry.

The first is, that even where the fracture and depression of bone is complicated with a wound of the scalp, there is not, in all cases, the same absolute necessity for the application of the trephine. The bone may be depressed in such a manner as to allow the escape of the pus which is formed on the surface of the *dura mater*, although the depression is not elevated; or its position may be such as that the abscess can find no external opening. The danger in the first case must be infinitely greater than that in the second. A boy was admitted into St. George's Hospital who had received a severe blow on the head. The scalp was wounded, and there was a fracture and depression of bone: but as the depressed bone was not of a large size, as it was not much below its natural level, and as it produced no symptoms of importance, I did not apply the trephine. Eight or nine days after the accident, the boy complaining of pain in the head, the pulse having become fre-

quent, and there being an expression of anxiety in the countenance, I divided the scalp beyond the wound which already existed, so as to expose the fracture more completely. I now discovered that suppuration had taken place beneath the bone, but the edge of the depressed bone was so much below the level of the bone in the neighbourhood that there was a very free opening for the escape of the pus, which was distinctly seen at the bottom of the wound, rising and falling as the pulsations of the brain were communicated to it. No further operation was performed. The symptoms were relieved by the more free division of the scalp; the wound healed, and the patient left the hospital quite recovered about five weeks after his admission.

The last observation relates to a circumstance, the possible occurrence of which adds to my unwillingness to divide the scalp in cases of fracture and depression of the cranium, where it has not been already divided with violence inflicted on it at the time of the accident. I have seen two cases in which the scalp remained entire, but in which the bone was fractured and depressed, and the dura mater lacerated, and the brain itself wounded by the edges of the fracture. Such a complication may be sufficiently dangerous under any treatment; but if we are to judge from the analogy of what occurs not only in cases of simple and compound fractures of the extremities, but of other mechanical injuries, we must suppose that the danger would be much aggravated by the addition of a wound of the scalp. Suppuration of the brain and its membranes, to a greater or less extent, must necessarily ensue, if they are exposed under the circumstances which have been described, and it seems not improbable that such mischief may be avoided if the scalp be allowed to remain entire for their covering and protection. Of course this remark applies only to the conduct of the surgeon in the first instance. The treatment to be pursued, if, at a later period, suppuration should be actually established, is not under our present consideration.

#### SECT. 9.—*Treatment of Contusions and Wounds of the Scalp.*

As the treatment to be employed in cases of concussion and compression of the brain involves questions of peculiar interest, which demand the earliest attention of the surgeon in the greater number of instances of injury of the head, I have thought these subjects not undeserving of our first consideration. It remains for us to determine the course which is to be pursued in other cases, of which the principal are, those of wounds and contusions of the scalp; and of fracture, unattended with depression, where there is no reason to believe that there is extravasation of blood beneath the bone; and those of wounds of the brain or its membranes.

Extravasation of blood in the cellular texture of the scalp seems to require for the most part no particular attention. Here, as elsewhere, the swelling made by the extrava-

sation gradually becomes less prominent, and more diffused, and no great length of time elapses before it disappears altogether. I was consulted concerning the case of a young gentleman in whom there was an effusion of blood under the scalp, extending from the superciliary ridges to the nape of the neck, and from ear to ear. When I saw the patient the blood appeared to be still in a fluid state, or at any rate not completely coagulated; and it had been poured out in such quantity that the cranium itself was not in any part perceptible to the touch: nevertheless, in the course of a few weeks, with no other application than that of a cold lotion, the whole tumour disappeared.

It is evident that, whatever was the vessel ruptured in this instance, it must have continued to bleed for a considerable time before so large an extravasation could have taken place. In another case in which a vessel under the scalp was bleeding in the same gradual manner, and threatening to produce similar results, I was enabled to ascertain the point at which the extravasation began, and by making pressure in this situation to stop its further progress. The patient was a child who had received a blow on one temple, I believe, from the corner of a table. Soon afterwards the nurse observed a swelling in the part which had been struck, which however attracted but little attention at the time. On the following day the swelling had increased, and the parents brought the child to London, a journey of several miles. During the journey, the swelling became still larger, and when I was consulted soon after their arrival in London, it occupied the whole temple. I directed the child to be kept quiet, and the head to be bathed with a cold lotion. Next day, however, the swelling had extended over a considerable part of the head adjoining the temple, presenting an appearance exactly similar to that which was observed in the case last mentioned. I now inquired of the nurse, more particularly than I had done before, what was the exact spot at which the head had been struck, and in which the swelling was first discovered; and having ascertained this, I applied a graduated compress and bandage, such as is used after bleeding in the temporal artery: and from this time there was no further increase of the swelling.

Punctured and incised wounds of the scalp require (in the first instance at least) no peculiar treatment. Nothing that has occurred in my own experience would lead me to believe that there is any reason why adhesive plaster should not be employed to approximate the edges of a wound of the scalp, as well as those of a wound elsewhere. Erysipelas not uncommonly follows a wound of the scalp, but it seems to me to occur equally, whether the wound is dressed with adhesive plaster or in any other manner.

When a portion of the scalp is separated in the manner of a flap, so as to expose the tendon of the occipito-frontalis muscle, or the pericranium, if it be carefully and neatly re-

placed, it will often become united by the first intention to the parts from which it has been separated. In many cases however there will be no adhesion, as where some time has elapsed before the wound has been dressed; or there has been considerable contusion; or the surface of the wound has been smeared with dirt, or other extraneous substance. In other cases there will be partial adhesions, some parts of the wound becoming united while there is suppuration elsewhere; and (as I shall have occasion to observe hereafter) this state of things requires much attention on the part of the surgeon, lest the formation of abscesses in certain places should do injury to the pericranium and bone, and destroy the adhesions in the neighbourhood.

In those cases also, in which the pericranium is separated from the bone, it is for the most part right to replace the scalp, with the torn surfaces in contact, and to allow them to have the chance of becoming united, whatever that chance may be. Such union will not unfrequently take place even in the adult, where the bone is not exposed to a great extent, and the parts are nicely adjusted to each other; but there is much more reason to expect it in the young person, on account of the greater vascularity of the harder textures before the period of growth is concluded.

#### SECT. 10.—*Treatment of Fractures of the Cranium unattended with Depression.*

It seems to be the general opinion of modern surgeons that a fracture of the cranium, where there is no depression, and no evidence of any considerable extravasation between the dura mater and the bone, requires nothing beyond the strict antiphlogistic treatment, which ought to be resorted to in all cases of injury of the head. The fractured surfaces being here in contact are under circumstances the most favourable to the process of union, and the removal of a portion of the bone with the trephine must be regarded as a considerable, and as far as the fracture itself is concerned, a wanton addition to the mischief already inflicted, which, instead of expediting, cannot fail materially to retard the patient's ultimate recovery.

The application of the trephine, under these circumstances, has nevertheless been recommended by Mr. Pott; and I should be guilty of a serious omission if I were to pass over in silence a question of such importance, and relating to a point of practice which has received the sanction of such high surgical authority.

In the perusal of Mr. Pott's treatise on Injuries of the Head, we cannot but feel some degree of astonishment that that eminent surgeon should have resorted to an operation with so little hesitation in a number of cases, in which the existing symptoms were of trifling importance, and in which there was no evidence of immediate danger. It does not appear however that Mr. Pott, on these occasions, acted merely under the influence of his early prejudices, or of the example of those

who had gone before him; and although not formally stated in his writings, the following argument may be deduced from them in favour of the practice which he recommended and adopted.

1st. The blow which occasions a fracture of the cranium, is likely to do such further injury to the vessels of the dura mater as may lead to inflammation and suppuration of the external surface of that membrane, and the formation of an abscess between it and the bone.

2dly. If such an abscess be formed without a free external opening, the case must terminate fatally.

3dly. If immediately after the accident a portion of bone be removed by the trephine, the pus formed afterwards on the surface of the dura mater is enabled to escape, and the danger arising from its confinement beneath the bone is avoided.

But it may be urged in opposition to this doctrine, 1st, That Mr. Pott seems, on the one hand, to have greatly over-estimated the danger of suppuration between the bone and the dura mater in cases of simple fissure of the cranium; and that such mischief will be avoided in the very great majority of cases, provided that, from the moment of the accident, the patient be kept in a state of perfect repose, on a spare diet, with the head cool, blood being taken occasionally from the arm, and these remedies being combined with the use of saline purgatives.\*

2dly. That he seems on the other hand to have under-estimated the evils which may arise from the removal of a portion of the cranium, to which in fact no allusion is made in any part of his writings. On this subject it is needless to repeat the observations which I have made in a former part of this paper.

3dly. That even if suppuration should take place between the bone and the dura mater, a watchful surgeon may generally detect the circumstance before pus has been formed to any great extent, and that the patient has still the chance of being preserved by the timely application of the trephine.

We can scarcely hesitate to admit that the reasons for abstaining from the use of the trephine under the circumstances which have been described, are more conclusive than those which may be urged in favour of a more active treatment; it being at the same time borne in mind that cases of fracture of the cranium, even without depression, are always to be re-

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\* Whoever reads Mr. Pott's observations on this subject, and compares them with what is now seen in hospital practice, will, if I am not mistaken, find good reason to believe that suppuration between the dura mater and the bone in consequence of a fracture, is less common at the present period than it was, when Mr. Pott wrote; a difference which may fairly be attributed to the more strict antiphlogistic treatment, which modern surgeons do not fail to adopt in all cases of injury of the head, whether the early symptoms be or be not of a dangerous description.

garded with a jealous eye, especially where the scalp is wounded and the pericranium separated from the bone, it being in these cases especially that danger exists of the formation of matter between the dura mater and the bone.

SECT. 11.—*Treatment of Wounds of the Brain and its Membranes.*

Although the condition of the patient who labours under a wound of the brain, or dura mater, is essentially different from that of one in whom no such wound exists, the general treatment required in these two orders of cases is nearly similar; and bleeding, purgatives, low diet, and a state of perfect repose, form an important part of the remedies to be employed in cases of wounds, as well as in those of concussion and compression of the brain.

The object of the local treatment, where there is a wound of the brain or its membranes, is not so much to relieve the existing symptoms as to prevent future ill consequences, the principal of which are (as I shall show hereafter,) 1st, inflammation extending from the wound over the membranes of the brain, and producing an effusion of serum and pus; 2dly, inflammation, suppuration, sloughing, and dissolution of the substance of the brain; 3dly, protrusion of the brain, in the form of what is commonly denominated a *hernia cerebri*.

A judicious surgeon will always bear in mind, that, especially on these occasions, the first rule of his art is not to add to the mischief already done. If splinters of bone have penetrated into the brain, and can be removed with perfect facility, and without the smallest additional disturbance to the injured organ, such removal cannot be improper, and may probably be useful. Many persons however have recovered, in whom an opposite practice has been pursued. I saw a gentleman in whom detached fragments of bone remained imbedded in the brain, many months after he had received a wound in the head from a pistol bullet, and who suffered scarcely at all from the injury. Do not such cases justify us in leaving splinters of bone untouched, where there is any kind of obstacle to their easy extraction? Are they not even sufficient to show that any other mode of proceeding would be improper, and that it is better to leave the patient to take his chance with the splinters lodged in the brain, than to commit the smallest additional violence in an endeavour to remove them?

A similar observation may be made respecting depressions of bone when complicated with wound of the brain. If the edge of the depressed bone be imbedded in the substance of the brain, it may be proper to restore it to its natural level, provided that this can be readily accomplished with the forceps or elevator. But individuals have recovered, in whom a depression of bone has been allowed, under these circumstances, to remain without being elevated; and it cannot be advisable to risk this chance of recovery, whatever it may

be, if the elevation requires the application of such a degree of force as is likely to cause the most trifling additional injury to the wounded brain. I have myself been led to doubt the expediency of applying the trephine in those cases in which there were no circumstances making the operation absolutely necessary. The motion of the saw must occasion more or less jar to the tender substance of the brain; and this, which may be of little consequence where the brain and its membranes are entire, may make a serious difference as to the degree of danger, where these parts are already lacerated and contused. There is, moreover, the same objection here as in other instances, to the removal of any considerable portion of the parietes of the cranium, namely, the liability which it occasions to the formation of a *hernia cerebri*.

The lodgement of a musket-ball, or other foreign body, in the substance of the brain, is undoubtedly a very serious occurrence, and one attended with the greatest danger to the patient. If the foreign body be of such figure and dimensions, and so situated, that while one extremity of it is inclosed within the cavity of the cranium, the other extremity projects externally, it may of course be extracted, and, probably, ought to be extracted at all risks. But with respect to a musket-ball or pistol-bullet lodged in the brain, it may be observed, first, that it rarely happens that it can be discovered and extracted even by the lightest and most practised hand, without such a degree of violence as must be in itself sufficient to produce a train of evils, which in all probability would terminate in death: and, secondly, that there are numerous instances of persons who have recovered, although the ball was allowed to remain in the brain; some of whom have suffered no more than they would have suffered from its being lodged in a less important part of the body. Taking all these things into consideration, ought we not to regard it as the general rule, that the extraction of a ball should not be attempted; an exception to the rule being made only in those cases, in which, from its more superficial situation and other circumstances, the extraction can be easily accomplished without the employment of force, and without adding in any degree to the mischief already done?

On the whole (according to the view which I am led to take of the subject,) there seems to be in the very great majority of cases of wounded brain, more wisdom in resorting to negative, than to active local treatment. At any rate, as the restorative powers of the animal system are on all occasions the principal agents in the reparation of mechanical injuries, we cannot be wrong wherever there is a reason for doubt as to what should or should not be done, in leaving nature to take her own course, in trusting to her efforts rather than to human science and art.

My own experience, as far as it goes, is in favour of what is here recommended. I do not mean, however, to assert that what I have seen of cases of wounded brain is in itself suf-

sufficient to justify me in forming these conclusions, unaided by a general knowledge of disease, and by arguments derived from analogy. In fact, the cases of wounds of the brain, which occur in the routine of a civil hospital, are so few in number compared with those of other injuries of the head; they exhibit such numerous and various complications; and the proportion of recoveries from such wounds, whatever system is pursued, is so small (especially among adult patients,) that it would be bold of any surgeon, engaged in the ordinary duties of his profession, to declare that he had been able to make a comparison of the different modes of treatment on such an extended scale, as would enable him to lay down rules of conduct founded wholly on his own practice and experience. The opportunities of military and naval surgeons must be, at certain periods, more considerable, but the circumstances under which they occur are very unfavourable to that minute observation and accurate judgment, which would be necessary to enable them to derive from their opportunities, all the advantages, which they might otherwise afford. Where the experience of individuals fails, we are called on to look for other sources of information. I have referred to all the cases of wounded brain recorded in the works quoted below,\* and the general results which they exhibit will be found not uninteresting, if viewed in their relation to this point of surgical practice. These cases are thirty-eight in number, of which twenty-six terminated favourably, and twelve unfavourably. This, of course, affords no information as to the actual rate of mortality in cases of this description, the fatal cases being for the most part regarded as too much a matter of course to be worthy of publication, while a very different opinion is entertained respecting the cases of recovery. But the following facts afford some useful information as to the circumstances under which recovery takes place.

In nine cases of wounded brain in which the bone was fractured, but not depressed, no operation whatever was performed. In two of them the patients died; in the remaining seven they recovered.

In fifteen cases no operation was performed, beyond that of removing some splinters of bone with the forceps. In five of these cases the patients died, while in ten the patients recovered.

In four cases the wound of the brain was complicated not only with fracture, but with depression of bone. In one of them in which

the depressed bone was allowed to remain without being elevated, the patient recovered. In the three remaining cases the depression was elevated with the assistance of the trephine; and one of these patients recovered, and two of them died.

In ten cases a musket-ball was lodged in the brain. In two of them the ball was extracted, and one patient recovered, while the other died. In the remaining eight cases the ball was allowed to remain, no attempt being made for its extraction, and two of these patients died, while six of them recovered. Of these last, however, one died several weeks afterwards of inflammation of the brain induced by intemperance in drinking, and another after having been sufficiently well to resume his duties as a soldier, died in the course of the following year, of what was regarded as a *coup de soleil*.

It appears then that in fourteen out of twenty-six patients who recovered, no operation whatever was resorted to, and that in ten of the remaining twelve, there was no operation beyond that of removing splinters of bone with the forceps. Of those in whom a ball was extracted from the brain one died, and one recovered; and of those in whom the ball was not extracted two died, and six recovered. It is needless to add, that the conclusions to be deduced from these statements illustrate and confirm the observations which have been already made as to the principles which should direct the surgeon in his treatment of these formidable injuries.\*

There is one circumstance connected with this subject, which is too important to be passed over in silence, and which may very properly be mentioned in this place, as it must very materially influence us in the opinion which we give, at the time of the accident, as to the probability of the patient's recovery. I have not been able to discover, among all the works which I have consulted, a single

\* Since these calculations were made, a very interesting case has been published by Dr. Rogers in the thirteenth volume of the *Medico-Chirurgical Transactions*,† in which the breech-pin of a gun was lodged in the anterior lobes of the cerebrum, and extracted at the end of twenty-eight days, the patient afterwards recovering. Here the foreign body remained in the substance of the brain until inflammation had been going on for an entire month, so that the brain and its membranes must have become extensively agglutinated and consolidated around it. The question as to the extraction of a foreign body after such a lapse of time, and under such circumstances, belongs more properly to another part of these inquiries; my observation at present being intended to be confined (as nearly as that can be done) to the treatment to be employed immediately or soon after the occurrence of the injury.

† *Vide Journal of Foreign Medicine*, Vol. I. page 213.

\* *Mémoires de l'Académie Royale de Chirurgie*.—*Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge*.—*Duncan's Medical Commentaries*.—*Duncan's Annals of Medicine*.—*Edinburgh Medical Journal*.—*Medico-Chirurgical Transactions*, Vol. I. to Vol. XII. inclusive.—*Le Dran's Observations in Surgery*.—*Hennen's Military Surgery*.—*Collection d'Observations Cliniques par M. A. Petit*.

instance of recovery from a wound of the posterior lobes of the cerebrum, of the cerebellum, or medulla oblongata; and in the great majority of cases in which a cure has taken place the injury has been confined to the frontal bone, and that part of the brain which is covered and defended by it.

SECT. 12.—*On the Treatment of some other Cases which are not included under the foregoing heads.*

In those cases in which a particular class of sensations is destroyed or impaired,—as where deafness, or a loss of smell or taste follows an injury of the head,—I am not aware that advantage is to be expected from any particular mode of treatment, or that the use of any remedies is indicated beyond those which are resorted to in ordinary cases of concussion of the brain. The patient generally recovers the sensations of which the accident had deprived him in the course of one or two years; but his cure is to be attributed not so much to the skill of the surgeon, as to the restorative powers of his own system. All cases however do not prove equally fortunate in the result. I saw a gentleman in consultation with Dr. Francis Hawkins, who had lost his sense of smell in consequence of a blow on the head many years before, and in whom no improvement in this respect had ever taken place. He was not even sensible of the odour of valerian, although he could distinguish the medicine by the taste.

When a patient is affected with furious and raving delirium, blood should immediately be taken from the arm, and, if possible, in a full stream. I scarcely remember a single case, in which delirium of this kind, occurring soon after a blow on the head, did not yield to a copious blood-letting. The patient may very probably relapse into the same state, as soon as the first effects of the loss of blood have subsided, and it may be necessary to resort to the same means a second or third time, before the relief is permanent.

As there is reason to believe that convulsions and furious delirium occur under nearly parallel circumstances, we may expect that the treatment which is useful in cases of the former description, will also be useful in those of the latter. When convulsions occur soon after the accident, blood-letting is undoubtedly indicated. It is not however always easy, when the patient's limbs are thus moved and agitated, to succeed at once in the operation; and in many cases, the convulsions having been once established, they will continue for a certain period, notwithstanding that a considerable quantity of blood has been taken away, subsiding at last spontaneously. In a few instances (as has been already explained) convulsions occur at the expiration of some days from the time of the injury. At this later period they may exist in combination with inflammatory symptoms, which may require the further use of the lancet. But they may also exist independently of inflammation, being aggravated by any additional abstraction of blood,

and subsiding on the patient being allowed to take some more substantial nourishment than that which had been allowed him previously.

A case has been related in a former part of this paper, which will serve to illustrate this last observation.

From the London Medical Gazette.

ESSAYS ON SYPHILIS. By JOHN BACOT, lately Surgeon to the First Regiment of Guards.

[Continued from page 424.]

This, and the following essay, will be principally devoted to an examination of the writings, and a detail of the opinions, of authors of the present day. Nevertheless, it will be my duty not only to detail fairly and impartially the result of their labours, but also, as the occasion presents itself, to make such comments upon their doctrines as they appear to require; and to point out, without reserve, the errors into which it appears to me that some of them have fallen.

I have already mentioned the general state of practice in syphilis at the time Mr. Hunter published his Treatise on that disease: particular points of doctrine were, indeed, the occasional subjects of discussion in different publications and lectures; but practically, no one dreamed of curing the complaint without a course of mercury, still less was it imagined that the symptoms *could* be cured in any other way, although it now is quite certain that on the continent of Europe, and more especially in Germany, the common plan of treatment had undergone a considerable change—that the corrosive sublimate had there become the favourite remedy; but even that medicine was prescribed in very inefficient doses, according to the dogmas of the day, as taught in this country. The Peninsular war, however, opened to the medical officers of the British army new views relative to syphilis, and they lost no time in communicating to the profession the information they had thus acquired. Of these, Mr. Ferguson was the first who published an account of what he had seen in Portugal: his paper is to be found in the fourth volume of the Medico-Chirurgical Transactions. From a perusal of this paper, it is evident that this gentleman considered the conclusions to which he arrived as totally inapplicable to this country, though true as far as they regarded the natives and the climate of Portugal. Mr. Ferguson's opportunities of observing the venereal disease in the Peninsula were very extensive, since he had held the situation of Inspector of Hospitals to the Portuguese army upwards of two years before he wrote his paper, which is dated in May, 1812. It contains some highly interesting paragraphs, which it will be necessary to bear in mind, since they tend in no inconsiderable degree to explain what has hitherto appeared most obscure and difficult of solution in this intricate inquiry. The facts we learn from this paper are principally the following:

—It was customary among the native practitioners in Portugal to cure all primary venereal affections with topical applications only; the native soldiers, as well as those in civil life, were accustomed to perform their duty, and follow their usual avocations, with sores on the penis, not merely such as were of a trivial nature, but such as made Mr. Ferguson shudder to look upon; the only difference in the treatment adopted by the military and civil practitioner in such cases being, that the latter generally combined the decoction of the woods with the local remedies, but in both instances the use of mercury was reserved for those in whom the bones had become affected, when a very small quantity, usually of calomel, was prescribed, together with Dover's powder, warm baths, and other sudorifics. Dreadful examples of mutilation did, indeed, sometimes occur; but these bore no proportion to the number of those who had suffered from the primary symptoms of the disease; and the affections of the bones, when they did occur, were usually slight; thus proving, that in this climate at least, the complaint had become so much mitigated, as to run generally a mild course, until it at length exhausted itself spontaneously.

Very different, however, was the progress of the symptoms in the British army: among the soldiers its ravages were so frightful, that Mr. Ferguson says it is probable that more men had sustained from this cause the most dreadful of all mutilations, during the four years the army had been in Portugal, than the registers of all the hospitals in England could have produced in the last century; so that, not only were the primary sores more intractable to mercury than in England, but also secondary symptoms made their appearance in no small proportion, even whilst the constitution was actually under the influence of mercury.

Such are the principal facts which Mr. Ferguson has detailed. I now come to consider the reasonings he has founded upon those facts. After inferring that syphilis has lost much of its virulence in Portugal, or in other words, has exhausted itself, he remarks that the same change has occurred in the same country with respect to the small-pox, which is permitted to run its natural course unmolested; and so mild has it become, that not one case of fatal termination presented itself to Mr. Ferguson's observation: yet he adds, "I have no doubt that this mild disease, communicated to a tribe of Indians or to a plantation of negroes, or any other class of people, who had never before known the small-pox, would desolate with all the fury of a pestilence wherever it could find victims, and never cease until it had destroyed the whole population." Applying this analogical reasoning to syphilis, he considers the inoculation of the virus of this mitigated form of lues venerea into the constitution of the British soldier, as having produced a disease of more than ordinary violence; and here we cannot fail to observe the effect of early impressions, for Mr. Ferguson remarks, contrary to the direct tenor

of the cases he proceeds to detail, that this new organization of disease cannot be combated by such means as the natives employ, and concludes that mercury affords to the patient the only chance of salvation; yet, strange to say, the detail of a very interesting case teaches us that bleeding, cold lotions, free purging, and the strict antiphlogistic regimen, were the true and efficient means of safety, and not the exhibition of mercury in any shape whatever. The case that calls for this observation is that of an officer, whose penis, four days after a suspicious connexion, became enormously swollen, of a deep red colour, with malignant ugly-looking sores on different parts of the prepuce, and two on the glans penis, which are compared, in appearance, to holes made by a rusty nail in a piece of mahogany or logwood: the general health was also proportionably deranged. The effect of the depletory plan of treatment above-mentioned was magical; but although Mr. Ferguson had no doubt that the violence of the inflammation had superseded the specific contagion, yet, in compliance with old custom and the patient's fears, a mercurial course was afterwards pursued. Another curious circumstance relative to this case must not be forgotten: this officer had been infected by an opera-dancer at Lisbon, who continued for several months afterwards on the stage occasionally infecting others, but without communicating a disease of any peculiar or extraordinary malignancy in any other instance. Mr. Ferguson makes one other observation, which I shall extract, since it is highly deserving of consideration:—"I think it is probable (he says) that, by the resistance we in England have opposed to syphilis and variola, we have retarded their natural decay among us; that we have made both more rare I believe, and that we may finally succeed in extinguishing them I devoutly hope; but whenever we are revisited by either the one or the other, I fear they will not come to us disarmed of their terrors." There are three points in the above narrative which I think ought to be borne in mind, because they are not only of considerable importance in themselves, but because I shall have occasion to revert to them more particularly on a future occasion; they are these—1st, the cure of the officer's ulcers by bleeding, purging, &c.; 2dly, the fact of the same woman communicating a disease of a milder nature to other men; and, 3dly, the conjecture that probably a more severe form of syphilis may at some future time appear amongst us.

Pursuing the course of my history, I have next to mention a very important document, for which we are indebted to Mr. Rose, who, having himself served several years in Portugal, was well qualified to form an estimate of the comparative merits of the two plans of treating syphilis, both Portuguese and English, and who, soon after his return to the Peninsula, adopted the only rational plan—that of putting the question to the test of experiment, discarding all preconceived notions, and looking solely to the natural progress of the

ease when left to itself. The results of these experiments, made in the hospital of the Coldstream Regiment of Guards, during a period of nearly two years, were given to the world in the year 1817. In this publication Mr. Rose announced, that during the above period, he had been enabled to cure *all ulcers* on the parts of generation that had presented themselves, as well as the constitutional symptoms to which they give rise, without the exhibition of mercury. Mr. Roes does not assert that the sores in all these cases were syphilitic; but he tells us, that the battalion in which they occurred consisted of upwards of a thousand men, stationed in London, accustomed to associate with the lowest class of prostitutes, and, therefore, must have afforded (independently of the character of the sores) many undoubted instances of the disease. These, and some other prefatory remarks, are followed by the detail of nearly thirty cases of ulcerations of the genitals, which are divided into three classes: the first includes those not followed by secondary symptoms; the second, those followed by papular eruptions and other symptoms; and, thirdly, of those in which the eruptions differed from the papular form. The only general remark that I shall make respecting the first class is, that the sores were, with exceptions, either attended with much inflammation or sloughing, thus rendering it probable that the rapidity of their progress had superseded the absorption of the poison; a fact of which Mr. Pearson has alluded, in speaking of the efficacy of the cinchona in certain spreading sores on the penis. With respect to the second and third classes, it would seem probable that the occurrence of secondary symptoms was the result of the great length of time that these ulcers had been permitted to run their course, before any plan of cure was sought for by the patients themselves; and this is conformable to the opinion maintained by many medical authorities, that the permanence of the cure, and the security of the constitution, depends much upon the speedy extinction of the virus by mercurial action, where there is nothing in the character of the sore to forbid its use. Mr. Rose's paper concludes with some ingenious reasonings, founded upon the result of this practice; but it does not enter into my views to notice this now: the only conclusion I have to draw from what he has related is, the undoubted fact of every form of primary ulcer on the genitals being curable without mercury; and also the possibility of conquering the constitutional affections that supervene in consequence, without administering a particle of that medicine. During a period of two years, it is to be likewise remembered, that only one or two affections of bones had occurred, in no instance leading to caries. The publication of Mr. Rose's paper made a great impression on the medical public; it excited the curiosity of the profession highly, and stimulated many, who, from their situations as army surgeons, had an opportunity of confirming these experiments by adopting a similar line of

conduct, to repeat them. In the several regiments of Guards this plan had been the object of emulation for some time past: at the military hospitals at Chatham and Fort Pitt, as well as at York Hospital, Chelsea, it was likewise resorted to. And in the same volume which contains Mr. Rose's Essay, is to be found a communication on the same subject by Mr. Guthrie. With that gentleman's reasonings I have nothing at present to do; I quote him solely for the purpose of confirming what had been before advanced relative to the cure of *all ulcers* indiscriminately without mercury. His evidence, then, goes to prove, that for eighteen months Mr. Dease, Dr. Arthur, Dr. Gordon, and the writer himself, had been in the habit of treating all ulcers on the penis, whatever their appearance might be, with simple means only, and they all got well. Mr. Guthrie informs us also, that the same plan was pursued at Dover, Chatham, and Edinburgh, as well as by some regiments both abroad and at home. He had also seen the reports of 400 cases treated in the same manner, and with the same success; though it would seem that in many of these cases the cure was very tedious, and the cicatrices of the sores were frequently giving way. Of the secondary symptoms resulting from these sores the cure was likewise tedious, though they were generally of a mild nature; and only two instances of affections of the bones were met with. Mr. Guthrie next proceeds to contrast the result of his practice with mercury, whilst surgeon to the 29th regiment, between the years 1801 and 1809; and he remarks, that during this period, when his patients generally underwent a moderate course of mercury, he very seldom had a case of secondary syphilis; and he is not aware of his having either lost, or been obliged to discharge a man, in consequence of that disease.

In the half year ending the 24th June, 1817, fourteen hundred cases of the venereal disease were treated in the army of occupation in France with mercury, and only fourteen cases of secondary symptoms occurred; whilst of 521 cases so treated in England, ten instances of secondary symptoms appeared—so that the true average proportion of the two numbers united is 1 in 75; whereas in the mode of treatment denominated non-mercurial, the average number of those affected by secondary syphilis was at first stated to be 1 in 10, though, in truth, this proportion was soon discovered to be very much underrated, and there is reason to believe that 1 in 4 or 5 would have been nearer the truth.

Notwithstanding this, however, the non-mercurial plan of cure was extended by degrees to the military stations of England, Europe, and even America, under the sanction and direction of the present Inspector General of the medical department of the army, who has always been among the foremost in promoting every inquiry in which either the interests of humanity, or the advancement of professional knowledge, is concerned. The result has been a collection of reports connected with

this subject, detailing the cases of nearly 2000 venereal patients, whose symptoms, both primary and secondary, had been treated upon the new system. From this mass of information certain conclusions were drawn, and which were afterwards transmitted to the surgeons of regiments, for their information and guidance. From this circular letter it appears, that between the months of December 1816 and 1817, 1940 cases of syphilis had been treated without mercury, of which number 96 had afterwards secondary symptoms of various sorts. Of these 96 patients, 12 were afterwards subjected to mercurial treatment, chiefly for reasons of expedience, rather than of necessity; and even in these cases it was found that alterative doses of mercury were sufficient to effect a cure with several of them. Of the whole number of primary sores, 65 were cured finally by mercury, in consequence either of the slow progress they had previously made, or from their evincing a disposition to spread; though at the same time we are informed, that the non-mercurial practice, both in the primary and secondary forms of the disease, *generally* occupied less time than when mercury was had recourse to. Such was the result of the number treated without mercury.

In the same period of time, 2827 men, with ulcerations of the penis, were treated with mercury; and of these, 51 only had secondary symptoms: but these last appear to have been extremely severe, and more intractable than when mercury had not been used for the primary sore; so that two men were obliged to be discharged the service, in consequence of the injury sustained by their constitutions. Among the general observations with which this document concludes, we must not omit to notice the discrepancies in the reports from several regiments: thus, in one, four cases of secondary symptoms supervened out of twenty-eight treated with mercury, whilst, in another, sixty-eight men were so treated, and not one example of secondary affection was observed during the space of fifteen months; to which space of time this report extends. It is also asserted, that no peculiar forms of secondary symptoms were fairly traced to any peculiar primary sore; that, in cases treated without mercury, iritis had frequently been met with as a secondary affection—sometimes alone, at others in combination with eruptions of various kinds; and in these, mercury was generally resorted to with success: finally, the frequent reappearance of the primary sore, and repeated attacks of eruption, have most commonly been the reproach of the non-mercurial treatment. Another singular circumstance developed by these returns, is the infrequency of syphilis in the West Indies, compared with its ravages in Hindostan: so striking is this difference, that Dr. Good, who has compared these returns, asserts, that every two regiments in the East Indies furnished, at least, as many cases, both of genuine and doubtful syphilis, as are furnished by the whole army in the West Indies; for example, the whole number, in the year 1823, in that part of the

world, amounted to 36 only, whilst one regiment in the East Indies afforded 177 cases in the same period.

I should be almost afraid of wearying the reader with these accumulated facts, but I feel it my duty to consider this subject as one entirely novel and unknown; and that, as professing to give an entire and complete body of doctrine relative to the disease, I should not feel myself justified in passing by any series of observations on public record, which tends to put this question in a clearer point of view: but it only remains now to give the result of Mr. Hennen's labours, and this part of my subject will be completed. The substance of what Mr. Hennen has detailed may be thus shortly stated;—The first trials of non-mercurial practice were witnessed by this gentleman at the Hospital at Hilsea, in 1816, under the superintendence of Dr. Knox, where, between the months of May and September, out of 58 cases of primary sore, 28 were healed without mercury. It was not, however, until October 1817, that, being principal medical officer in charge of the district of North Britain, Mr. Hennen had an opportunity of trying this plan upon an extensive scale, and he thus sums up his opinion.

"Every thing I have seen of this practice confirms me in the belief of the possibility of healing primary sores on the genitals, of whatsoever description they may be, without the employment of mercury; and I have met with nothing to make me question the propriety of the trial: of some hundred cases, none have hitherto resisted." But farther on, he adds—"Secondary symptoms occur more frequently, and appear at an earlier and more determinate period than when mercury has been used; but they have not proceeded from bad to worse; they do not exhibit the same violent and unrelenting symptoms which we have observed in many instances where mercury has been used; the eruptions have not run into ulceration; they have not formed into large scabs, or extensive blotches, nor have the bones of the nose, or other parts, been affected with caries." All these points are clearly established by several tables, very perspicuously and accurately drawn up.

From the above mass of evidence the following conclusions appear to be fairly deducible:—1st. That all sores of the genitals, without exception, are curable without mercury. 2dly. That secondary symptoms occur in the proportion of at least one in ten of those cases where no mercury is used; whilst on the contrary, the proportion of such cases is only as 1 to 75 where that remedy has been employed. 3dly. The possibility of curing nearly all forms of the secondary syphilitic symptoms without the assistance of a particle of mercury. 4thly. The mildness of these symptoms, which, excepting in about half a dozen instances, were confined to eruptions in the skin, and ulcers in the throat. 5thly. That the period required for the cure of the primary sores by the non-mercurial plan was not in general greater than where mercury was employed; though it is

admitted that the cicatrices of the sores remain frequently in a state of disease, were often ulcerating again, and that the secondary symptoms, though not violent, were very tedious; and when apparently cured, would not unfrequently recur again and again. I ought here to observe, that the practitioners in France had long been in the habit of curing all ulcerations on the genitals without mercury, though they did not pursue this plan in consequence of direct experiment, but from a conviction that, generally speaking, these sores healed more readily by the employment of simple means only, but they were in the habit of prescribing the corrosive sublimate internally, in very small doses, for the purpose of preventing the attack of secondary symptoms; such for many years had been the practice of Cullerier, of Paris, whilst other of their surgeons relied entirely upon diet drinks, of which sarsaparilla formed the basis.

We may now, perhaps, be tempted to exclaim with an anonymous French writer, "there is no venereal disease at all;" and passing from the extreme of timidity to that of confident rashness, be disposed to place the belief in syphilis in the same rank with that concerning the contagion of the plague, and the existence of hydrophobia, as held by some sceptical philosophers of the present day; nevertheless, such a conclusion would, I conceive, be equally premature in either of these cases, for the experience of a few more years, whilst it has left the facts above cited untouched and uncontradicted, has amply shown that the proportion of secondary symptoms, as well as their obstinacy, the slowness and uncertainty with which primary ulcers heal, their frequently breaking out again under the non-mercurial system, rendered it highly inexpedient, and in fact impossible, to introduce this practice into general use; nay, more, in several instances, even among the military, little accustomed to regard consequences, it began to excite uneasiness; the proportion of cutaneous affections, of ulcerated throats, of pains in the larger joints, and other concomitant evils, became a serious evil, and induced many regimental surgeons to remodel their practice, and to adopt a plan of treatment less exclusive with regard to mercury.

Evils, still greater, but which are not fairly ascribable to the above investigations, also arose throughout the country; for the general confidence in the power of mercury having become shaken, if not destroyed, and nothing like fixed principles established in its stead, many practitioners were satisfied with a very trivial or slovenly exhibition of that remedy; it was often given out without any precaution, and the result was, that a few of those who became affected with primary syphilis escaped some after consequence: this circumstance, formerly so rare, soon produced a reaction in the opinion of professional men, and the new doctrines did not fail to suffer in the estimation of those who had at first been among the number of their warmest advocates, and to this day the practice continues in a state of un-

certainty, of which this, I conceive, is no exaggerated picture. Still farther to confirm, and extend this confusion, other circumstances have very much contributed: I allude especially to the inquiries instituted into those diseases resembling syphilis, as well as the recent distinctions drawn by Mr. Carmichael, the direct consequence of which has been, that by endeavouring to distinguish with accuracy the origin of particular ulcerations, and restricting the syphilitic sore to one peculiar form, in relying entirely upon verbal descriptions of ulcers, which no two surgeons perhaps have seen in the same point of view, or in the same *state of their progress*, the practitioner has become involved in a labyrinth of contradictions, and the patient has too frequent cause to lament that his security has been sacrificed to unnecessary refinement.

The direct course of my inquiry now leads me to consider that branch of the subject to which I have just adverted—that is, to diseases resembling syphilis; but before I do so, I would wish to point out the real benefits which are to be practically derived from the investigation into the natural history of the disease of which I have just given you a pretty extended account. In the first place, then, it must be obvious, either that the venereal disease has been sadly misrepresented in former times, or that its symptoms have become much milder, either from the mere lapse of ages, or in consequence of the change which the continued exhibition of mercury from generation to generation has produced. To me it appears very unlikely that our ancestors have made any very gross mistake in their account of the symptoms of syphilis; that occasionally some doubtful affections might be admitted among the number is very probable, but if we take, not the particular opinion of one writer, but the general account of a number of contemporary authors at any period subsequent to the middle of the 17th century, the descriptions they give rather differ from what are now met with in the severity of the symptoms than in their identity; and we must remember, also, to deduct from this account all those consequences which are universally admitted to have been produced by the profuse and very incautious manner of administering mercury, at that time in use; it is, therefore, I think, nearly as improbable that mercury can be allowed to have the merit of having modified or lightened the symptoms. Had its character, as a specific, been indeed so absolute and undeniable as been more than once asserted, we might have expected the disease to have become extinguished rather than modified; whereas, as far as we can collect from authors, or from our own experience, whenever mercury has been given without effecting a cure, so far from the disease having any tendency to become milder, it has been actually aggravated. We are, therefore, reduced to embrace the only remaining supposition, that the progress of time, bringing with it a better and more wholesome mode of living, both with respect to food, clothing, and lodging,

together with much greater cleanliness of person, and a more discriminating and temperate plan of treatment, have been the real and efficient causes of the milder aspect of the disease in these latter days; though, perhaps, after all, we are boasting of what may only be a temporary blessing, for I would suggest the probability, that at those particular periods in which we have found practitioners abstaining from the use of mercury, as, for example, in the days of Fallopius, Abercrombie, and afterwards of Morgagni, and many others before and since, there is reason to suppose that they did so in consequence of having had to treat a milder form of the disease, just as in our own day we have seen one surgeon speaking of the disease in Portugal as very severe, whilst a few years later that severity was not recognised; and still later it has been observed, that in Ireland primary sores of great malignancy have been met with in some seasons which have been unknown at others; therefore, whilst I admit the fact that syphilis is much milder now than formerly, that is, I mean within the memory of practitioners now living, yet I think it by no means impossible that this condition of things may not endure, and that more severe forms of the disease may again become prevalent, in the same manner that the small-pox epidemic shall remain mild and mitigated for some years, and afterwards return to us with renewed violence. However this may be, it may fairly be asked, granting that the disease is now mitigated in severity, what has the profession gained by the experiments above mentioned? and to what practical purpose can they be applied? My answer would be, in this point of view they are invaluable, since they have shown us that we may safely, nay, advantageously, dispense with the use of mercury upon all those occasions wherein we discover, or suspect that it is operating deleteriously upon the constitution. Whenever fever is excited, or pains, either local or general, are induced, without apprehending any of those formidable consequences that used formerly to alarm the surgeon as well as the patient, we may await patiently and tranquilly the favourable moment for exhibiting the medicine; we may apply to the ulcers on the genitals the same principles of cure which would be applicable to sores on any other part of the body; nay, more, in those constitutions prone to struma, we may confidently forbear its employment, or when necessary to do so, we may prescribe it either in so mitigated a form, or under such combinations, as to disarm it from all those dangers which occasionally render its exhibition a cause of more real suffering than the disease itself; and yet let me not have it imagined that I am one of those who recommend the exclusion of mercury from practice in the venereal disease; on the contrary, it is my object to prove that in the vast majority of cases it is our sheet anchor.

Those who recollect the summary manner in which all breaches of surface on the parts of generation, were, at no great distance of time, condemned to mercurial treatment,

without any reference either to the condition of the sore or constitution; the frequency with which sores so treated were accustomed to inflame and spread, instead of healing; the fever that was occasionally lighted up; in short, the combat excited between the powers of nature and a mistaken line of practice, may be inclined to wonder that no author, prior to Mr. Hunter, should have attempted to draw any distinction between the different species of ulceration met with on the parts of generation; the more especially, since the fact of some of them being aggravated by the use of mercury, was at that time universally admitted, and acknowledged to be a conclusive proof of the nature of the affection.

From the time of Mr. Hunter's publication, then, a new page of our history may be said to be opened; until then syphilis was not doubted to be one disease, and all the variety of symptoms were attributed to one poison; but from that date a new host of diseases became acknowledged and admitted into the catalogue of human woes; these were said to resemble lues in appearance and progress, but yet they were thought not to be syphilitic. This, then, is the next subject that demands our attention, for this is in truth the foundation upon which Mr. Carmichael has built his theory of a variety of syphilitic poisons.

Now, although I am inclined to admit that good has in many respects followed the investigation thus commenced by Mr. Hunter, and that many complaints, which were formerly confounded with syphilis, have since been discriminated from it, and some progress made towards a more accurate classification of the symptoms, yet it cannot fail to be observed that much of the reasoning employed by Mr. Hunter, and subsequently by Mr. Abernethy, relative to diseases resembling syphilis, falls to the ground, since the fact of all forms of primary ulceration being curable without mercury has been admitted; for all their distinctions are built upon the converse of that proposition, and with regard to the term pseudo-syphilis, first employed by Mr. Abernethy, I must beg to observe, though perhaps the remark is rather out of place here, that I consider it as a term most unfortunately chosen, since it cannot fail to lead to a confusion of ideas, and as long as it is employed must rather tend to prevent than facilitate a discrimination so much to be desired, for these diseases are either syphilitic or they are not; and, therefore, at once to assert they are not so, and yet to employ a term that brings the actual name of the original disease to the mind, cannot fail to create and perpetuate confusion. But to return from this digression. Now, although the belief of the existence of diseases simulating lues venerea is repeated by almost every modern writer upon this subject, so that Dr. Good has even given them a distinct place in his nosological arrangement, I do not hesitate to declare that I do not believe in their existence, and I cannot conceive that we are justified in drawing any such marks of distinction now that we have seen that syphilis itself, ac-

knowledge and undoubted syphilis, under all its forms, is curable without mercury. When that fact was either unknown, or denied, it certainly became necessary to seek some escape from the dilemma which occasionally presented itself on finding certain symptoms, so similar to those of syphilis as not to be distinguishable from it by the senses, getting well either with sarsaparilla or without it; or again, other symptoms aggravated instead of being cured by the action of mercury. But surely we have now learned, by the thousands of experiments that have been made in this country and on the continent, that this distinction is not founded on facts, that all forms of syphilis may get well without one particle of mercury, and that under peculiar circumstances, that mineral may act as a poison, although the disease for which it was prescribed was undoubtedly syphilitic.

There appear to me to be three questions connected with this branch of my subject, which it would be very desirable to decide:—1st. Whether it is possible to ascertain by the appearance and progress of the ulcers on the genitals, if they be the produce of impure connexion or not? 2dly. Whether breaches of surface on the parts of generation not produced by sexual connexion, are ever known to be followed by constitutional symptoms of any determinate character? and 3dly. Whether sores acknowledged to be the result of impure connexion are regularly and invariably succeeded by peculiar trains of constitutional symptoms, having constant reference to a peculiar form of ulceration?

Towards deciding either of these three questions I am afraid it must be admitted that Mr. Hunter has not done much; he has certainly the merit of having first opened the road to future inquiries, but the cases he has brought forward in support of his opinion admit of a very easy solution now, and demonstrate the very rapid strides which have been made of late years in the knowledge of this class of diseases. In order, therefore, to trace the progress of this inquiry, it will be necessary for me to mention, shortly, the principal facts which Mr. Hunter has adduced in support of his views relative to diseases resembling syphilis, and we must recollect that his observations do not apply to herpes of the prepuce, to common phlegmon, or to erysipelas, which may attack the parts of generation as well as any other portion of the body, and of which affections he treats separately. Mr. Hunter commences by remarking that many diseases resemble each other in one or two of their symptoms; and that, therefore, in order to draw a just judgment, the aggregate of the symptoms should be considered, and this observation he deems more applicable to the venereal disease than any other, since he conceives that it has no one symptom peculiar to itself; and this he attempts to illustrate by the example of a gonorrhœa; but the most remarkable passage relating to this question is the following:—1st. That sores on the glans penis, prepuce, &c. in form of chancres, may and do

arise without any venereal infection; and again, other disorders shall not only resemble the venereal in appearance but in the mode of contamination, proving themselves to be poisons by affecting the part by contact, and from thence producing immediate consequences similar to buboes, also remote consequences similar to lues venerea: the inference, however, which he draws from these two positions leads us to the belief that the only criterion he admitted between a venereal and a non-venereal disease, was the possibility of curing one of them by mercury, and that whenever it happened that the symptoms went from bad to worse under its use, he supposed that he had been mistaken in the nature of the case. That this is a plain statement of the fact the relation of a few of his cases clearly demonstrates; the first is that of a gentleman in the West Indies, who having a wound in his finger, opened the abscess of a negro woman who was labouring under the yaws, and was conscious at the time of having inoculated himself; he had recourse to mercury, but in spite of it successive tumours formed over the hand and up the arm; in a month or two nocturnal pains came on, with other distressing symptoms, which persisted, although he used mercurial frictions for five months; afterwards, at the distance of half a year, a scabby eruption appeared over his legs, and his tumours ulcerated: the nocturnal pains being then mitigated, he never could bring on salivation, though the mouth was tender, and he arrived in England about two months later, where he obtained a cure by the use of mercury and sarsaparilla conjoined. You will perceive at once that this is not a case of the venereal disease, and has nothing to do with the question; the disease was the yaws, and ran its course in the manner usual with that complaint.

The second case is that of a gentleman, who, after undergoing a course of mercury for the cure of chancres, was restored to health in five weeks; he almost immediately had connexion with a woman; in a few days the prepuce appeared as if chapped all round the edge of its reflection. The connexion was, notwithstanding, continued, and the patient applying at length to Mr. Hunter, the chaps or fissures were found to be very deep, and paraphymosis had taken place. In this dilemma, Mr. Hunter considering the case not to be venereal, sent the patient into the country, and his sores all got well without any thing being done for them; but a fortnight afterwards the lady became ill, and after a slight fever had a swelling in the groin; its progress was slow, but it broke, and as it showed a disposition to heal, Mr. Hunter did not consider it as venereal; but at the end of six weeks, when it was perfectly well, eruptions came out on the skin of the face, thighs, hands, and feet. This staggered Mr. Hunter a little, but they got well, although nothing was done. Surely this is a case about which we should not be much puzzled now: a man excoriates himself violently, he continues to have connex-

ion, he becomes infected, the female in a very short time proves herself to have been infected by the appearance of a bubo: it is not even hinted that an examination took place to discover whether ulcerations in the pudenda existed or not, and in truth the whole curiosity of the affair is, that all the symptoms got well without mercury.

The third case is simply one in which the patient's health (he was a man of intemperate habits) was much affected, so that on prescribing mercury for a sore on the glans penis, attended with excessive pain, it was found to disagree, and the sore was finally healed by cinchona, sarsaparilla, and opium. This was followed some months after by a tumour of the scalp, and succeeded by an extensive caries of the cranium, attended with excessive pain; these sores healed up, and others ensued, which all got well, excepting that for a long period one large ulcer at the angle of the right eye remained unhealed, so that in this case also there was nothing but what the recent experiments above recited render perfectly intelligible; for here was evidently an irritable habit of body, which, combined with an improper use of mercury in the first instance, produced a hybrid disease, which has in most respects more the character of struma than of syphilis, and which indeed receives a very rational explanation in the following passage of this author's own work: "The venereal disease often becomes the immediate cause of other diseases, by calling forth latent tendencies into action." It is, therefore, I think, very evident that Mr. Hunter leads us but a very little way towards the solution of either of the questions above proposed, but a much more ample field opens upon us when we come to examine the works of Abernethy, Evans, and Carmichael. This task I reserve to my next essay.

(To be continued.)

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ON THE SANGUINEOUS TUMOUR OF THE FEMALE BREAST. By JOHN RODMAN, M. D., Paisley.

The disease which forms the subject of the present paper is one in which the medical profession must be much interested. Peculiar apparently to the female breast, it assumes a very anomalous character. From its locality it may be referred, according to Cullen's Nosology, to the class *Locales*; and from certain circumstances to be mentioned in its history, it may be thought to rank in the order *Tumores*. But it presents several peculiarities in its morbid structure and general history which entirely exclude it from a legitimate place with any genus or species otherwise distinguished in this arrangement.

Neither am I aware of any other mode of classification yet proposed in which it can be satisfactorily included. Even the attempts made to refer it to the class of encysted tumours, (*tumeurs enkystées, loupes, cystides*),

the contents of which are surrounded by a bag or cyst, have been found insufficient for embracing it, encysted as it is, from difficulties which will fully yet appear. To Mr. Abernethy's method the same difficulties apply; so that the principle of arranging tumours from their anatomical structure is in this instance inadmissible.

The usual seat of this disease in the female breast, and its resemblance to several of the malignant affections incident to that gland, must make it always be viewed with a suspicious eye. This difficulty in distinguishing its nature is augmented by the uncertainty of the principles on which its treatment has been hitherto conducted, and renders it of the utmost importance to the practitioner to determine what method of management is most likely to be, if not favourable, at all events least injurious. The opportunities which I have enjoyed of observing its disposition and character in different stages of progress, and some experience in its treatment, have led me to pay attention to its peculiarities; and it is with the hope of contributing to the alleviation of a malady accompanied not only with bodily pain, but much mental anxiety, that the following observations are now communicated to the profession.

The first distinct examples of this malady were published by Dr. Monro Primus in the fifth volume of the Edinburgh Medical Essays and Observations, under the title of "Histories of Collections of Bloody Lymph in Cancerous Breasts," with the reason for giving them publicity, namely, "no mention being generally made by chirurgical writers of a collection of bloody lymph in the breasts of women, as a consequence or attendant of the schirrous tumours of those glandular parts."

His second and third cases were of middle age, the first was about fifty, and the last about thirty-seven. In the first case, the tumour on the exterior of the breast was large; in the second, it was increased to a great bulk; the third, increased considerably; and the breast of the fourth was of itself very large. Of the first, the tumour was hard and unequal. The patient of the second had been two years sensible of a hard tumour. The whole breast of the third seemed to be schirrous; and that of the fourth was hard. The discharge from the first and second was bloody water, from the third blackish-red lymph, and the fourth bloody lymph. The first broke after the use of cataplasms; the second was opened, and another incision made next day; the third and fourth were opened with a very small lancet. The breast in the fourth case was amputated; and that of the second was after death dissected, and found to be an empty bag without any tumefied gland in it. Three died, and one was abandoned as hopeless.

The physical characters of the tumour varied a good deal. The second case had turgid cutaneous veins and cuticular redness; the third very large and varicose veins, the integuments at last turning red; the fourth had varicose veins of the skin, and the nipple shrunk

out of sight; and, with respect to painful sensations, the first and fourth cases had sharp, the third pricking, the second lancinating pain, in the last more violent after being opened, with supervening gangrene.

The inferences which these cases suggest I shall consider afterwards. Meanwhile, the analogy of Richter's case, (*Observ. Chirurg. Fasc. 3*; also *Trans. by Spence*;) and of one by Mr. James Briggs, Edinburgh, with the cases recorded by Dr. Monro, is such as to render it proper to insert them ere entering upon the proposed considerations.

Richter commences by stating, that "a lively healthy woman, 60 years of age, with a good appetite and digestion, used formerly to observe at the time of her menses a hemorrhage from the *mammæ*. She had never had children. After the cessation of the menstrual flux, the hemorrhage from the breasts still continued for a time, only it did not appear so regularly at certain periods as before.

"But at last another phenomenon made its appearance without any particular cause. The whole breast began to swell, and finally increased to a great size. It did not feel hard, but elastic, and equally so in every part. Fluctuation was nowhere to be felt. With regard to schirrus, of which I had been so much afraid, I was now perfectly at ease. Neither was the breast upon the whole painful.

"At last a place at the side of the nipple became very prominent, tense, and red, which, however, was not in the least painful, and after awhile threatened to burst of itself. I opened it with a lancet. To my great astonishment not a drop of purulent matter appeared, but only a great quantity of blood, partly coagulated, partly fluid, partly black, and partly pure red.

"As, on account of the great quantity of blood, some of which was apparently fresh, the great age of the patient, and surprise occasioned by the unexpected sight of the blood, I had reason to fear a syncope, and great debility, I stopt the discharge, and covered the wound.

"For four days I removed the bandage twice a-day, and each time a quantity of blood of the above description issued out. After this the discharge changed to a reddish watery ichor. The preternatural swelling of the breast was now gone. The schirrus had remained unchanged during the whole process.

"The ichorous discharge continued for six weeks; but at last it became more purulent, and the schirrus gradually lessened, till at the end of that time it was entirely gone.

"The abscess then gradually contracted, and at last turned into a small, narrow, shallow fistula, which remained open many years, indeed as long as the patient lived. It generally discharged only a small quantity of ichorous fluid, but sometimes a considerable quantity of pure blood.

"As the woman with this discharge felt herself otherwise well, and as I was afraid that an evacuation to which she had been accustomed for so many years could not be stopt

without danger, I had not the courage to attempt any thing decisive in order to obtain a complete cure."

Mr. Briggs' case of sanguineous tumour of the breast, which is dated 1810, occurred in a single woman, aged 45, and extended from near the nipple towards the axilla; was stated by her to have eighteen months before originated like a small moveable kernel under the skin; and was sometimes painful on the arm pressing upon it during sleep. Fluctuation perceived for three weeks.

Being punctured, four ounces of dark and inodorous bloody fluid abounding with serum, and not coagulating spontaneously, were discharged. Repeated discharges of blood by the orifice bursting open at intervals for thirty-one days, and becoming more frequent and profuse, induced her to submit to the removal by excision of the diseased part; and in about six weeks from the time of the operation the parts were perfectly healed. This woman continued in apparently good health for eight years afterwards without any appearance of relapse of the local complaint, and died of sudden illness in 1818.

"In the centre of the tumour, when examined, a cavity was found capable of holding three or four ounces of fluid. The inner surface presented a number of rugæ or irregular folds, not unlike those of the corrugated inner surface of the bladder, or the auricles of the heart, forming small interstitial cells or foveolæ communicating with the principal cavity. The entire parietes of the cavity, which were about one-third of an inch in thickness, as well as the small cells formed in the sides of it, were found to consist of depositions of successive layers of coagulable lymph. A small portion of the glandular part of the breast had undergone some change in its texture, but had no communication with the cavity before mentioned; and the change probably arose from pressure, in consequence of its contiguity with the principal diseased part."

The similarity of the principal phenomena in these two cases and the four preceding them, will entitle us to consider them as one and the same disease. That there is an appearance of diversity in some of the collateral circumstances, however, will not be denied. This appearance, therefore, lest it lead indirectly to any difference of opinion, renders a fuller investigation of the disease, and more extended acquaintance with its general characters indispensable. This purpose I shall endeavour to accomplish, first, by laying before the reader several cases of the disease; and, secondly, by attempting a general description of its progress and phenomena.

*Case 1st.* An unmarried lady, near the age of 60, at a distance from me of more than eighty miles, while she was amusing herself in the garden, placed her foot upon the iron head of a garden hoe, by which the small transverse wooden handle struck her on the left breast. This happened in the summer of 1813. The pain at the time was sharp, but moderate, subsiding in the course of a quarter of an hour.

She soon banished it from her mind, and thought no more of the subject till the following December, when she discovered unexpectedly some hardness in the body of the breast. Next morning a physician of extensive experience, who was consulted, ordered a leech and Goulard embrocations, considering it of little consequence.

I saw her for the first time in 1817. The tumour was fully an inch and a half in diameter, and had attained what I shall afterwards distinguish as the third stage. She had enjoyed a vigorous constitution, and possessed a lively, acute, and active mind, with very superior information; but her general health was greatly impaired. The history given me of the second stage did not precisely correspond with that which is related in this paper. A considerable variety of external means, by different advices, had been employed with a view to suppuration.

For a long time in the first stage there was no pain, and little or no sensible increase of swelling in the part. It was only within the last six or eight months that she had at times been distressed with the suspicion that the disease was cancer.

My first object was the restoration of her general health, which seemed of the greatest importance. For that purpose, better regimen and a more methodical and steady attention to due exercise in the country were prescribed. Besides attending to these prescriptions, she was directed from time to time to persevere in the use of tonics from the mineral and vegetable kingdoms, especially the former, or those of a chalybeate nature variously prepared; likewise carefully to watch over the alvine dejections, and to use appropriate aperients more regularly, in order to rouse the excretory functions from a state of habitual and neglected inactivity.

For the tumefaction of the breast various instructions were at different periods requisite. The liniments were diversified according to circumstances, in different degrees of strength, in different proportions of camphor, or camphor and opium, and similar remedies, with which, in the directions transmitted, they were frequently compounded. Moderate pressure, by means of thin sheet-lead properly applied and retained by bandages, was also from the first recommended, in consequence of many years experience that this was occasionally useful in certain varieties of mammary tumours.

This lady recovered her health and strength considerably, and for nearly two years the disease neither gained ground, nor seemed to differ materially from its state at my first inspection. I saw her last in April 1819.

A little prior to this, while enjoying herself with some friends, she received from extraordinary and very active exertions, an injury, which caused the tumour so speedily to increase in bulk, that before I could see her it had burst, and was discharging its bloody contents. Her medical friend expecting, as in some abscesses, to accelerate reunion, en-

larged the sore by incision. In this, however, he was disappointed; for after employing divers judicious means, he failed to excite the healing process; and the patient gradually sinking from the continued discharge of the open sinuous cavity, died about eighteen months after, without any painful or distressing sufferings, as I was informed, beyond the exhausting effects which accompany protracted ulceration.

*Case 2d.* While a widow lady, distinguished for literary talent, was alighting from a carriage soon after the summer of 1816, the hand of the servant, while assisting her, pressed incidentally upon the right breast. The pressure occasioned a degree of uneasiness for some hours. She was at this time about fifty.

My attendance commenced in July of the subsequent year, when the disease was advancing to the latter periods of the first stage. She was subject to dyspepsia. The treatment was similar to what is mentioned in the preceding case; and till the middle of 1822, the disease, in place of making progress, appeared by the means that were employed to be rather receding, and gave no pain; nay, almost no trouble.

At this time, in consequence of falling from a horse, the clavicle of the same side was fractured. From this injury she recovered in the usual time, no direct impression upon the breast having been perceived. Yet it felt more tender, more sensitive, and more easily affected after this accident.

By the end of March 1824, the tumour was so much enlarged, that its central diameter was equal to two inches and a half. She now came to reside in my neighbourhood, and for the first time I emptied it of its dark, grumous, bloody, and variegated contents, by introducing in a lateral direction an instrument resembling the couching needle. The aperture uniting and soon closing up, I kept it open by introducing night and morning the sharpened point of a silver probe for twelve days successively, during which the cyst contracted, and the integuments had become in feeling and appearance almost natural. In this way I was able to remark that the discharge varied on different days, and at different periods of the same day; being sometimes thick, clotty, and dark coloured blood, sometimes oily, yellow, and watery, like serum, according to the corporeal exertions of the patient.

She returned home, and without enjoying, as formerly, the opportunities of attending to the necessary treatment. I had occasion from the re-accumulation of the fluid contents afterwards to pierce and evacuate the tumour four times, twice in May of the same year, once in June, and once in August. At three of these times it appeared to me proper to treat the complaint on the principles of treating hydrocele, by emptying the sac, and then injecting port wine by the small or narrow aperture. The first time the wine was scarcely equal to the quantity of water that was mixed with it, for I feared the possibility of high inflammation; but neither pain of any consequence nor

inflammation ensued. The second time the quantity of wine was equal to that of the water, and the third time it was a third more. The pain excited by this injection was such for a little as to occasion a short nervous paroxysm; and the local inflammation was more obvious than ever before, but by no means excessive.

Five weeks after this, the cyst felt contracted like a small moveable body somewhat of a fleshy firmness, so that hopes were entertained that a cure was effected. Yet the fluid again collecting about the close of that year, I committed the case to the care of another practitioner, by whom, in February 1825, the breast was extirpated.

It was filled at the time, as he informed me, with the usual contents; and on inspecting the amputated breast, the fluid was discovered in two large cavities, having capsules or cysts exceedingly thin and delicate. It appeared during the operation that there was but little remaining of the glandular part; and with this was mixed a small portion of pus, not unlike that of a strumous nature. The structure of its body was formed chiefly of adipose and cellular substance—there were no varicose or distended blood-vessels, or schirrous tumours, but six separate spheroidal bodies of the size of garden peas, filled with limpid fluid, with some appearances of a spongy nature nowise redder than the adjacent parts.

The lady has now been nearly two years in good health.

*Case 3d.* The third case is that of a lady, the mother of a numerous and healthy family, herself peculiarly active, and favoured with an uncommon share of excellent health.

Her attention was first directed to the examination of the right breast, by observing appearances, time after time, of blood upon the linens. At length she discovered it issuing from the nipple; and in the course of some months perceived extending from its base a firmness of substance.

I saw her not long after this in May 1819, when her age exceeded seventy-six. No distinct or insulated tumour was then perceptible. The body of the breast was well formed and of good bulk; atlantal and dextral to the base of the nipple there was circumscribed tumefaction of some degree of firmness, accompanied with scarcely any uneasy sensation. The nipple indeed had been frequently very tender, tumid, and painful; and when most painful, it was seemingly sufficient in its inflamed and irritable state to account for this adjacent swelling, which was invariably then most considerable. It seems not unlikely, from several circumstances, that the nipple and anterior of the breast at some former period sustained external injury, of which she retained no accurate recollection.

The disease was only in its commencement, or merging into the first stage.

The morbid state of the nipple and adjacent parts became for a long time the main object of medical treatment. This embraced preventive as well as curative means, to the pro-

per comprehension of which it is only necessary to state, that whenever she was affected by exposure to cold, which not unfrequently revived pains of a rheumatic kind of the same side, the nipple became disposed to the discharge of blood, appeared inflamed and tumid, feeling sore and tender, especially if, as often happened, the bowels were at the same time more than ordinarily constipated. The regulation of diet, of exercise, of temperature, and the exhibition of appropriate laxatives—neither drastic, however, nor mercurial—besides the topical applications enumerated in former pages, were therefore most obviously requisite, and, in conjunction with strict care in the mode of dressing, were attended with very happy effects.

Such was the tardy nature of the disease under this treatment, that it was not till a short while previously to October 1822, that it attained the second stage; and the third stage was only so far advanced in April 1824, as, after some objections and hesitations on the part of the patient, rendered it necessary to urge her to have the tumour pierced, its central diameter being about two inches.

This I effected in the same manner, and by the same instrument, as related in the case preceding. The discharge consisted of dissolved blood free from any fætor, of a glutinous, clammy nature and dark brown colour, with oleaginous globules on the surface—no pure blood.

After discharging the contents frequently, as in the history of the above mentioned case, for three weeks, the wound, which generally closed in a few hours, being for some days united, the parts having assumed a very natural appearance, and she being without pain or other complaints, except from the tenderness of the nipple, nothing more than precautionary treatment was farther required for thirteen months.

About the conclusion of that period the sac was again filling; for some time previously another tumour, small, yet similar, had made its appearance on the opposite side of the breast. I found it therefore necessary to pierce the sac a second time in May 1825, when, as in the case before referred to, I also had recourse to an injection of equal parts of wine and water; but without exciting either the pain or the inflammation which was wanted.

It was on the 13th of May these steps were taken; and yet there was no abatement of bloody effusion within the sac from this period till the 12th of the ensuing December. On the 15th of May, I was obliged again to commence the process of drilling with the probe, as lately noticed, in order to open the tender aperture, and evacuate the dark coloured venous blood with which the sac was distended. Indeed, so frequently was this process afterwards required, that I was under the necessity of performing it eighty-three times by the day of December above stated; so that, by calculating the bloody and serous discharge at the average of three ounces each time, although its quantity was often greater,

the amount of the whole for these seven months is believed not to be much short of twenty-one pounds, or nearly three gallons.

The weather of the latter date was particularly damp, cold, and chilly. Her apartment had been suffered to become unusually cold during the night; and she was suddenly attacked with rigours, and subsequently with fever of a rheumatic tendency, which increased for three days, and was only subdued at the end of a week.

In less than two days the whole breast became swelled and inflamed; and as the sac continued to fill, I was obliged to have recourse again to repeated perforation with the probe. For three days at first the discharge was a mixture of blood and pus, but almost ever afterwards it was wholly purulent. The evacuation of this matter always relieved the local disease, and therefore I continued the use of the probe in opening the same aperture for that purpose, night and morning, nearly six weeks, the quantity of matter discharged towards the end becoming less and less.

In the beginning of the following February, seldom did any kind of discharge take place from the use of the probe; for its use was persisted in till the breast was thoroughly healed. During the continuance of the purulent, as well as the sanguineous discharges, every possible means were used to support the patient's strength. This was accomplished by prescribing variations of palatable and nutritious diet, as well as frequently repeated, but moderate, doses of vinous or spirituous liquors diluted, along with various preparations of cinchona.

Her health was now so completely restored, that the breast having assumed its natural and full appearance, without ulceration, she was able for four months to go about pretty freely, to ride in a carriage, and even in a considerable degree to employ herself in several active occupations.

Early in the following summer, she was once more attacked with fever of the rheumatic type, from which she recovered; then towards the end of August with a species of cholera.

Although she recovered from the main symptoms of this last disease, her strength was so exhausted by it, that she was afterwards in great measure confined to bed. Great debility was her chief complaint; and on the 24th of September of the same year, 1826, she suddenly and unexpectedly expired, having passed her eighty-fourth year, and lived nearly nine years from the commencement of the breast complaint.

Case 4th. It may not be improper merely to advert to another case of encysted tumour in a lady of about thirty, and a constitution more than commonly vigorous. When I first saw her she had observed the tumour of the right breast, then nearly an inch in diameter, above fourteen months. All the symptoms indicative of its merging upon the third stage were very manifest. Her youth, health, and

vigour appeared favourable, however, to the hope of promoting suppuration. This was accomplished by employing the usually internal means of invigoration, and external supplicative remedies for seven weeks, during the greater part of which she was persuaded to remain for that purpose mostly in bed. At the conclusion of that period the part was opened, and I had the happiness of seeing it healed up in little more than the usual time.

From all the circumstances in the survey of the four cases of Dr. Monro, it is natural to ask, is each of the discordant symptoms to be considered as necessary to constitute the specific character of the disease? Part of the morbid results no doubt succeed the mode in which some of the cases were treated.

Although the tumefaction be considerable, its precise magnitude is uncertain; and this peculiarity is another perhaps of the instances of the imperfection of every classification of tumours which has made its appearance. It is only when a cyst has been formed in the mamma, which contains a fluid partaking less or more of sanguineous constituents, along with other circumstances applicable to the disease, that the tumour can be regarded as that collection of bloody lymph in the female breast, which distinguishes the disease under consideration.

It is observed by Mr. Benjamin Bell, that "It is not what a tumour may eventually become, but what it is on its first appearance, that can admit of any description." Yet the tumour of which we are speaking is one which must be an exception to such a rule. Its first appearance, when compared with that of encysted tumours generally, would on no account justify the conclusion, that, at a future period, it will become an encysted tumour containing nothing but fluids of a bloody consistence.

It is only necessary, in proof of the difficulty of concluding from first appearances, or symptoms most obvious at the commencement, to refer again to the cases Dr. Monro has recorded. In the first case there was a large tumour; in the second the tumour had gradually increased to a great bulk; and in the fourth case, when the Doctor first saw the patient, the breast was very large. But the tumour in the first case was not only large, it was likewise very hard and unequal; the second was also hard; the third, from a small hard tubercle, proceeded, ere fluctuation was perceived, till the whole breast seemed schirrous; and from a small red tumour in the fourth case, the breast in which he felt fluctuation was large and hard.

I must mention, however, that in no case which has come under my observation, has there been a hardness such as that which seems to have been discovered in these cases. In no instance was there ever any kind of tumour which, strictly speaking, was entitled to the denomination *schirrus*; or which had any thing resembling the firm whitish bands, such as are described by Dr. Baillie in his *Morbid Anatomy*. Neither have I at any time

or stage of the complaint been able to perceive in the tumefied part any of the circumstances which Mr. Abernethy considers necessary to characterize carcinoma, and to distinguish it from other sarcomatous tumours. Nor does it seem compatible with the laws of the animal processes, that a portion of mammary substance, hardened into genuine schirrus, and completely disorganized, should of itself melt down, and become a sac filled with inodorous, mild bloody fluids.

The contents are often variable both in constituent parts and in consistence. At one time the tumour contains a dark-coloured, grumous, thick, and tenacious bloody matter, which has no doubt given rise to the denomination sanguineous. At another time, the proportion of serum is so considerable as greatly to alter the character of the whole in consistence and fluidity, and to entitle it to the appellation sero-sanguineous. Again, there is frequently little, nay, often none of the colouring matter of blood, the serum being almost the alone constituent, thin and attenuated, from the great deficiency in the relative proportion of albumen. These variations, as has been noticed, are not to be found in different cases only, but also often in the same cases at different times; and even at different times of the same day in some instances,—the discharge in the morning being bloody, thick, and grumous,—and in the evening attenuated serum, having scarcely the tinge of blood; and *vice versa*. From the contents of the tumour, therefore, it is impossible to draw any essential character.

The contents of the tumour are liable to considerable change in the progress of the disease. This change I have observed to occur, first, from the nature and kind of the external applications used; secondly, from the degree and kind of exertions in which the patient engaged; thirdly, from the temperature of the season and circumstances of exposure; and lastly, under various degrees of healthy and distempered conditions of the general system. These changes form instructive indications, by which the means of cure were indicated.

It is of some importance to advert to the local injury which the parts sustain, from the effect of long-continued and increasing distention. Without entering minutely into the consideration of the manner in which such violence is known to disturb the sound organization of the integuments, so as to induce increased morbid disposition, and destroy the tendency to the process of reunion, we may acquiesce with Mr. James in the remark, "that too little is known of inflammation of cysts of chronic abscesses, or diseased action which causes the formation of such cysts, and the secretion of the fluids which distend them, to speak with confidence upon the subject." But, while all this is granted, who can deny the pernicious results of excessive distention in the cases of encysted tumour to which we are now alluding? Independently of every other consideration, the eye of expe-

rience, and what is usually termed common sense, cannot be blind to the baneful consequences.

It may indeed be asked, what, then, is to be done in such a case? If I am correct, I believe it is an observation of Mr. Hunter's, that, while in genuine abscesses the suppurative inflammation diminishes from the moment they are opened, on the contrary, the opening of encysted tumours is frequently the commencement of high inflammation. Resting, therefore, upon the authority of one, to whose observations the highest value is attached, practitioners may be deterred from opening tumours such as those under consideration. Whether this has been the case; whether practitioners have often been swayed in former times by a like opinion, or continue still under its influence in treating the tumour we are now considering, may be left as matter of future inquiry. The main point at present is, have any proofs appeared from the few cases on record of any good being effected by allowing the contents of the tumour to become very abundant,—to accumulate till it burst spontaneously? And have we not the strongest reason for suspecting that Mr. Hunter's view may have arisen, either from some inadvertence in the mode of opening them, or from some mistake in the time of opening them, when we know that encysted tumours are daily opened now with impunity?

The observations I have had an opportunity of making during the progress of the cases detailed above, induce me to divide the disease into three stages,—first, That stage which is distinguishable by a certain kind of almost general swelling and disturbance of the mammary organ, previously to the actual formation of any distinct or insulated tumour; secondly, the stage during which a distinct and permanent tumour can be readily ascertained; and, thirdly, that in which the tumour may without hesitation be pronounced to contain an accumulating fluid.

By thus dividing the progress of the disease into three stages, the description will accord with the natural order of morbid phenomena as they occur, the history will be rendered more intelligible, and the matter treated of will be more thoroughly comprehended in all its bearings. In tracing the history of the disease on these principles, I avoid all reference to the term cancer, as I conceive no proof of its alliance with that malady has been yet adduced.

First, then, the disease commences with a gradual and moderate swelling of the mamma, not altogether unlike that periodical fullness and distention which, in delicate females, so regularly recurs in it, from what is called the sympathy or periodical influence between the breast and the uterine system. If all things be duly considered, however, there will be little difficulty in distinguishing the one swelling from the other, when it is known that in every instance the supervention of the former is always more speedy or sudden than that of the latter swelling, sensibly more considerable,

attended with sensations of a nature more uneasy and stationary, more acute, and consequently upon the whole more troublesome.

It ought at the same time to be carefully distinguished from every kind of inflammatory and acute active swelling, to which the breast is so much disposed from morbid impressions in general. This also may be found a matter of little difficulty; for continued sharp pain, distressing tension, inflammatory redness, great heat, or throbbing, or feverish accessions, are symptoms of morbid activity with which it has never, so far as I have witnessed it, been accompanied.

But that it is a swelling which arises from a certain disturbance of the local functions will probably be granted from this consideration, that its immediate cause is often external injury of the parts. In two of the cases with which I have been concerned, its origin was traced either to a bruise from a certain degree of temporary pressure that was nowise violent, or to sudden collision with a solid, though not very hard or heavy body. None of the cases, it is necessary always to keep in mind, exhibited a disposition to any thing like a painful or severe affection. They were of such a mild nature, that the effects were more correctly entitled to the character of occasional uneasiness, than to that of very troublesome or distressing feelings; a circumstance which not only regards the time of infliction, but even the state subsequently, and appears to be one that may be viewed as almost peculiar to this disease.

Hence, if it be admitted that Carmichael, Smith, Pinel, and Bichat, in the arrangement proposed by them, on the principle of referring to the elementary tissues certain morbid processes included under the general idea of inflammation, intended likewise to comprehend the modified process of functional disturbance which has just been noticed, as the result of a particular kind of external injury, there will possibly be less objection to my considering this process as a species of chronic inflammation, peculiar in such circumstances to the cellular tissue of the female breast. Perhaps, on this admission, the hardness of tumour recorded in Dr. Monro's cases may be accounted for, by allowing that the primary or subsequent affection may have been accompanied, at particular times, by a kind of inflammatory operation, whose action, allowing that it was modified, and of a mitigated nature in general, was at these times deeper and more extended than in the cases which have come under my observation.

My reason for this supposition is, that during the first stage of the disease of which we are speaking, there will, for a short while, occasionally be observed, as if it were deep in the substance of the breast, a gradual approach towards a superficial firmness, resembling in some degree that which occurs from recent and temporary renewals of mild or chronic inflammation, and extending more or less considerably; and it is a symptom indicative of this stage being nearly terminated whenever

the firmness begins very easily and with more than usual frequency to make its appearance. But it seldom remains more than a few days at a time in that state of approaching firmness, the circumference of the firmness by degress subsiding, till all the continuous parts yield a feeling of softness on examining them, which, by a cursory examination, might be pronounced sufficiently natural. In the centre, however, may soon be felt something of a similar firmness, greatly reduced in size, of irregular dimensions, and figure; and this eventually diminishing, and as it were melting into a characteristic shape, can be ascertained, in the course of time, as a permanent, insulated, and small but not hard tumour; settled in the spot precisely where, by pressing gently with the finger at the commencement of the disease, it may often be discovered to differ from the parts around it, in being tender or slightly painful.

This, then, is about the time or period I have been disposed to regard as marking the introduction of the second stage. How long the first stage may indeed continue cannot be exactly stated. The symptoms by which it is distinguished may continue for two, three, or six months; sometimes near to or beyond twelve months, during which, as will naturally be expected, they are liable, after subsiding, to be occasionally again roused by new occurrences.

The second stage having commenced, and the tumour being distinctly discovered, it is felt as a small conical or oval body, neither so detached, as to roll easily under the finger, and yet so loose as, in lateral directions, to be easily moveable. In these circumstances, it may be readily distinguished by a circumscribed pulpiness, and a kind of soft doughy elastic firmness, with a peculiar smoothness of circular and compressible exterior. It is altogether free from much preternatural heat; there is no inflammatory redness of the cuticle, and in the parts immediately around there is a dulness of pain, and often a notable obtuseness of feeling. Its uniform ovoid figure is a striking contrast to the generic character of encysted tumour, which is said to be irregular and always varying in its form.

While the form, the mobility, the uniformity, and the sensations of the tumour in this stage are peculiar, its situation also merits attention. In the usual description of encysted tumours one of their characteristics is, that, being formed in the cellular membrane, they are to be felt immediately underneath the common integuments; a description undoubtedly leading to the expectation of feeling them almost in direct contact with the epidermis. In the state of the tumour alluded to at present this is not to be expected. For although it may be traced as in the cellular texture, and consequently not so deep as ordinary mammary tumours, still it feels deeper and more distant than the description above would indicate. It is somewhat superficial, and is felt at the same time as if a little softness of substance intervened between it and the skin,

which moves over it with a certain degree of sliding motion, affording one more criterion by which, in connexion with the other circumstances already mentioned, the nature of the disease may not unfrequently be ascertained with a considerable degree of accuracy.

The circumstances which have been just related are considered as symptomatic of the second stage. But, like the former, it is impossible to define the period during which this stage continues. The actual growth or increase of its size may be scarcely perceptible for years, or circumstances may occur by which it is obvious in a few months. It deserves to be remembered, however, that at times there appears to be an enlargement when there is actually none. At these times there is an appearance of its having acquired an increased solidity of texture; and yet, some time afterwards, it will be found to have returned to its former dimensions, and to have resumed its characteristic of circumscribed softness and pulpy feeling, while the surrounding parts have again become softened. When a change is thus effected, it recovers its loose and detached state, so that, from being in adherence somewhat firmly to the integuments, it may be again freely moved by the finger as formerly. Alterations of this nature are induced by various external causes, and consequently the disposition to grow is accelerated or retarded according to the frequency of their recurrence; which at once must be seen to render its duration indeterminate. It happened in one instance that the temporary appearances of increased disease came on a short time after a fall, even although the breast, so far as it could be ascertained, had at the time sustained no direct external injury.

It is now that the third stage is ushered in; not merely by the increasing bulk of the circular tumour, but by the progressive accumulation of fluids within the embryo cyst. But there may be some difficulty in acquiring a distinct knowledge of this latter circumstance, previously to the cyst attaining the diameter of half an inch, or three-quarters of an inch. The reason of that difficulty seems to be an irregular thickness of the encompassing coat; for sometimes it feels as if there were either an increased quantity or an increased bulk of soft substance intervening between it and the finger, while at other times that intervening bulk is not perceived, which irregularity renders the sensations communicated by the contents very different on different examinations. This tendency to the thickening of the parts, however, gradually disappears; and hence, in the course of time, the accumulating fluid can be satisfactorily observed as well as the slenderness of the cyst's structure.

The state of the tumour as to the actual accumulation having become sufficiently manifest, it might be supposed that the future augmentation of bulk would be rapid and regular; but this is not always the case. It often proceeds as slowly and as irregularly in this respect as we have noticed it to have done

in all its previous conditions. But the irregularity of this process is much more remarkable before the tumour attains a certain magnitude than afterwards,—an observation of great practical moment; for in proportion to its bulkiness, so is its tendency to rapid accumulation and consequent distention of the integuments. This circumstance was strikingly verified in one of my cases, from there being two tumours of the same nature in the superior portion of the same breast. The one tumour was about an inch distant from the other, without any communication, so far as various attempts to ascertain that point could be trusted. The one was also much smaller, being much later in appearing than the other; which afforded opportunities of acquiring information both as to that circumstance and as to several others of considerable importance.

The only important change in the character of the tumour, then, after the third stage has fully commenced, (waiving those considerations which relate to the effects of distention, either in causing a varicose appearance of cutaneous veins, or other local morbid impressions,) is the distressing bulk the tumour may attain from the increased quantity of sanguineous or fluid contents.

On the formation of the cyst little is accurately known.—The opinions of Bichat (*Anatomie Générale*) who refers the formation of encysted tumours generally to laws analogous to those which regulate the growth of organized parts, appear, if literally applied, scarcely compatible with the circumstances of the formation of the membranous envelope in question, which is the product, not of salutary but of morbid processes. But, not to enter into the various discussions upon this point, it seems probable that the cyst under consideration is not a new production in the part, but originally formed from extravasated fluids, collecting in one or more cells of the cellular membrane, along with an increasing tendency to accumulation. If this be the case, it seems not unreasonable to believe, that, with the increase of collected fluids, there may be a progressive dilatation and extension of its coats, which, by deriving accessions of compacted tissue from the adjacent cells and those in close contact, may thus acquire whatever is necessary for extension and strength. The objection that the neighbouring cellular membrane does not disappear or diminish in every instance, whilst the sac acquires a large bulk, is, if the animal operations during disease be duly considered, of very little importance.

The second consideration relative to the cyst regards its texture; and here it is proper to mention, that in the cases I have at one time or other had the opportunity of examining carefully, at an advanced period of the third stage, the cyst was almost invariably slender, with little vascularity, and little or no attachment to the surrounding parts. The uniformity of slender texture was not alike perceivable, however, on external examination, at every period of this stage. Sometimes it felt as if a little thicker, tenser, and

firmer than it did at other times; and yet this circumstance may be easily accounted for, by remembering what has been already stated;—namely, that the disease commences with a swelling, moderate, yet more or less extended; and that during the first and second stages symptoms of temporary disturbance of the vascular functions are not unfrequent, resembling chronic inflammation, and appearing in great measure to be confined to the cellular tissue, although varying both in the force of its activity and in the sphere of its operation. When the tendency to these affections is roused in this third stage by an incident unusual, yet so light as almost to pass unnoticed, the vascularity is re-excited, the parts affected, and the texture for a short time altered, as above remarked.

The third or remaining particular is the hemorrhagic process—that defect of integrity of the vessels in consequence of which the sanguineous congestion in the cyst is effected. In speaking of this, however, it is necessary to remember, that what is to be attempted with a view to illustration will refer to the third stage of the tumour principally and during the latter periods of tumefaction. If it is not likely that the effusion proceeds immediately from arteries, it must be admitted to issue from veins. Let us, therefore, see if this opinion be sanctioned by any thing like demonstration from practical experience.

In the first place, then, it will be allowed that veins may be so diseased as not only to cause an essential derangement of their functions, but also an obvious and very considerable dilatation of their coats or caliber. To account for this it is presumed none will be disposed to object to the belief, that, in some instances at least, the dilatation ensues from the loss of that tone in the tunics or walls which is necessary to their healthy condition; and it will scarcely be affirmed that in all instances the varicose state of veins is brought on solely by the loss of the necessary support of the adjacent parts, however possible it is that such a loss must add to the general effect.

The possibility of the loss of tone being therefore admitted, we may more easily account for the effusion of bloody matter within the sac, on the principle of open mouths of venous branches, which are more diseased at one time than at another, and more or less at these times in a weakened and relaxed state.

That the effusion may partly depend upon an increased *vis a tergo*, brought on by an increase of disease, is not unlikely; yet the possibility of additional causes removes not the possibility of the main cause, namely, the deficiency of healthy tone in those vascular branches which are immediately concerned with the diseased parts.

This may be farther explained by an observation which any one in such cases may make, and which I have had occasion to make in several instances. When, by means of a small aperture, made by introducing an instrument similar to the couching needle, I was under the necessity of emptying the sac

of its contents repeatedly, it frequently happened, if the patient confined herself at these times to bed for a few days, or avoided the usual bodily exertions for a little afterwards, that the discharge, on soon again introducing the same instrument, had become much thinner, having less and less of the red matter of blood, and more serum, varying in proportion to the degree of rest.

So much was this the case, that the discharge at times was altogether without the colouring part, and not unfrequently near the transparency of limpid water; and even although it was limpid one day, the alteration the day following might be striking, from the patient in the interval taking a short airing in a carriage; the red matter being not merely augmented in quantity, but the fluid greatly more consistent and thickened, the breast at the same time having invariably less or more the appearance of new disease. Hence it seems perfectly obvious, whether venous trunks, branches, or capillaries be said to open into the sac, that the diameter of their caliber is influenced by morbid impressions, permitting them to pour out the grosser or finer parts of the circulating fluid; and hence also such occurrences seem to afford a pretty sure mark, that none of the vessels thus affected, under the like circumstances, can be very large.

It is not improper to mention, that, in Mr. Briggs' case already referred to, which, in consequence of Scarpa's observations on the same disease, was published in 1822, a large venous trunk was exposed during the operation for extirpating the breast, immediately below the edge of the pectoral muscle. It bled profusely, and was considered as the source from which the tumour had originated, although the mode of communication could not be demonstrated by an opening in the sac. In one of the cases given above, nothing worth notice could be traced on carefully inspecting the amputated breast, except a few scattered bodies, not unlike pretty large beads, which, from their limpid appearance, have been generally denominated incipient hydatids.

I should now proceed to investigate the most rational and proper methods of treatment applicable to this disorder. From the statements already made several practical inferences naturally result. On these it is unnecessary to dwell long; and I shall, therefore, conclude with a few brief remarks.

In the first place, it is obvious, that, previously to proposing any plan of local treatment, the stage of the disease ought distinctly to be determined. The stages differ so materially from each other in certain particulars, that what may be proposed for one, may be wholly inapplicable to another. A moderate degree of topical pressure by proper bandaging proves highly beneficial, for instance, in the second and third stages, but less so in the first stage. The repeated evacuation of the contents, when the tumour is very bulky, and the sac is much distended, in order to prepare

for other modes of treatment, is in great measure only applicable to certain periods of the third stage. The appropriation of medicinal means to the nature and circumstances of each stage tends to simplify the choice of remedies, and become a matter of no small importance.

Farther, if by piercing occasionally at an advanced period of the disease, the contraction of the sac is not only encouraged, and the growth arrested, but the integuments prevented from assuming that morbid disposition which excessive and increasing distention is sure to induce, while the subjacent parts are relieved from the pressure by which morbid operations are excited, little argument is required to prove the salutary effects from piercing as early in the third stage as circumstances permit. By doing so, the probability of success by vinous or other injections is greatly strengthened.

Again, the deleterious effects of incisions in the third stage, or openings of the sac with a lancet, and the safety with which, along with measures of subsequent precaution, the sac may repeatedly be pierced by a puncturing instrument, as described in the cases, it is hoped, are facts satisfactorily established.

Still farther, when it is considered that the disease originates in the vascular, but not the glandular part of the breast, many difficulties in the course of treatment will speedily vanish. To extirpate the embryo cyst or sac about the termination of the second stage would be a process attended with very little difficulty; and to speak of amputating the gland in advanced cases generally is almost superfluous; for, as the disease advances, the glandular substance seems at last disposed to disappear spontaneously.

To conclude. As a bad state of general health tends to influence every local disease, it is indispensable to use every means of improving the former. Sedulous management of the process of digestion, which should be conducted in such a manner as to nourish without oppressing, is the great object to be kept in view.

From the London Medical Gazette.

#### MEMOIR ON THE OBSTACLES PRESENTED TO DELIVERY BY THE MALFORMATION OF THE FÆTUS.\*

By A. DUGES, Professor to the Faculty of Medicine, Montpellier.

We shall speak, first, of the obstacles presented by excess of size of the whole or part of the fœtus—as hydrocephalus, dropsy, &c.; and, secondly, of the difficulties resulting from the addition to the body of the child of some part of another fœtus, or the partial union of twins.

##### *Excess of Volume.*

There can be no doubt but that the size of

the child, when considerable, may render the labour more slow and painful, particularly if the passage is but little dilated, and not sufficiently supple, as in a first confinement; or if the pelvis be rather narrow; and still more if to these be added an unfavourable position. But, independent of these accessory circumstances, it may be stated, that a large size of the child, provided its body be well proportioned, is never an entire bar to the spontaneous completion of labour. It is difficult, indeed, for a fœtus to exceed certain limits in its growth: either the uterus would resist a distention greater than it receives at the full period, under ordinary circumstances, and then the child would perish from the pressure; or else this organ, incapable of sustaining the expansion produced by the preternatural dimensions of its contents, would open and expel them. Children are said to have been born measuring 23 or even 25 inches from the vertex to the heels. These dimensions, however, have, no doubt, been made by guess, and as approximations: the last, indeed, would equal the stature of a child a year old. The general length is 18 inches, and the extreme would appear to be 22. I have seen an infant born of this last dimensions, and, next day, another a little less: the latter being 20 inches; it was plump, and weighed nine pounds and a half—the first born weighed about a pound more. Twenty-two inches from the vertex to the heels, then, may be stated as the extreme size of a well-proportioned fœtus; and it is easy to prove that the head of such a one will not exceed the dimensions of an ordinary pelvis. In fact, we know that the head of the fœtus, in passing the superior isthmus, always, in natural cases, performs an evolution which brings into relation with one of the oblique diameters of the isthmus its *occipitobregmatic* diameter, which would not, even in the case we suppose, exceed four inches, or rather less. Now this is six lines under what is generally assigned to the part it has to pass through. The opposite oblique diameter is there presented to the bi-parietal, which is about the same length as the other. The occipito-frontal is not really presented to the abdominal isthmus of the pelvis, except in imperfect positions; and the same remark applies *à fortiori* to the occipito-mental diameter. These alone can present powerful obstacles to spontaneous labour: the former, indeed, is about five, and the latter five and a half inches. These unfavourable diameters may present in labours where the feet have come down, and when ill-directed efforts have been made by pulling to facilitate the delivery. The natural efforts alone would scarcely produce this inconvenience, as M. Desormeaux has shown, because they would produce an evolution analogous to that which takes place in the presentation of the vertex. This was completely proved in the case above-mentioned, where the fœtus measured 22 inches: the limbs and trunk were easily extracted, and efforts made to accomplish the delivery by pulling—but without avail. On leaving the pa-

\* Memoires de l'Academie Royale de Médecine.

tient for some time without assistance, the head was spontaneously expelled.

It is principally when we are obliged to turn an infant of large stature that great difficulty is experienced; and it is then that redoubled care is necessary, to avoid suffering the arm to cross the neck—to turn the face first towards one side of the pelvis, then towards the sacrum, and to depress the skin in such a manner as to render the sub-occipito-bregmatic, and the bi-parietal diameters, alone parallel to those of the narrow parts of the pelvis, and to the external organs.

I do not speak of the other indications which may present themselves, in the application of the forceps, &c.—the difficulties arising here, from the disproportion between the head of the child and the pelvis of the mother: it is evident that the precepts are the same as for the first degree of narrowness of the pelvis. I merely wish to speak of the diagnosis.

Of all the means which may lead to the discovery of a fœtus being larger than natural, none is either certain or easily applied; and none, therefore, is unequivocal, except the expulsion of one of the members before the rest of the body. The size of the abdomen after the escape of the waters, the uniform nature of the tumour it presented before, contrasted with the inequalities to be felt through the parietes of the uterus and abdomen—such are the marks which will tend to distinguish the case in question: first, from the distention produced by the waters; and, secondly, from the existence of twins, which give to the abdominal tumour a *bilobed* form, and in which we hear the heart beating in two different parts of the womb.

To these data we ought to endeavour to add the measurement of the part which presents—of the head, for example. Various contrivances have been suggested for this purpose, the accuracy of which I doubt. The simplest instrument is the finger, introduced per vaginam; but how deceitful is this method to an inexperienced practitioner! He who is only accustomed to judge of the dimensions of the head by sight, cannot be persuaded but that one, the surface of which he feels in the pelvis of the mother, is immense. Practice easily dissipates this illusion; and a finger accustomed to it is the best gauge of the size both of the head and the parts it has to pass through. It cannot only be passed along the former, but it can compare it with the circumference of the upper isthmus—judge how much it fills of this aperture, in what degree it presses upon its parietes, &c.; and it is always the *relative* proportion on which depend the practical results. In these investigations it must not be forgot, first, that the tumefaction of the integuments of the cranium often increase its volume as to height; secondly, that this tumefaction, as it were strangled by the orifice, or by the arch of the pubes, always constitutes a portion of a much smaller sphere than the entire head; thirdly, that, in the first period of the labour, the head, not yet mould-

ed to the parts, presents all the extent of its upper or vertical oval; fourthly, that, at a more advanced period, it is the occiput which becomes more particularly accessible to the finger. By overlooking these circumstances one would be led to think the head larger than it really is, in the first and third case, and smaller in the second and fourth.

The above remarks also apply entirely to excess of size, limited to the head, without any real disease of that part. Thrombus, to a considerable extent, beneath the skin of the cranium, sometimes deserves attention: less, however, on account of the increased size of the head, than from the deformity which it produces interfering with rotation; for example, when the tumour is engaged under the arch of the pubes, and becomes, to a certain extent, moulded to the parts. It would be still more difficult with the infiltrations which take place while the integuments of the head are putrid; and this circumstance only deserves notice on account of the great size which the distended integument sometimes attains. It might then, indeed, give rise to the idea that hydrocephalus existed; from which, however, it may be distinguished by its softness; by the fœtid discharge from the uterus; by the facility with which it accommodates itself to the dimensions of the passage which it traverses, &c. There can be no doubt but that this has constituted the majority of the cases of *external* hydrocephalus mentioned by the older writers.

#### *Case of Voluminous Head—Prolapsus of the Cord—Turning.*

F. Mathe, aged 41, arrived at the full period of her second pregnancy, without any other inconvenience than considerable constraint in walking. She was brought to the Maternité at midnight. The os uteri was almost completely dilated, and perfectly soft; the vertex presented in the first position, and a portion of the umbilical cord, retaining its pulsation, floated in the vagina. The waters continued to come away at intervals. To obviate the danger resulting from the compression of the umbilical cord, recourse was had, without delay, to turning. This operation was begun without difficulty, in the usual way, and the extraction was easy until the head came into opposition with the superior isthmus, but it was then arrested by an unforeseen obstacle: in vain were gentle efforts made by laying hold of the shoulders and lower jaw; already the application of the forceps was in agitation, when a pain, aided by gentle pulling, perhaps better directed than before, produced the expulsion of the head, the great size of which afforded some explanation of the difficulty which had been experienced. This head was exactly five inches from the front to the occiput, and four across the temples; yet the child did not weigh altogether more than seven pounds and a half. It only lived a quarter of an hour. As to the mother, the placenta had scarcely come away when she began to complain of acute pain in the loins, which, in-

creasing, became fixed in the sacroiliac symphysis, and afterwards in the symphysis pubis. On examination per vaginam, it was found that a separation of the bones had taken place at this last, to the extent of two or three lines. Local and general antiphlogistic remedies, such as leeches, cataplasms, baths, and venesection, diminished these symptoms by degrees; at the end of a month the patient walked, but the convalescence was very slow, although it at last ended in complete recovery.

#### *Water in the Head.*

The only objects connected with water in the head, which we have to consider, are those which influence parturition; its frequency at the time of birth; the signs by which it may be known; its effects on labour; and the manner of affording the necessary assistance.

As to its degree of frequency, in consulting the registers left by Mad. Lacapelle, I find that, of 43,555 labours, which took place at the Maternité, between 1799 and 1820, only 15 cases of hydrocephalus at birth are mentioned, giving a proportion of 1 in 2904. We must, therefore, expect to meet with them very seldom in civil practice, and take care not to suspect their existence on vague grounds.

Among the signs, there are some which may be called conjectural—such as certain circumstances which may be looked upon as giving rise to hydrocephalus: for example, serous infiltration of the cellular membrane in the mother, during pregnancy, or a very large quantity of liquor amnii. We may also mention an hereditary disposition—for instance, if the woman has already given birth to one or more children labouring under this kind of dropsy; if she has herself a large and prominent forehead; if she is of a lymphatic temperament, and disposed to anasarca; we may then apprehend that all her offspring will be hydrocephalic. These, however, after all, are merely conjectural; and it is to the sensible signs—those afforded by the touch, that we must trust.

A surface which is large and little convex—which covers all the points of the superior isthmus, without, however, passing into it—a consistence which varies at different points, but which always presents resistance during the pains—softness, or even fluctuation, produced by the finger during the intervals: these are what is first perceived. By proceeding regularly, portions of the bones may be felt separated by membranous interstices, and the fontanelle—sometimes as large as the palm of the hand. If any other part than the vertex has presented, so that the head is only accessible at its base, the separation of the bones will be much less, but still will be easily appreciable. Such is the description of an hydrocephalus which is considerable; and the same marks measured by a smaller scale will also detect an instance of the affection proportionally less; but the head being then more convex, will also be less soft, and will protrude more into the pelvis.

We have pointed out in the preceding part the characters of an infiltration external to the cranium: we may mention, with regard to sanguineous infiltrations, that clammy softness which retains the impression of the finger, and which will be sufficient to prevent any risk of these being confounded with hydrocephalus. The head of a healthy fœtus is possessed, even when large, of a density of the bones, and narrowness of the fontanelles, which admits not of mistake. Sometimes a softness of the parietals is met with, which might give rise to error: this softness depends upon imperfect ossification of their inner and posterior part—there they are often very thin, pierced by spaces not yet ossified, and easily broken even by the process of labour: they yield to pressure with a crepitation like that of dry parchment, and spring up again in the same way. This last character is pathognomonic: once, however, I saw this region of the skull absolutely membranous, to the extent of an inch and a half in every direction; but the neighbouring bones did not yield to pressure in the same way as those of an hydrocephalic child, and the sutures had their accustomed arrangement.

I shall here also mention another source of error. It is an unnatural direction of the fœtus, in which the trunk is directed towards the loins of the mother, and the head rests above the pubes. This constitutes the *posterior obliquity*, denied by Baudelocque and others. The axis of the fœtus, far from being parallel to that of the superior isthmus, crosses it at an acute angle, and the head rests above, propped upon the anterior part of its circumference. The elevation of the head, and its immobility, notwithstanding the uterine contractions, and the natural dimensions of the pelvis, may the more readily lead to deception, as we cannot reach the vertex with the point of the finger without difficulty, and can scarcely measure its size by the ordinary process of the touch. But only to speak of the most important signs—this very elevation, and the hollow which remains between the head and the sacro-vertebral angle (which is easily discovered,) are quite sufficient for the diagnosis.

The indications do not depend merely upon the size of the head, which we never can ascertain with precision—they must also be guided by its flexibility—the disposition it evinces to enter the pelvis. A head of moderate size, soft and flexible, a vigorous mother, and contractile uterus, are circumstances which would lead the practitioner to trust to the spontaneous termination of the labour; but if the head advanced slowly—if the woman be weak and exhausted—the forceps may be employed with advantage. The branches applied to the sides of the pelvis must be brought together with caution, and the accoucheur must pull very gently, lest he should produce laceration, or have the instrument lose its hold. If the infant presents the shoulder, and the head be disengaged, and appears of middling size, turning is indicated. The extraction of the head, if the child be living, may be assist-

ed by introducing the fingers into the mouth, and even by the application of the forceps: if it be dead, this will be known, because the trunk will have been already extracted, and we can then act without reserve, perforating the cranium, or applying the sharp crotchet; but if the ordinary perforator cannot be introduced either by the fontanelle or the occipital foramen, the *terebellum* (which I have proposed in cases of deformed pelvis) will pierce through the bone itself.

If the death of the fœtus could be ascertained with equal certainty when the vertex presented, the perforation of the cranium would still be applicable; but is it the same where the vertex presents, or the child has been extracted as far as the shoulders, and there exists a certainty, or at least a strong probability, of the contrary, the head being such that neither the forceps nor hand can effect its extraction? The hydrocephalic patient, it is said, will perish a few moments after its birth; it may, therefore, be destroyed to save the mother. But even in admitting this supposition, and considering the infant as the destroyer of the mother, does it rest with us to take away its life? We may, I think, be permitted to doubt this.

After it is punctured, the head often passes on from the mere efforts of the uterus; but, if assistance be required, it may be derived from the forceps, turning, or the blunt crotchet; but these manipulations do not come within the scope of the present discussion.

A young and robust woman gave birth, on the 23d November, 1819, at the full period of her third pregnancy, to an infant which was dead and hydrocephalic. This disease had been ascertained during the labour; but as the head made progress, although slowly, it was not deemed necessary to have recourse to any operation. It was not till twenty-four hours after the commencement of the pains that the delivery was completed. The mother did well. The child weighed altogether seven pounds ten ounces; the serum contained in the head rather more than thirty ounces. It was reddish and turbid, contained as usual within the ventricles, the parietes of which were very much attenuated. The head had the following dimensions: occipito-mental diameter, 6 inches, ten lines; occipito-frontal, 6 inches, 8 lines; bi-parietal, 4 inches, 11 lines.\* I need scarcely remark that a head so voluminous as the above could not have been spontaneously delivered except from the assistance afforded by its flexibility.

A woman of strong constitution, aged 24, had rather a distressing pregnancy. Labour commenced on the 3d of March, 1824, and the membranes ruptured at five o'clock in the evening. The head remained above the superior strait, although the dilatation was complete. The pains ceased soon after, and, not having returned at eleven next morning, Madame Legrand directed one of her assistants

to turn the child and terminate the delivery. On introducing the hand, she found the face of the child to the left and behind. She then laid hold of the left foot, and being unable to find the other, contented herself with proceeding methodically with the one she had reached. The delivery was readily effected of all the fœtus, excepting the head. This, however, resisted every effort: the forceps slipped over it, the blunt crotchet was of no avail, and it was soon perceived that the child no longer exhibited any signs of life. In an hour after, M. Dubois made fresh attempts to effect the delivery, with the same instruments, but in vain. He then took a sharp crotchet, and pierced the left side of the head, near the mastoid fontanelle: immediately a serous fluid escaped in abundance; the head was extracted, and it was discovered that it had been distended by a dropsical effusion. This case affords a striking illustration of the disposition which some women have to give birth to hydrocephalic children—both the instances above related having occurred in the same individual.

#### *Dropsy of other Parts of the Body.*

Hydrocephalus does not always distend the whole head equally, but sometimes forms irregular tumours, which, however, on account of their softness, rarely present any obstacle to delivery. Ascites, still more uncommon than hydrocephalus, and water in the chest, yet rarer than either, do not necessarily prevent the child from being delivered either spontaneously, as I once saw, or with a little assistance. Indeed dropsical children are generally born before the full period. The infant above alluded to was born at the eighth month; one mentioned by Ramsbotham at seven months; and another by Portal, at the same period. A very large quantity of liquor amnii, and ascites on the part of the mother, may be causes of, and consequently conjectural signs of dropsy of the fœtus; but the truth, cannot be ascertained in a satisfactory manner, until the expulsion of some part of the body has taken place: retained then by the enlargement of the abdomen, or thorax, it is arrested, and the accoucheur finds the pelvis filled by a large, soft, fluctuating tumour, which is easily evacuated by puncturing it with a trocar. The delivery will not fail to take place without difficulty, and unassisted by the operations recommended by various writers.

I shall only add one word with regard to those harder steatomatous tumours which may interrupt the progress of labour. They will often yield to pulling; and thus to remove them, if possible, or to empty them, if they contain a fluid, but always with the greatest possible tenderness towards the infant, if yet alive, are the only general directions which can be given.

#### *Multiplication of Parts in the Fœtus.*

Baudelocque has justly observed that it is extremely difficult to recognise the true state of matters under such circumstances. We

\* French admeasurement.

shall, therefore, only give in this paragraph, some of the signs by which the presence of twins may be distinguished from that of a double fœtus. Before the labour, the division of the belly into two lobes, the movements felt by the mother in two very different places; the beating of two hearts, heard at a great distance from each other, by means of the stethoscope, but with some variety in the situation; these are the marks rather of twins than a double child—unless, indeed, as in the case related by Walter, the uterus contains both twins and a double monster. If, when the labour has begun, we perceive two membranous bags, and the waters come away at two different times, the presence of twins may be looked upon as certain, for there are never two distinct envelopes for a double monster, and very seldom are natural twins contained in one. If one or both feet come down with the head, and if they are extracted by gentle pulling, without the head having a tendency to ascend, then we may be sure that there are two separate children; for a monster is never formed of two individuals, so placed as to have the head of the one by the feet of the other. But if several members present at once, it is only by carrying the hand into the uterus that it is possible to ascertain whether the individuals to whom they belong be joined together or separate.

The facility with which the natural efforts, either alone, or assisted even by persons of little skill, effect the delivery of monsters of the most disadvantageous formation, with regard to the mechanism of the parts, has always excited the astonishment of accoucheurs. The chief impediment is presented by the existence of two heads; and we shall briefly consider the cases where, along with this, the trunk also is double, and those in which it is single. If the two heads come down first, can the delivery be spontaneously effected? I think not, unless they are either very small, or have little consistence. It may be accomplished, however, if two fœtuses are loosely united, so as not to be always exactly parallel, but to present the parts successively instead of simultaneously. The direction of the fœtus, according to the axis of the superior isthmus, causes the head which is situated anteriorly to be likewise the inferior, and it is engaged in the pelvis while the other is kept back by the sacro-vertebral angle. The first head, as it advances, may be followed by the second, if small and soft, and the delivery be thus accomplished. But if the heads are both large, the second will, as it were, turn over the sacro-vertebral angle, and thus oppose the delivery.

It does not, however, happen thus if the feet or buttocks present; then the trunk, whether single or double, is expelled; after this the head, which is placed posteriorly, being the lower, (in consequence of the direction of the fœtus, which is then parallel to the inferior isthmus) becomes first engaged, and is afterwards followed by the other, the whole process being unattended with difficulty.

As to the monsters united by the vertex or

occiput, they would afford no real difficulty unless the two heads presented at once. In this case, if the adhesions were sufficiently loose, they would follow the same course as in the preceding instance, but if the feet of one presented, the other would follow without difficulty.

As to monsters united by the breech, the point of union is never sufficiently flexible to admit of a double presentation, the two trunks being connected in a direct line, so that they can only advance by one of the heads, and the birth is then effected without difficulty.

From the *Lancet*.

#### ON SINGLE VISION, AND THE UNION OF THE OPTIC NERVES.

There is no subject which has more engaged the attention of philosophical men, than the phenomenon of single vision with two eyes. How does it happen that man, being provided with two eyes, has, nevertheless, unity of vision?

Dr. Wollaston believes that the faculty of single vision is attributable to a semi-decussation of the optic nerves; namely, that the contiguous half of each optic nerve on reaching the sella turcica, and there uniting with its fellow, does cross, and ultimately serve to furnish the retina to the nasal side of the opposite eye; the retina of the temporal side of each eye being formed by the expansion of half of the corresponding nerve. This distribution of the nerves has not been proved by anatomical demonstration; but Dr. Wollaston considers it established by induction, from the symptoms of disease, in some instances, which he relates.\* It occurred twice to Dr. Wollaston himself, that he was not able to see but on one side of the axis of vision. The first time, the left side of each eye was affected, and he saw but the half of a man's face, or of any object he looked at. The affection did not last long; but several years afterwards, the same phenomenon occurred with the right eye. Two cases of a similar nature, are also recorded by Dr. Wollaston. Desmoulins† mentions a case in which this affection of vision occurred three times; the first two times objects, situated to the right of the axis of vision, were invisible; the third time, objects were seen on the right only of this axis.

Mr. Twining has a very ingenious paper on the foregoing subject, in the second volume of the Transactions of the Medical and Physical Society of Calcutta; and he denies, *in toto*, the accuracy of Dr. Wollaston's premises and conclusions. The following anatomical observations, respecting the structure of the optic nerves and thalami, and the effects of disease on those parts, he thinks, sufficiently establish the fact, that no decussation, or semi-decussation of the optic nerves, exists in the human subject.

\* *Philos. Trans.* 1824.

† Vide Elliotson's *Blumenbach*, p. 261.

*Observation 1.*—Mrs. Scott had a fungus of the left eye, for which the eye was extirpated. Several months afterwards, the patient died; and, on dissection, the left optic nerve was found to be of inky blackness, and this dark colour extended backwards from the orbit, far beyond the point where the nerves join. The diseased nerve, within the cranium, was as thick as the little finger, and the corresponding thalamus was about a third larger than the opposite one, but of natural structure. The dark colour above mentioned was confined to the left side. On the right side, the optic nerve was of its natural size and colour, and was merely attached to the black diseased nerve of the opposite side by cellular shreds, where the nerves come in contact on the *sella turcica*.

This patient had never observed any affection of the eye, until two years before the operation, when the morbid changes commenced; and, in the course of four months, she became gradually blind of the left eye.\*

*Observation 2.*—Morgagni states that Hildanus had dissected a subject that had been blind of one eye, and he found the corresponding optic nerve wasted, even beyond the usual union of the nerves on the *sella turcica*.

*Observation 3.*—A man was afflicted with paralysis of the left side of the body; he was completely blind of the left eye, and the lids of both eyes were closed. The man died, and, on dissection, an ounce of coagulated blood was found in the right optic thalamus, extending into the lateral ventricle. Here was an injury beyond the junction of the optic nerves, producing blindness of one eye, not half blindness of both eyes, which it might be expected to do, if the semi-decussation of the optic nerves did exist.—See Sir E. Home's *Attempt to ascertain the Functions of certain Parts of the Brain*.

*Observation 4.*—A patient was affected with paralysis of the right side of the body. Dissection discovered erosion of the right thalamus. *Hemiopia* not noticed in this case.—See M. Bayle on *Paralytic Affections of the same side of the Body, with Organic Lesions*.

*Observation 5.*—A patient had hemiplegia of the right side, and lived four years after the first attack. On dissection, an effusion of blood was found in the thalamus. *Hemiopia* not observed in this case.

Rostan mentions in his work, *Sur le Ramolissement du Cerveau*, that the disease, when deeply seated, most frequently affects the corpora striata and thalamus of the right side. He states that imperfection of sight and blindness are frequent symptoms in that disease, and sometimes one pupil is more dilated than the other.

*Observation 6.*—Cæsalpinus says, “repertus est aliquando in anatome, alter ex nervis visoribus attenuatus, alter plenus; visus autem erat imbecillis in oculo ad quem nervus extenuatus ferebatur.”

*Observation 7.*—Vesalius relates the dissection of the brain and optic nerves of a woman, who had, from a very early age, been blind of the right eye, the left eye being perfectly sound. The whole of the right nerve was attenuated in this case, whilst the left was sound.

*Observation 8.*—Morgagni states that Vesalius had observed the optic nerves to remain separate throughout their whole course, in a man who had always very strong sight.

*Observation 9.*—Mr. Cheselden relates the case of a gentleman who had strabismus, with double vision, produced by a blow on the head. By degrees, the most familiar objects came to appear single again, and in time all objects did so, without any amendment of the distortion. This fact shows, that points of the retina, not originally endowed with the joint possession of the correspondence supposed to depend on peculiar distribution of the optic nerves in the retinas, may, by habit, acquire that correspondence.

Such are the evidences from morbid anatomy adduced by Mr. Twining, as subversive of the doctrine held by Dr. Wollaston, whilst, for further refutation, he appeals to the labours of Vicq-d'Azyr, Wenzel, Reil, and Haller, who, although they dissected and studied the structure of the brain with great assiduity, failed in demonstrating a decussation of the fibres of the optic nerve.

It appears from an extract given by Mr. Twining, from Harris's *Posthumous Treatise on Optics*, published in 1775, that the sentiments of Sir Isaac Newton on single vision, were precisely in accordance with those advanced by Dr. Wollaston; and that, in some points, there is almost a verbal accordance between the opinions of these great men.

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From the Transactions of the Medico-Chirurgical Society of Edinburgh.

**OBSERVATIONS ON THE EFFECTS OF THE SUN'S RAYS ON THE HUMAN BODY.** By JOHN DAVY, M.D., F.R.S., Physician to the Forces, Corresponding Member of the Medico-Chirurgical Society of Edinburgh.

It is known to every one, that exposure to the sun's rays renders the skin brown; but I am not aware that this well known effect has hitherto been investigated with any minuteness, if at all, either in relation to the manner in which it is produced, or the parts of the skin in which it takes place, or its exact cause, or its consequences.

In this communication I shall have the honour of submitting to the Medico-Chirurgical Society of Edinburgh, the observations which I have made, with the desire of elucidating these points.

1. *Of the Changes connected with the Discolouring Effect of the Sun's Rays.*

For the purpose of ascertaining these

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\* Burn's *Surgical Anatomy of the Head and Neck*.

changes, a portion of the back of the fore-arm, which had never before felt the sun's action, was exposed to bright sunshine, in Corfu, during an hour and a half, on the 29th July, 1826, in the middle of the day, when the thermometer was at  $78^{\circ}$  in the shade. At the end of that time the skin was slightly painful, red, and hot. On the 1st August, the erythema was nearly in the same state; during the night the redness of the skin had been brightened, and the sensation of pain increased. On the 2d, there was very little alteration; a thermometer applied to the inflamed part rose to  $96^{\circ}$ , or  $1^{\circ}$  higher than when applied to the adjoining skin. On the 3d, desquamation had commenced at the circumference; hence, where the cuticle had separated, the part was brownish-red, and not painful. In the middle, where the cuticle firmly adhered, the colour continued to be rose-red, and the pain continued, though in a less degree. This middle part, it may be remarked, in which the erythema was most durable, was most inflamed, the sun's rays having struck on it perpendicularly; whilst, on the circumference, from the rotundity of the arm, they impinged on it obliquely. On the 5th, desquamation was making progress; pain had ceased; the part was reddish-brown at the edges, but still red at the centre; the temperature of the part was not above that of the adjoining skin. On the 8th, the part was uniformly of a reddish-brown; desquamation was still taking place, the new cuticle separating almost as fast as it formed, not in continuous pieces, as in the first instance, where the old was detached, but in small scales. On the 18th, the part was of a light-brown, with a very slight admixture of red, and its tendency to desquamation was very little greater than natural; in brief, it was in that state in which the skin is commonly said to be tanned by the sun.

## 2. Of the part of the Skin in which the Discoloration takes place.

Dr. Bostock, in his learned and useful "Elements of Physiology," remarks, "It has not been ascertained upon which part of the integuments the sun acts, whether upon the epidermis, the corpus mucosum, or the cutis;" and he immediately adds, "but it is probably upon the epidermis, because we are informed that the tan of the skin is removed by blisters."

Were it a fact that the skin is rendered fair by blisters, the argument would be plausible,—I had almost said conclusive; but, as it is well known that blisters themselves render the skin brown, this argument can hardly be received. From the observations which I have made, and from analogical reasoning, I am disposed to believe that the discoloration takes place beneath the cuticle, and that the seat of it principally is the surface of the cutis.

1st. As the sun's rays bleach hair, and as there is a considerable analogy between the

hair and the epidermis, its effect on the latter, it might be expected, would be similar.\*

2dly. I have carefully examined the cuticle detached in consequence of inflammation from insolation, and I have not found it tanned in the slightest degree.

3dly. Are not the phenomena described in the preceding section, relative to the immediate effects and consequences of exposure to the sun's rays, almost sufficient to convince one that the cutis is the true seat of the discoloration? Were the epidermis the seat of it, it ought to be immediately discoloured by the sun's rays, which it is not; and when the epidermis separates, the skin should be fair; but the reverse of this is the case,—not till it separates does the skin lose its bright rose-red hue; and not till after several successive desquamations is the tan of the skin well impressed and established, and many months elapse before it disappears.†

4thly. I have examined, with some attention, the cuticle of the Negro, of people of colour, and of Europeans who have become dark brown from exposure to the sun's rays within the tropics. In each instance, when detached, it has appeared much less coloured than the skin; and, when minutely inspected, it has been found to owe its colour to colouring matter attached to it, detached from the cutis.

Lastly. I have preferred assigning the surface of the cutis as the seat of the discoloration, (supposing it to be proved that the cuticle is not,) rather than the rete mucosum or corpus mucosum of authors, as the very existence of such a texture is problematical. From the experiments which I have made on moles, and the coloured areola of the mamilla of fair persons, and on the skin of the Negro, I am disposed to believe that the colour, in all these instances, is owing to a colouring matter deposited in minute particles or filaments, on the surface of the cutis, as a secretion analogous, in its chemical properties, to the pigmentum nigrum of the eye.‡ In the skin of

\* Vide some remarks on this subject, contained in a paper on the Specific Gravity of different parts of the Human Body, which I have submitted to the Society.

† The discoloration produced in August, by exposure of the skin for an hour and a half, now, at the expiration of seventeen months, is just visible. I may add, that I have found it to continue much longer on a part always covered, as the arm, than on the back of the hand, which has been covered only in the open air.

‡ I find that the colouring matter of the skin of the Negro, and the pigmentum nigrum of the eye, are acted on very similarly by the three mineral acids, and a solution of potash, when heated, and by the sulphurous acid. By the former, both are dissolved; by the sulphurous acid, they are rendered of a light brown colour. They are not dissolved by these acids, or by the alkali, when cold; nor

the white, even in the parts discoloured, as the foresaid areola, I have not been able to discover any traces of a corpus mucosum, when the cuticle has been separated by means of immersion in the sulphurous acid. I have found the brown colouring matter, as I have already noticed, impregnating the surface of the cutis, and to be separated with difficulty by scraping it. In the case of the Negro, the colouring matter is deposited more thickly, and more in the form of a membrane; yet I have not been able to detach it as a membrane, and only in very minute portions, and that by scraping, when the cuticle has been raised and separated with as little inflammation as possible. The evidence in favour of the existence of a corpus mucosum, obtained either by maceration of the integuments, or by the application of blisters, appears to be very doubtful. By the first process, a gelatinous or mucus-like surface may be formed; by the second, a false membrane may be produced by the effusion of coagulable lymph, exactly resembling a corpus mucosum. I do not make these remarks hypothetically, but from experience,—from observing the effects of blisters on parts of the skin in which there have been moles; on the areola of the mamma; and on the skin of the Negro. In all these examples, the effects generally are very similar. If the blister is mild, the cuticle is simply raised; in the instance of the Negro, with a very little colouring matter adhering to it. When severe and long continued, not only is the cuticle raised by serum effused, but also by coagulable lymph, to which is attached colouring matter, and which may easily be mistaken for a coloured rete mucosum, and which is easily separated as a continuous membrane. When severe inflammation and supuration is excited, the colouring matter either comes away spontaneously, or is most easily detached.\* It appears to be most firmly connected with the cutis in the instance of moles, next in that of the brown areola of the nipple, and least in that of the black skin of the Negro. The part, in healing, when covered with the first formed cuticle, is red; it soon becomes brownish, but a considerable time elapses before it acquires its former intensity of colour. In the instances of the Negro, in which I have watched its progress, the secretion of colouring matter began at the edges, and spread towards the centre; and then, after a few days, spots of black appeared in the middle, which enlarged till the whole area was coloured. When a part not discoloured

is blistered, in healing, it passes from red to brown; and it is often a long time before the part regains its healthy hue; generally, I believe, the fairer the skin, the less it is made brown by a blister, and the sooner it recovers its original whiteness; and I believe the hotter the climate and season, so much the slower it regains it.

### 3. *Of the Cause of the Change of Colour, and of the Manner in which it operates.*

My experiments relative to the cause of the change of colour produced in the skin by the sun's rays, are not so satisfactory as I could wish. They tend, however, rather to prove that the effect is produced solely by the uncompounded rays. I have exposed, for more than two hours, and that repeatedly, the delicate skin of the under part of the fore-arm to the solar spectrum; and I have concentrated the differently coloured rays of the spectrum, by means of a lens on the skin, but without occasioning either erythema or discoloration.

Relative to the manner in which the effect is produced, is it immediate and direct; or mediate and indirect? In other words, is it the simple effect of the sun's rays impinging on the skin; or the effect of the inflammation which they occasion; or do the sun's rays act both ways?

That they act powerfully indirectly in producing discoloration, by exciting inflammation, the facts already mentioned, are, it appears to me, sufficient to prove. Indeed, whatever cause excites inflammation or irritation of the skin, seems to have an analogous effect, and to discolour it. Erysipelas, erythema, most of the exanthemata, burns, ulcers, excoriations, all occasion this effect (and, I believe, cold even is not an exception)\* in different degrees, and very much in proportion to the intensity of the preceding inflammation, but whether exactly in that ratio it is difficult to determine. And we witness something of the same kind in mucous membranes. At least I have observed that the cicatrices of old ulcers of the intestines are always discoloured, and either gray, blue, or almost black, apparently according to the degree of severity of the local disease which they followed.

I have just said it is difficult to determine if the effect of discoloration be exactly proportioned to the inflammatory effect. There are circumstances in favour of its not being so. The erythema produced by strong acetic acid, and the vesication occasioned by the leaf of the common walnut tree, are followed by discoloration unusually dark and durable. Nor are there facts wanting which indicate that the change of colour may take place without inflammation, and go on increasing in intensity gradually, from continued exposure

is their colour changed by a solution of chlorine in water,—contrary to what is commonly asserted of the colouring matter of the skin of the Negro. Both bear a high degree of temperature, apparently without change, viz. that nearly of a dull red heat.

\* Thus it may be obtained in large pieces, very much resembling a membrane; but the connecting medium, I suspect, is coagulable lymph.

\* Is not the dark colour of the inhabitants of the Arctic regions as much owing to the inflammatory or irritating effect of the extreme cold of winter, as to the scorching influence of the continued sunshine of summer?

to the sun, or even too bright light, without inflammation having been once produced. I remember an instance demonstrative of this, in the person of an excellent and most amiable young officer, (now no more,) a case of tubercular phthisis, complicated with other organic disease, who, in hope of deriving benefit from sailing and sea air, was taken from his room, where he had been confined many months, and conveyed on board ship, where he was placed under a convenient covering constructed on deck, sheltered always from the direct rays of the sun, but exposed to the bright light of the summer sky of the Mediterranean. In a short time, thus situated, he lost the pallid hue of the sick chamber, and became almost as deeply tanned as a native of southern Europe; and I was particular in ascertaining that the change had not been preceded by the slightest erythema, or any sensible desquamation. I may mention, in confirmation, the result of exposing, a second time, to the sun's rays the part tanned, as in the experiment first related.

On the 18th August, the part first acted on was exposed for two hours, between 10 and 12 o'clock, when the sky was unclouded, and the temperature, in the shade, about 80°. Immediately after this exposure, the tanned part was browner than before, and the adjoining white part, now exposed for the first time, was slightly red. On the 19th, the tanned part was distinctly browner and redder, a very little warmer than natural, and very slightly tender. The adjoining part was florid red, slightly painful, and hot. On the 23d, the tanned part was merely brown, a shade darker than before, while the adjoining part was undergoing desquamation, and beginning to lose its vivid inflammatory hue. And, farther, in confirmation, I may relate, that I have been at some pains to learn from natives of these islands, especially of the lower classes, who are very much in the open air, what effect they have experienced from the sun's action. The result of my inquiry is, that very few of them have ever experienced the blistering or scorching effect of the sun; and when they have experienced it, it has commonly been on a part of the body not accustomed to be exposed to light; and on some occasion of unusual exposure, as that of bathing in the open sea. From all which, may it not be inferred, that the sun acts both indirectly, by the medium of inflammation, in changing the colour of the skin, and directly, without the intervention of inflammation, in producing the same effect, or in heightening it when produced?

*Lastly,—Of the consequences of Discoloration of the Skin.*

Sir Everard Home has published an interesting paper in the Philosophical Transactions for 1821, in which he proves, that, when the skin is painted black, it is defended from the scorching effect of the sun's rays; and from whence he infers, that the dark rete mucosum of the Negro possesses the same protecting power.

I may remark, that I have made experiments similar to those of Sir Everard Home, and have modified them, and all of them with the same results. I may mention, that all the opaque colours which I have applied to the skin, whether red, orange, blue, or green, have afforded protection from the scorching influence of the sun's rays, equal to that afforded by black; and, I may notice cursorily, that the habit which the ancient Britons indulged in, of painting their bodies, may thus be referred to a purpose of utility, independent of show and ornament,—the paint with which they bedaubed themselves answering in part, in place of clothing.

But though I have confirmed the experimental results of Sir Everard Home, it appeared to me, when reflecting on the subject, that his inference was not so well established as at first view might be conceived. It is founded on analogy, and that analogy not perfect; as there is this difference between the skin of a white person painted, and of a negro with a black skin; that, in the one instance, the black surface is laid on the semitransparent cuticle, whilst, in the other, it is situated under the cuticle, and on the surface of the cutis. In the one instance, the extinguishing medium is external to the insensible covering of the body; in the other, it is in contact with the sensitive surface, and may be considered as part of it. Circumstances, too, relative to the very great penetrating power of the sun's rays, have had rather a similar tendency to augment my doubts of the strict accuracy of this analytical conclusion. As the facts which I now allude to, appear to me to be new and curious, I shall mention some of them. When the sun's rays are concentrated by a lens, they penetrate, I find, through bone, as a portion of the cranium;\* through nine folds of black crape; and, what is most extraordinary, through rolled platinum. It was easy to ascertain their penetrating through the former substances, by a luminous point appearing on a surface beneath; but through the opaque platinum no light passed, yet the rays of heat passed, which was best indicated by the sensation produced, when the metal was placed on the sensitive skin, the only part of which affected was that corresponding to the focus of the lens, the metal itself not becoming sensibly warmer. Taking, then, into consideration the difference between the painted cuticle and the dark "rete mucosum," and this very remarkable penetrating power of the sun's rays, it appeared to me that more direct experiments than those of Sir Everard Home were requisite, to ascertain, beyond all doubt, if the function of the colouring matter of the skin of the Negro is really such as it has been inferred to be. With a view to this, I have subjected the skin of the negro to the direct rays of the sun, and I have made a similar trial on a mole on a

\* This circumstance may help to explain the effect of the sun on the brain, in producing that malady commonly called *coup de soleil*.

fair skin: After two hours exposure to the sun, its rays moderately concentrated by a lens, (for the experiment was made in winter when the temperature was between  $50^{\circ}$  and  $60^{\circ}$ ;) the part acted on, in which a dark brown mole was situated, became slightly red; the following day it was red, and just perceptibly painful; and about the fourth or fifth day, desquamation of the part commenced. The desquamation took place over the mole as well as the adjoining fair part, and the mole was evidently rendered of a darker colour. On the 27th December, when the sky was clear, and Fahrenheit's thermometer in the shade at  $56^{\circ}$ , a similar experiment was made on the fore-arm of a Negro, and continued the same time. The skin acted on was a little hotter than the rest, just perceptibly darker, and it felt, he said, slightly sore. On the following day, the part appeared to be very little darker, and he said that it was slightly painful and swollen, but this last mentioned effect was not perceptible to my eye. On the 31st December, the pain had ceased; there was not the slightest appearance of desquamation, and it was only just perceptibly darker than the adjoining skin.

From these results, I am disposed to infer, that the colouring matter of the skin of the Negro affords some protection from the scorching effects of the sun's rays, but not complete protection, and that were his skin as much disposed to inflame from the action of the sun's rays as the skin of the fairest European, this colouring matter would not prevent occasional vesication. Some of the facts already mentioned tend to support this opinion, especially the circumstance that exemption from the scorch-effect of the sun is not confined to the African Negro, but is enjoyed equally by all the various races of men,—the inhabitants of hot climates, who are much in the open air, and exposed to bright light rays, whether the colour be almost black, like that of the lower classes of Singalese, or of a dull straw colour, like that of the Bosjesman of Southern Africa, or of a ruddy brown, as in the instance of the Albanian shepherds of the mountains of Greece.

Nature, then, I may remark, is very provident, adapting the skin, impressed by the sun's rays, to bear them afterwards without inconvenience, or at least without painful suffering, the impression having apparently a protecting effect from farther annoyance, like the first attack of many of the infectious exanthemata; but with this difference, that the susceptibility to a renewal of the action is not long suspended, unless the cause is in constant activity. How long it is suspended is difficult to determine: it is suspended in different degrees, probably in persons of different complexions and temperaments, least in the fair, more in the brown European races, and most of all in the deeply coloured Asiatic and African tribes. Besides the foregoing, there are other means which nature employs to counteract the influence of the sun's rays on the human body, and to keep down animal heat within the bounds of health, in situations where otherwise it

would be most apt to be in excess, and mount to a feverish height. These means seem to be of two kinds, one external, the other internal. Sweat constitutes one of the first; it not only cools the surface by evaporation, but by dispersing the sun's rays, it prevents their scorching effect, which is easily shown, by comparing the effect of the sun's rays, concentrated by a lens on a dry and wet skin. The internal means are not very obvious; but that they do exist, and often act beneficially, I have no doubt. Within the tropics, when on a journey, I have tried the temperature of my palenkeen bearers, just before setting out, when they were agreeably cool; and I have tried it again after three or four hours' exertion exposed to the sun, at a temperature between  $80^{\circ}$  and  $90^{\circ}$ ; and I have found their temperature rather diminished than increased, as indicated by a thermometer, the bulb of which was placed under the tongue, or in the axilla.\* Whatever occasioned this reduction, or keeping down of animal heat, this I allude to as the internal means of counteracting external heat. However it is explained (and it would not be difficult to offer a plausible explanation of it,) the fact seems to be curious; and if it is as new to others as it was novel to me, when I first made the observation, I need offer no apology for introducing it. Other circumstances in addition recur to my mind, when reflecting on this subject, by which nature fits man to bear with impunity, and with little inconvenience, the heat of the hottest regions of the globe of which he is a native. Within the tropics, the cuticle appears to become thinner, so as to confine the animal heat less, excepting on the sole of the foot and palm of the hand, when exposed unprotected to the action of a hot surface, or subjected to much pressure, when it becomes exceedingly thick. I remember observing, with surprise a Negro, lying with his feet so close to a fire, that the soles were heated to such a degree, as to be almost scalding to my touch, whilst to him they merely transmitted an agreeable warmth, propitious to the comfortable sleep in which he was indulging. In the inhabitants of the tropics, the exhalant arteries of the skin appear to be unusually expanded, and the whole apparatus peculiar to this texture unusually developed; and I believe the blood itself is less viscid, more fluid, and flows more freely through the

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\* Observations which I made many years ago within the tropics, with some care, indicated, that moderate exercise raised the temperature of the internal parts of the body, as well as of the surface; and that severe and long continued exertion rather had a contrary effect, as in the instance above recorded. Supposing it to be generally so, it will serve to account for the impunity with which a person having taken the first mentioned degree, may plunge into a cold bath, and the fatal effect (sometimes witnessed) of doing the same, when the exercise has been of the latter description.

cutaneous and subcutaneous vessels, so as to promote perspiration; by that means contributing to the cooling of the surface; and being cooled itself, it contributes again, when it flows back to the heart, to the reduction of the temperature of the internal parts. I deduce this opinion of the blood being more fluid at a comparatively high temperature as  $88^{\circ}$  or  $98^{\circ}$ , than at a low one, as  $38^{\circ}$  or  $48^{\circ}$ , from experiments on the blood, showing that a certain degree of cold thickens blood, and that a certain degree of heat renders it more liquid; so that in the one state it is better fitted for torpid hibernating animals, and in the other for animals in whom the functions of life are performed with energy. Moreover, the constitutions of the inhabitants of the tropics, and more especially of the Africans, are different from the constitutions of the fair races of the temperate zone. The African enjoys the best health, is in the highest spirits, and capable of the greatest exertions, in hot moist regions, where the temperature is seldom below  $80^{\circ}$ , and is almost entirely exempt from those fevers of the intermittent and remittent type, which have been, and probably always will be, the scourge and destruction of Europeans in hot climates.\* But reverse the situation, and place the African where the European recovers his lost energy, shakes off the languor of the tropics, and is restored to health and strength, there the African droops, becomes languid, feeble, and diseased, and soon sinks into the grave. And thus it is, no doubt, that each race is propagated and multiplies in the situation most suitable to the development of its faculties and powers.

From the Transactions of the Medical and Chirurgical Society of London.

**OBSERVATIONS ON DEPOSITIONS OF PUS AND LYMPH, occurring in the lungs and other viscera, after injuries of different parts of the body.** By THOMAS ROSE, Esq. M. A., Late of Balliol College, Oxford: Surgeon to St. George's Hospital.

It has long been known to pathologists and surgeons, that abscesses occasionally occur in some of the principal viscera of the thorax and abdomen in consequence of injuries of the head; and that, from the same cause, purulent effusions sometimes take place into the cavities of the pleura and peritoneum.

If we consult the writings of Morgagni, we shall find that so curious a fact did not escape the notice of that distinguished anatomist.† He tells us that Valsalva was induced, by his

own observations, to say that the viscera of the thorax were sometimes affected in wounds of the head, and that he might have been so also by those of others, as Nicolaus Massa had, in 1553, met with apostemata in the thorax of a man who died delirious and paralytic, in consequence of a wound received upon the right side of his head, who had been known to be previously in good health, and not to have complained of pain in his breast, nor been troubled with cough, even after he lay ill of the wound: and as Marchetti, whose observations were better known than those of Massa, and were contained in the *Sepulchretum*, had often found the lungs and the pleura eroded after injuries of the head, and half the cavity of the thorax filled with pus, and had expressed his conviction that, in such cases, the matter descended from the head into the cavity of the thorax.

Morgagni further informs us, that there are some who have found pus in the belly, as the same Marchetti, who had even found a taint and purulent pustules in the spleen; that Bohn mentions the pleura, the lungs, and the spleen promiscuously, and that none of these omit the liver, which, by most others, is mentioned, as the only viscus into which pus can be carried after wounds of the head.

To show, however, that the latter opinion is erroneous, Morgagni states, that it never happened to him to see the liver thus affected, and that Valsalva in his numerous dissections only met with it once, and then matter was at the same time translated into the lungs also, and, in great quantities, into the cavity of the thorax itself. He refers us to Molinelli for a further confirmation, who had seen pus translated into different viscera, and not into the liver, and in some cases into the liver certainly, but just in the same manner from other wounded and ulcerated parts as from the head. According to Molinelli, however,\* the viscera thus affected were always in the number of those contained in the belly.

Morgagni disproves by his own dissections, and by those of Valsalva, the notion that had been entertained by Marchetti, of the matter descending from the wound in the head into the cavity of the thorax. He illustrates his observations by four cases from Valsalva, in which the lungs were found diseased after wounds of the head. In one of these the patient, who was a young man, survived the accident more than two months. On dissection, it appeared that matter had formed under the dura mater, and that the lungs were hollowed out by various small abscesses. It is possible that, in this case, some previous disease may have existed in the lungs, and been brought to a more speedy termination by the mischief in the head; but the other three cases, which he refers to, were much more rapid in their progress, and in them the abscesses in the lungs obviously arose from the injuries which

\* In regions more unwholesome to Europeans than the Maremma of Italy, where not one European in a hundred escapes fever, and the majority attacked die, a case of intermittent or remittent fever occurring among Africans is rare indeed.

† Vide Morgagni on the Seats and Causes of Diseases, translated by Dr. Alexander, Vol. III. p. 100. et seq. Ed. Lond. 1769.

\* Vide Morgagni, Vol. I. p. 786. Same Edition.

had preceded them,—one patient dying on the 14th, one on the 22d, and the third on the 25th day after the accident, and none of them having previously shown any tendency to disease of the chest.

In the first volume of the *Memoirs of the French Academy of Surgery*,\* M. Quesnay, in a treatise on the operation of the trepan, gives a case of abscess in the liver, which followed a fracture of one of the parietal bones. The patient was conveyed to the *Hôtel Dieu*, and placed under the care of M. Boudon. On the tenth day after the accident he was perfectly tranquil, but he afterwards fell into a state of heavy and very disturbed sleep, accompanied by occasional rigours. It being supposed that effusion had taken place under the *dura mater*, M. Boudon, on the 14th day, removed two portions of the parietal bone with a trephine, and divided the *dura mater*, by which he gave exit to a spoonful of extravasated blood. After the operation, the rigours continued to recur, the patient complained of a sharp pain in the right hypochondrium, became comatose, and died on the 17th day from the time of the accident. It had been suspected, previous to his death, that a deposition of matter was forming in the liver, and, on a post-mortem examination, an abscess was found in the substance of the great lobe of that viscus.

This case is very analogous to one which occurred several years ago in a public hospital of this metropolis, where an eminent surgeon performed the operation of trepan under similar circumstances:—A dustman was brought into the hospital in consequence of having received a blow on the side of his head, which had detached a large flap of the scalp, and denuded a considerable portion of one of the parietal bones. The man was for a day or two extremely noisy and delirious, but these symptoms gradually left him, and he appeared, for a fortnight, to be recovering favourably; after that period, febrile symptoms came on, with violent rigours, which were followed by profuse sweats. The formation of pus being clearly indicated, it was judged advisable to remove a part of the parietal bone where the pericranium had been most detached. This was done, but the inner table of the bone was found adhering to the *dura mater*, which was perfectly healthy, and the operation of course afforded no relief. After the patient's death it was ascertained that a very large collection of matter had formed in the cavity of the pleura, and that the brain and its membranes were free from disease.

M. Bertrandi and M. Andouillé have each given a paper in the third volume of the *memoirs* already referred to,† expressly treating of abscesses in the liver formed in consequence of injuries of the head. The former

of these authors attributes their formation to the obstruction to the return of blood into the right auricle, by the *vena cava inferior*, in consequence of the additional quantity which the *cava superior* has to bring back from the head. Passing over this theory, in the correctness of which M. Andouillé fully concurs, we shall find in the paper of Bertrandi some valuable information, and several very interesting cases of this disease. He remarks that authors, who have treated of these abscesses, have seldom been aware of their existence until they have discovered them in examinations made subsequent to the death of their patients; and he states that in his dissections he has often found such abscesses in those who have died of wounds of the head, where no suspicion whatever of their formation had been previously entertained. He further remarks that abscess of the liver from this cause is situated deep in the substance of that viscus.

If we refer to the valuable treatise on wounds of the head contained in the surgical works, or exposé of the doctrine and practice of Desault, published by Bichat, we shall find that Desault considered abscess of the liver to be one of the most common effects of injuries of the head; certainly much more common than is consistent with the experience of others. In speaking of the erysipelas which attends wounds of the scalp, he observes, “qu'il est rare que les symptomes deviennent violens, sans que le foie ne s'affecte, ou même qu'un dépôt ne s'y forme.”

Desault regards these formations of matter as a consequence of the disturbance excited in the nervous system, and observes that they form a complication which in cases of concussion of the brain is almost inevitably fatal. Richerand, in his *Nosographie Chirurgicale*,\* endeavours to rebut this theory, and to prove that these abscesses must depend upon some injury which the liver had sustained at the time of the accident. But this explanation is certainly erroneous, as it will be seen that they occur under circumstances where such a supposition cannot possibly be entertained.

It is curious that, whilst Desault represents abscesses of the liver as one of the most common consequences of severe wounds of the head, our countryman, Mr. Pott, who has treated so fully of the various effects resulting from these wounds, should have been entirely silent on the subject; and that the appearances which are presented by the viscera thus affected, should in like manner have escaped the notice of Dr. Baillie. Mr. Pott† has given one case in which a fatal peripneumony followed the operation of trepan, but he appears to have regarded that disease as an accidental occurrence, and not as a consequence of the mischief done to the head.

\* *Mémoires de l'Académie Royale de Chirurgie*, Tom. I. fol. 147. Paris, 1819.

† *Vide Mémoires de l'Académ. Royale de Chirurgie*, Tom. III., fol. 439 et 452.

\* *Vide Nosographie Chirurgicale*, Tom. II. fol. 220. Paris, 1812.

† *Chirurg. Works of Percivall Pott*, Vol. I. p. 127. London, 1779.

Mr. Samuel Cooper has expressed his suspicions,\* "that the affection of the liver and *primæ viæ* (after injuries of the head) has been exaggerated by the French surgeons, since English surgeons, in their dissections, certainly do not find the liver frequently inflamed and suppurated in patients who have died of concussion."

But although abscesses of the liver, under such circumstances, may not by any means be so common as Desault would lead us to suppose, yet their occurrence in that, as well as in other viscera, after injuries of the head, seems to me to have been too little considered in the writings of English surgeons. Nor is it after injuries of the head alone, as the learned authors, whom I have hitherto quoted, would lead us to infer, that such abscesses are formed. They equally follow wounds of other parts of the body; and, during the Peninsular war, I met with several instances of their occurrence, in the lungs particularly, after amputations, and after other wounds of the extremities. I communicated these circumstances to Sir James M'Grigor in 1813, being then with our troops in Spain, requesting, that as the opportunities for observing the phenomena resulting from every description of injury to the body were at that time so extensive, he would inquire if similar affections of the lungs and different viscera had been observed by others.† I pointed out to Sir James as an excellent illustration of the disease in question, a case given by M. Larrey in the first volume of his "*Mémoires de Chirurgie Militaire*," three volumes of which interesting work I had then recently received, in which case abscess both in the liver and in the lungs followed amputation of the arm. The case‡ is that of General Caffarelli, and occurred during the occupation of Egypt by the French. The General died on the nineteenth day after he had undergone the operation, the wound from which was going on favourably, and on the sixth day after the attack of those febrile symptoms, which, as was ascertained after death, had indicated or led to the derangement of the internal organs. M. Larrey attributed the fatal result, and the disease of the viscera, to the effects of the Egyptian climate, with fatigue and other causes; and did not at all seem to suppose that they were connected with the previous wound or operation. But in the fourth volume of the same work, which was published at a subsequent period, that is after the peace of 1814, he gives§ an instance of a large abscess of the

liver, in a Prussian soldier, occurring after a compound fracture of the arm. With a view to destroy in this case an artificial joint, M. Larrey introduced a seton between the fractured portions of bone, which, after a few days, was followed by enormous tumefaction and suppuration of the arm, and by an abscess in the liver which burst into the cavity of the abdomen. The period of the man's death is not stated. M. Larrey observes, that no doubt could be entertained of this abscess of the liver being attributable to the irritation and inflammation of the arm, as the man had not previously experienced any indisposition which could lead to the suspicion of his having hepatic disease. He gives in the same volume three other cases of abscess of the liver, following wounds of the head, and he states his opinion, that such abscesses are owing to the irritation excited in the liver by sympathy with the inflammatory action which had been established in the fibrous membranes of the cranium, or of the bones of the upper or lower extremity, but chiefly, he says, those of the same side, and by the metastasis to this viscus of the "*miasmes ichoreux, ou d'un fluide plus ou moins acre et subtil*." He adds that the communication of these morbid humours with the hepatic system, takes place more easily when they have not to cross the median line.

Mr. Hennen has also given three very interesting cases of the same nature in his work on the Principles of Military Surgery.\* Two are cases of disease of the lungs, and one of disease of the liver, and all followed amputation. They do not tend to confirm M. Larrey's notion of the morbid humours not crossing the median line.

It appears therefore that the occurrence of abscesses in the viscera as a consequence of injuries of the head, more especially where these abscesses take place in the liver, has long been generally known, and that the circumstance of their following wounds of other parts of the body has of late been clearly pointed out, but the silence of the most distinguished pathological writers of this country respecting them, and the little notice which has been taken of their peculiar appearances, have induced me to think that the subject, although it has not any novelty to recommend it, might not be deemed entirely unworthy of the attention of this Society.

I have seen repeated instances of the disease in the lungs, in the liver, and in the spleen, and after various accidents. I have not been able to discover any peculiarity of constitution which could be regarded as predisposing to it. Many of the patients were young and healthy individuals, who, until the time when they met with the accidents, had never been affected with disease. Some of them were treated on the strictest antiphlogistic plan throughout, in consequence of the

\* First Lines of the Practice of Surgery, Vol. I., p. 399. London, 1819.

† Sir James, in answer to this communication, informed me that Staff-surgeon Irwin had lost a patient of disease of the lungs following amputation of the thigh.

‡ Vide *Mémoires de Chirurgie Militaire*, et Campagnes de D. J. Larray, Tom. I., fol. 306. Paris, 1812.

§ Vide *Mémoires*, &c. de M. Larrey, Tom. IV., fol. 229. Paris, 1817.

\* Vide Principles of Military Surgery, by J. Hennen, fol. 271. Lond. 1820.

nature of the accident they had experienced. In others (in compound fractures for instance,) as soon as the first inflammation had subsided, means were used for supporting the strength of the system. No difference as to the formation of the internal abscesses could be observed. In all the cases which I have seen, these abscesses took place at some period between the end of the second and that of the fifth week after the accident which gave rise to them.

The theories which ascribe their formation to injury done to the liver itself at the time of the accident, to obstruction to the entrance of the blood into the right auricle through the vena cava inferior, or to a direct communication for the transmission of matter from the head of the cavity of the thorax, are all obviously absurd. That of Desault, which attributes them to the disturbance of the nervous system, resulting from the injury, is probably the only explanation which can be given of their cause. They are to be classed amongst the effects of constitutional irritation arising from local injury, and are certainly striking illustrations of the irregular action in the vascular system to which that irritation may give rise. The attention of the members of our profession has lately been directed to this most important subject by the very valuable work of the President of this Society,\* and it is to the principles which he has so ably illustrated that I should look for an explanation of the phenomena which I am now attempting to describe.

It is not very uncommon to find inflammation or congestion taking place in particular organs immediately after the constitution has rallied from a shock given to it by a severe accident or surgical operation, though that accident or operation be in a part of the body remote from these organs. In such cases the symptoms of inflammation are sufficiently marked; and should the disease proceed to a fatal termination, the appearances in the affected organ would, no doubt, correspond with those produced in it by inflammation or its consequences arising from any other cause. But the affections of the viscera, to which I have referred in this paper, have a peculiar character; and it appears to me that this may, in some degree, be accounted for by the rapidity wherewith, in the state of the constitution during which these abscesses occur, any congestion or inflammation, in whatever part it took place, would be followed by effusions of purulent fluid and of lymph. It is at the time when the parts, in which the injury took place, are in a state of suppuration; and in particular when, from the nature of these parts, or from the confinement of the matter, great irritation of the system has been for some time kept up, that such internal abscesses are apt to form; and it often happens,

as is remarked by Bertrandi, that they have not been discovered until a post-mortem examination. But although constitutional disturbance, evidently referrible to an unfavourable state of the wound has, in all the cases which have come under my observation, preceded the formation of these visceral diseases, yet a favourable change has often taken place in the wound before the symptoms of the internal abscess have begun to manifest themselves; and we are sometimes able to detect the existence of the latter by the presence of rigours and other symptoms of suppurative fever at a time when the wound itself is disposed to heal.

The examination after death of those who have been affected with this disease, presents appearances which are well worthy of notice, though it is not easy to convey a correct idea of them in words. The disease consists, apparently, of depositions in the cellular texture of the affected organ, partly of a white or yellowish coloured lymph, and partly of pus. These depositions vary in size from beyond the bulk of the largest walnut to something less than a common pea. Where the lymph is most abundant, they may be described as a soft white tubercle of irregular shape, not contained in a cyst, but embedded in the cellular substance of the part, and gradually blending with its natural structure. When pressed, some pus exudes from them. Where the pus collects in greater quantity, it is lodged in an irregular cavity, probably in the middle of some of the tubercles, and the walls of the abscess are formed of flakes of lymph. The number of these tubercles and abscesses vary in different instances, there being sometimes only one or two, and sometimes the whole viscus being filled with them. In the lungs they are chiefly formed in the parts adjacent to the pleura pulmonalis, and there is often at the same time an effusion into the cavity of that membrane of a sero-purulent fluid mixed with lymph. In the liver and spleen they are dispersed throughout the substance, sometimes showing themselves in one or more yellowish patches, not elevated, on the convex surface of the great lobe of the former viscus, and at other times lodged in its substance. The parts adjacent to them show evident marks of increased vascularity.

I have said nothing of the treatment, and have little to suggest on that head. Our efforts must be directed, first, to subdue any excess of arterial action, and secondly, to quiet the disturbed state of the nervous system. When the abscesses are once formed, we shall find the truth of the observation of Desault, that they are almost invariably fatal.

I fear that I have prolonged this paper to much too great an extent, I shall therefore conclude by laying before the Society a short abstract of four cases of the disease, arising from injuries to different parts of the body.

\* Vide An Inquiry concerning Constitutional Irritation, by Benj. Travers, Esq. F.R.S. Lond. 1826.

*the pleura, after wound and amputation of the arm.*

A soldier of the Coldstream guards, received a musket-shot wound in the elbow-joint of his left arm, at the storming of St. Sebastian's, on the 31st of August, 1813: The ball fractured both the condyles of the os brachii, and the coronoid process of the ulna. He was attacked with a considerable degree of irritative fever a few days after, but the inflammatory symptoms in the arm did not run particularly high.

After rather more than three weeks, these febrile symptoms continuing, with copious discharge from the wound, and his general health and strength declining, it appeared to me necessary to amputate his arm, and I was in hopes that the disturbance of his system would subside, when the only exciting cause of it, which I could discover, was removed. This was done on the 24th of September. On the second morning after, he appeared cheerful, and the febrile symptoms had diminished; but towards the middle of that day, he was seized with a slight rigour, which lasted for ten minutes or a quarter of an hour, and was succeeded by a most profuse sweat. The rigour returned on the evening of the 27th, and during that night, and through the whole of the 28th, the perspiration was constant. On the latter of these days, the stump was examined; union had taken place everywhere, except at the openings for the ligatures, and there was no tenderness in the part of the arm above it. He was ordered acid drinks, ripe fruit, and light nourishing diet. On the 29th, the same symptoms continued, with a dry shining tongue. All the ligatures came away, except that on the brachial artery. On the 30th, his breathing was found to be more hurried, but he took a full inspiration when desired, and it occasioned no pain. He had slept a good deal in the night, but his sleep was disturbed, and he moaned frequently. He still continued to perspire copiously. His bowels had from the first been perfectly regular. At this time the stump was flaccid, but union was going on. During that day the hurried breathing increased, and at four the next morning he expired; being the seventh day after the operation, and the thirty-first after he had received the wound.

I examined the body on the day he died. In the cavity of the thorax, on the left side, more than a pint of sero-purulent fluid was found effused, mixed with loose flakes of coagulable lymph. The pleura pulmonalis and pleura costalis were glued together in parts by the lymph, and were highly vascular. Numerous circumscribed abscesses were found imbedded in the cellular structure of the lungs; principally in those parts of them which are nearest to the pleura. These abscesses were perfectly distinct from the parenchymatous substance of the lung, by which they were surrounded, and which appeared in no way affected, except by showing higher vascularity. They did not appear to be in-

vested by any cyst of condensed membrane; and in many of them instead of pus, or mixed with pus, was a whitish substance, probably common lymph. On the right side of the thorax, the appearances were somewhat similar, but the effusion was to a much less extent. The viscera of the abdomen were healthy.

*CASE II.—Abscesses in the Lungs, Liver, and Spleen, after compound fracture of the leg.*

William Deane, 21 years of age, was admitted under my care, into St. George's Hospital, on the 23d of July, 1825, with a compound fracture of the tibia and fibula of his right leg, occasioned by a load of gravel having fallen upon him.

On the 27th considerable tumefaction had come on in the limb, inflammation having diffused itself through its cellular tissue, and a good deal of bloody serum had begun to ooze from the wound. This was followed by a sharp attack of erysipelas, which spread over all the thigh, and over the principal part of the integuments of the abdomen. By free incisions wherever matter could be detected, venesection once or twice repeated in moderate quantity, and saline diaphoretics, these symptoms subsided favourably, but left him a good deal emaciated. On the evening of the 2d of August the erysipelas had disappeared; he had little fever, but profuse discharge, and he then began to take light nourishment with appetite.

On the morning of the 3d of August he was seized with a severe rigour, followed by sickness. His pulse at noon was 120; his tongue brown and dry; he had great heat of skin, and restlessness, and complained of a sense of uneasiness about the pit of his stomach. An aperient medicine was given him, and afterwards effervescing draughts, with small doses of antimonial wine.

On the 4th he was better, and his pulse had sunk to 108. There was a slight relapse of erysipelas over a part of the abdomen. The wound discharged profusely; but no matter was lodged, as there were free and depending openings.

On the 5th he was still better, his pulse was 96, and the erysipelas was again subsiding. Some wine was allowed him; and light nourishing but liquid food continued.

On the 6th his countenance was not so favourable. He was ordered sulphate of quinine. This produced no good effect, his tongue gradually becoming more parched and dry.

On the 9th, he complained of an unpleasant sense of rising from his stomach, with an excessive heat in his throat, but he could bear pressure on the abdomen without pain. In the evening of that day he had an attack of stupor, and lay for many hours in a state of nearly complete insensibility, with contracted pupils. He died on the evening of the 11th, being the twentieth day from the date of the accident.

The body was examined on the following

day. The vessels of the pia mater and brain were more turgid than natural, and there was a considerable effusion of serum into the ventricles. In the thorax, there were several circumscribed abscesses in the lungs on each side, but chiefly in those on the right. These were situated in the outer part of the lungs, towards the pleura, and varied in bulk, from that of a small pea to that of a large nut. Their contents were evidently a loose sort of lymph, through which pus was everywhere beginning to be diffused, as could be shown by its issuing when they were gently pressed. On the upper part of the convex surface of the great lobe of the liver a large mass of a similar character was visible, of a perfectly white colour, appearing under the peritoneal covering. It was two or three inches in diameter, and when cut into, was found to extend at least two inches in depth, into the substance of the liver, which everywhere bordering on it, had a natural appearance, and did not seem to be in any way condensed. A somewhat paler line marked where the two structures, that of the liver and of this mass, were blended. The mass consisted of loose lymph, with pus diffused through it, as in the lungs. On the right edge of the great lobe, under the short ribs, there was another mass of the same nature, but of a smaller size, and one or two similar patches under the capsule of the spleen. No attempt at union had taken place in the fracture.

*CASE III.—Abscesses in the Lungs, Liver, and Articulation of Clavicle and Sternum, with effusion into the Thorax, after a bruise and wound of the foot, and a fractured fibula.*

George Stacey, 18 years of age, and apparently of a healthy constitution, was admitted under my care into St. George's Hospital, on the 17th of July, 1827, in consequence of an accident from a cart-wheel having passed over the outside of his left foot. There was a small wound under the little toe, made apparently by some sharp substance, which had penetrated under the first phalanx, about an inch into the sole of his foot. Considerable ecchymosis had taken place over all his instep and foot, and there was a simple fracture of his left fibula two inches above the ankle. Leeches, cold lotions, and aperient medicines were ordered, and the limb was kept quiet, and supported on a pillow. The leeches were repeated several times.

On the 23d he had shiverings, after a restless night; and these were followed by diffused cellular inflammation over every part of the foot, and by erysipelas extending up the leg and thigh, with enlarged glands in the groin. The integuments in different parts of the foot were divided, to set the inflamed parts at liberty; and on free openings being obtained for matter which had formed under the fascia plantaris, the febrile disturbance began to subside.

On the 4th of August he was reported convalescent, and at his earnest request was or-

dered some meat for his dinner on the following day.

On the 5th he had a severe rigour, which lasted for more than an hour. A purgative medicine was ordered, and he was again put on light diet; and it is to be observed that the rigour came on before he had taken the meat.

On the 7th, the rigour returned at the same hour as on the 5th, and lasted about the same time. The limb continued perfectly quiet, all the wounds were healing, and no cause could be discerned for these febrile attacks. He had never had ague, but stated that where he had been working that disease prevailed. He was directed to take two grains of the sulphate of quinine every four hours.

The rigour returned again on the 8th, followed by much heat and a very quick pulse, and continued afterwards to recur at irregular intervals, being generally succeeded by profuse sweats.

On the 10th it was observed that he had slight ptosis of the upper eyelid of the right eye; his pulse was quick, nearly 150; his tongue dry; his countenance unfavourable, and with a yellowish tinge. There was no appearance of matter forming in any part of the leg, and he could bear pressure over the abdomen. In the evening some degree of emphysema and a little effusion of fluid were detected at the articulation of the right clavicle with the sternum. He had met with no accident in the part to account for this. On the evening of the 11th he died, being the 26th day after the accident.

The body was examined on the following day. In the head the arachnoid appeared more opaque than natural, and there was some lymph effused on the under surface of the anterior lobes of the cerebrum and round the junction of the optic nerves; matter was found effused into the cellular membrane over the sternal extremity of the right clavicle, and into the synovial cavities on each side of the inter-articular cartilage between that bone and the sternum.

The pleura on both sides of the thorax was very vascular, and distended with a considerable quantity of sero-purulent fluid mixed with loose flakes of lymph. This was more abundant on the left than on the right side of the chest.

The lungs on each side contained numerous small abscesses and soft tubercular masses, principally adjoining the surface of the pleura. These varied in size from that of a hazel-nut to less than that of a small pea; and in the middle of some of the tubercles there was an irregular cavity filled with pus. One small abscess was found in the substance of the great lobe of the liver, at some distance from its surface.

*CASE IV.—Abscesses of the Liver and Spleen, after fracture of the Skull, &c.*

A French gentleman, upwards of 30 years of age, was brought to St. George's Hospital on the evening of the 27th of July, 1825, and admitted under the care of Mr. Keate. He was in a state of complete insensibility, in con-

sequence of having fallen from his horse and pitched on the side of his head. He died on the 18th of August, the twenty-third day after the accident.

On examining the body, it was found that a fracture had taken place, commencing a little above the posterior and inferior angle of the left parietal bone, and extending across the occiput to the foramen magnum. There was a considerable quantity of blood extravasated at the base of the skull. The brain itself was ruptured at the lower part of the posterior lobe on the left side, and pus had formed at that part between it and the pia mater. Both the liver and spleen were studded over their surface, and throughout their substance, with soft tubercular masses consisting of lymph mixed with pus, and with circumscribed abscesses of different sizes.

The following very interesting cases will further illustrate the subject. They are communicated by Mr. Lawrence, who has allowed them to be annexed to this paper, a permission of which the author of it is happy to avail himself.

*Cases communicated by Wm. Lawrence, Esq.  
F.R.S. Surgeon to St. Bartholomew's Hospital, &c.*

Thomas Scarborough, æt. 33, was admitted into St. Bartholomew's Hospital on the 6th of January, 1827, in order to have a loose cartilage removed from the knee-joint. He had laboured under an inguinal hernia for six or eight years, and had been subject, during that time, to bowel complaints. He had a somewhat sallow and unhealthy look, and a whitish tongue, yet considered himself in good health, and had followed his ordinary occupation as a labourer, to the time of his admission, having experienced only temporary inconvenience from the complaint in the knee, which had existed about three years.

After some attention had been paid to the state of his health, the operation was performed on the 13th of January, and a perfectly smooth white piece of cartilage, with a small bony nucleus, was taken out of the joint, having been previously fixed on the external condyle of the femur.

17th.—The wound has united, without the slightest heat or swelling of the joint. In the evening, however, without any assignable cause, bleeding took place from the wound, and ceased spontaneously by the formation of a coagulum, which distended the incision. On the next day the sallow appearance of the countenance, which had been noticed at the time of admission, was much more conspicuous, and the conjunctivæ were quite yellow. During the following week, the joint, which was kept quite quiet, remained free from pain and swelling, and a thin fluid escaped from the wound. There was no fever.

After passing a restless night, he became very ill on the 26th, with heat of skin, thirst, loss of appetite, costiveness, white tongue, with a dry brown streak in the middle. The joint

was swollen and painful, the margins of the wound inflamed, and a purulent fluid mixed with synovia flowed from it on pressure. (Twenty leeches to the joint; aperient, and afterwards saline medicines.) He continued in nearly the same state till the evening of the 29th, when the febrile symptoms increased, and he was bled to fourteen ounces, the blood being strongly cupped and buffed. In consequence of continued febrile disturbance, twenty ounces of blood, exhibiting the same character as before, were taken from the arm on the 31st.

He was better on the 1st of February. Twenty leeches to the knee-joint, which continues inflamed and swollen, and discharges much pus.

During the night of the 2d he was restless and delirious. From this time he continued to sink: paralysis of the right side, more particularly of the arm, was observed on the 5th; and he expired on the 7th.

*Examination of the body twelve hours after death.*—The knee-joint contained a small quantity of healthy pus: the synovial membrane was thickened, vascular, and in some places dark coloured. An abscess on the outside of the knee, containing two ounces of pus, communicated with the joint; and the skin was separated from the subjacent textures, in the neighbourhood of the wound, by a cavity like that of an abscess. A small portion of the upper and anterior part of the tibia was denuded; the cartilages were unchanged in all other parts. Several small, yellowish, elevated spots were observed on the convex surface of the liver, which was slightly agglutinated to the diaphragm by recent adhesion. On cutting into them, a thin purulent fluid escaped, leaving a yellow fibrous substance, very much like the flakes of a scrofulous abscess. In some of them the fibrous substance predominated; in others, the thin yellow pus. These depositions varied in size, from that of a pea to that of a hazel-nut. They existed in great numbers throughout the whole liver, but the thick edge was more particularly loaded with them. I counted thirty on the surface of one section; there must consequently have been many hundreds throughout the liver. The other abdominal viscera were healthy. The arachnoid membrane covering the hemispheres was partially elevated by serous effusion under it: this was particularly apparent over the posterior lobe. A small deposition of healthy yellow pus, about the size of a horse-bean, was found at the side of one of the posterior convolutions of the left hemisphere.

H. A. Porter, 51 years of age, a corpulent man, addicted to drinking, was admitted into St. Bartholomew's Hospital the 19th of January, 1827, for an old ulcer of the leg, with much surrounding inflammation. He had a poultice to the ulcer, and was placed on milk diet; he was twice bled in the arm, and took opening medicine. Under this treatment the leg improved rapidly, and he felt altogether much better; but three days after the second bleeding the wound of the vein became pain-

ful, and was found to be slightly inflamed. (A bread poultice was applied to it.)

Jan. 30th.—Although the arm was easier after the application of the poultice, shivering fits came on last night, followed by heat and thirst; pulse 120. (Venesection to ten ounces: the flow of blood was arrested by syncope: the blood was not buffed. Twenty leeches to the arm. A dose of calomel and jalap. A saline draught every four hours, with antim. tart. gr. j. and potassæ nitr. ʒss.)

31st.—Inflammation has extended to the axilla, the arm being red, swollen, and painful on pressure, from the elbow to that part. (Thirty leeches to the arm, and a large blister afterwards.) A severe shivering fit was experienced this afternoon.

Feb. 1st.—Restless night from the blister; pulse 102, and small; tongue white; no appetite; bowels open. The antimony had been gradually reduced to a quarter of a grain in each dose, but it still caused so much sickness that it has been left off.

4th.—He has remained nearly in the same state, and has had several shivering fits. He complains to-day of severe pain in the left knee-joint, which is somewhat swollen. The arm is easy, and a small quantity of thin pus flows from the wound in the vein on pressure. The pulse hard, full, and 100; tongue white and dry; great thirst; bowels confined. (Senna mixture immediately; a saline draught every four hours, with tinct. digitalis ℥xij.)

5th.—He has passed a comfortable night. The left knee and thigh are greatly swollen and very painful. The joint is distended to the utmost with effused fluid, causing a large prominent tumefaction above and at the sides of the patella. All the superficial veins of the knee and thigh are excessively swollen, and form a very conspicuous net-work. The limb is slightly red, and preternaturally hot. Pulse 140, and soft; tongue white; bowels open. (Six doses of digitalis have been taken; let it be increased to ℥ xv: four grains of calomel every four hours: ʒ xvj. of blood to be taken from the knee by cupping.)

7th.—He complained yesterday of pain in the right shoulder, which continues, without swelling or redness. The knee was relieved by the cupping, and is nearly free from pain, though the swelling is not much diminished. The arm is less painful; bowels very open. (The calomel to be left off; the digitalis continued.) In the evening he was more easy; the pulse small, hard, and 120; tongue brown and dry; great thirst.

He expired on the morning of the 8th, and the body was examined ten hours after death.

The cephalic vein, which had been punctured, was thickened, and contained pus for about two inches below, and four inches above the wound, where a coagulum of blood was found, filling the cavity. Above and below these points the vessel was healthy; and the other veins exhibited no morbid change. The abdominal and thoracic viscera were healthy. The arachnoid membrane was thickened, opaque, and whitish. The cellular texture of

the pia mater was loaded with serum, and an increased quantity of fluid was found in the ventricles. The cavity of the knee-joint was filled with a tolerably thick pus, of an uniformly reddish colour, as if from an intimate admixture of blood. The synovial membrane was thickened, with an irregular and almost villous surface: it was extremely vascular in its whole extent. The cartilaginous coverings of the femur and tibia had undergone considerable absorption, so that the convexities of the femoral condyles and the corresponding excavations of the tibia were completely bare. The cellular substance covering the capsule of the knee, under the exterior muscles, was inflamed, thickened, and loaded with pus. This texture was in the same state on the surface, and throughout the whole substance of the vasti and cruralis muscles. Sections of these muscles presented a most singular appearance, their large fasciculi being separated apparently by layers of thick yellow pus. The matter, although precisely similar in colour and consistence to that produced by phlegmonous inflammation, was nowhere collected into an abscess, but was diffused through the cellular structure, as serum is in the case of anasarca. In the rest of the limb there was effusion of a bright light yellow serum. The cellular structure exterior to the orbicular ligament of the right shoulder was filled with thick yellow pus; but the cavity of the joint and the deltoid muscle were natural.

Captain L., 34 years of age, became the subject of calculus in India, and returned home to undergo the operation. He had feverish symptoms of intermittent character after his return; and I prescribed for him the effervescing saline draughts, under which the fever disappeared, and he also lost entirely the calculous symptoms. He was however still bent on undergoing the operation, which I performed for him, and removed a calculus of moderate size. It was necessary to bleed him largely from the arm on the evening of the third day. Soreness came on about the puncture in two days, and this was gradually followed by general swelling, and slight redness of the whole limb, with excessive pain and great feverishness. The local and general symptoms were not controlled by any of the measures adopted, and the case ended fatally at the end of the third week, symptoms of inflammation having come on on the same side of the chest as that on which he had been bled in the last forty-eight hours. The basilic vein was thickened by inflammation up to its termination, and the veins corresponding to it were in the same state down to the back of the hand. The coats of the vessels were red, and the surrounding tissue was indurated by inflammation. The interior of the inflamed veins was partially roughened, as if by the deposition of lymph: they contained pus throughout. The whole subcutaneous tissue of the arm was inflamed, and partially infiltrated with serum. The axillary vein and the continuation of the trunk to the heart were free from inflammation. The pleura was violently inflamed; the cavity

contained about a pint of whey-like fluid, mixed with pus and flakes of lymph.

A married woman, 25 years of age, who had been a great spirit drinker for some years, was bled in the left arm on account of an accident, and, pursuing her ordinary occupation, that of weaving, experienced a severe attack of inflammation in the vein and neighbouring part of the limb, for which she was received into St. Bartholomew's Hospital on the 2d of December, 1826, being the fifth day from the commencement of the inflammation. She died on the fourteenth, her symptoms at one time having been so much relieved that we entertained great hopes of her recovery. On the 7th the inflammation and swelling of the left arm were much diminished, and there was copious discharge from the puncture of the vein of thin matter, sometimes yellow, sometimes reddish. She now suffered very greatly from pains over the body, but more particularly in the extremities. She passed a very restless night from this cause, and suffered greatly the next day from pain in the calves of the legs. On the 11th she again suffered much from pains in the limbs. On the 12th it was found that matter had formed under the skin of the right arm, without redness, and five ounces of good pus were discharged by a puncture. At the same time a painful swelling of the left knee, from effusion into the cavity, was observed. We were not allowed to examine this part after death.

From the London Medical Gazette.

**MEMOIR ON ANEURISMS** *caused by Fractures and Gun-shot Wounds, and on their Treatment, according to the method of Ariel.*  
By M. DUPUYTREN.

Amongst those serious accidents which are liable to accompany fractures and gun-shot wounds, the tearing of a principal artery and the consequent effusion of blood, presenting the characters of an aneurismal tumour, form a complication which not only compromises the safety of the limb but also the life of the patient; and which, according to the practice hitherto in use, presents no other resource than amputation, with all its risks and consequences. In reflecting on the frequency and variety of fractures and wounds, it is evident that these aneurisms must be very common; nevertheless, authors scarcely contain any examples of the kind, either owing to their being in reality more rare than might be supposed, or, what is more probable, because attention must be awakened especially to this point in order that our observation may be directed to phenomena otherwise sufficiently striking. M. Dupuytren has only found one instance of this complication in authors: it is reported by Petit, who, in a fracture of the tibia, without any external wound, perceiving a large ecchymosis spread over the whole leg and foot, whilst at the same time these parts became cold and of a dark colour, thought that the artery (probably the anterior tibial) was opened; he therefore made an incision which laid the vessel bare,

and stopped the hemorrhage, but he does not say by what means. From this solitary example, which is defective in many important particulars, authors have generally repeated the fact of aneurism being an occasional complication of fracture or gun-shot wound, but without adducing any farther instances. After having observed that all writers agree in recommending amputation of the affected limb, M. Dupuytren relates three cases which occurred when Pelletan was surgeon in chief of the Hôtel Dieu. In the first there was a simple fracture of the left leg; a general swelling of the limb showed itself from the beginning, and continued to increase, but without any alteration of the colour of the skin. On the 26th day an incision was made in the centre of the swelling, and gave issue to some clots of blood at first, and afterwards to a jet of arterial blood, which was arrested by pressing the femoral artery; the thigh was then amputated, and the patient recovered; the source of the hemorrhage was not ascertained. In the second case the fracture was also in the left leg: up to the fifteenth day nothing particular had been remarked;—at that period the patient complained of pain in the calf of the leg; they persisted, and on the thirtieth day a tumefaction was perceived at the middle part of the leg, of a shining appearance and bluish colour. Soon afterwards all the characteristic marks of aneurism were perceived, and the swelling continuing to increase, the amputation of the thigh was performed in spite of the patient's weakness. Dissection of the limb showed an aneurismal pouch consecutive to the lesion of the peroneal artery, which had been torn by fragments of the fibula. The patient died of pneumonia forty-six days after the accident. In the third case there was also a fracture of the left leg, but accompanied by a wound, which gave issue every day to a greater or less quantity of blood: however, the consolidation of the fracture was completed the seventy-sixth day, when on a sudden a hemorrhage, attended with an enormous tumefaction of the leg, came on. The wound was enlarged, plugged, and the following day amputation above the knee was performed. The patient died the seventeenth day after the operation. Dissection of the limb showed the anterior tibial artery pierced by five or six openings, and the fractured bones united.

Thus in three cases amputation had only once succeeded, a circumstance that ought to induce practitioners to avoid it, more especially since not above a fourth of those who have suffered amputation of the principal members recover. It is matter of astonishment that in these cases ligature of the trunk of the wounded artery has not been resorted to; it was a case similar to the above that induced M. Dupuytren to depart from the ordinary routine, and to give the patient a chance of saving his limb.

*Case.*—On the 2d January, 1809, a woman, 62 years of age, made a slip in running along the street, fell, and fractured her left leg.

When brought to the Hôtel Dieu on the following day, M. Dupuytren, wishing to reduce the fracture, discovered in the calf of the leg a regular pulsation, sensible both to the touch and sight, isochronous with the contractions of the pulse, and which ceased when pressure was made on the femoral artery. These symptoms demonstrating the existence of an aneurism, caused without doubt by the rupture of one of the arteries by the fragments of the broken bone, M. Dupuytren thought that the ligature of the artery of the limb would be preferable to amputation. Independently of the cessation of the growth of the tumour, the ligature would prevent the necessity of exposing the seat of the fracture itself to inflammation and suppuration. In consequence of these reflections the femoral artery was tied in the middle of the thigh; the heat and sensibility of the limb were not for a moment interrupted. From the fifth day the tumour sensibly decreased; the ligature came away on the fifteenth day; the formation of the callus took place slowly, doubtless because the source of nutrition was in a great measure interrupted: it was scarcely formed at the end of the second month, but it was perfectly consolidated at the end of the fourth, when the patient quitted the hospital cured.

A similar instance was observed in 1815, by M. Delpech, who relates it in his Clinical Surgery. These two examples put the following principle beyond all doubt—viz. that the rupture of the arteries of a limb, caused by the fragments of a broken bone, may be cured by the ligature of the artery above the disease, even when this rupture is accompanied by an aneurismal tumour. Was it possible to conclude from the above cases, in which the skin was whole, that the same success might be obtained if the skin was torn, and the seat of the fracture in communication with the external air? It rests now to demonstrate that gun-shot wounds complicated with aneurism, do not require amputation more than fractures complicated with that accident, and that they may also be cured by the ligature of the principal artery of the limb. M. Dupuytren calls in the aid of facts to prove this position.

*Case.*—M. De Gombaut, Chef d'Escadron, received in February, 1818, a wound from a pistol ball, which passed through the upper part of the right leg, from before backwards, and from without inwards, passing between the tibia and fibula, which last it slightly injured. A very violent hemorrhage occurred at the time of the accident: a strong compression made upon both the wounds arrested it, and, assisted by the tourniquet applied to the thigh, no fresh hemorrhage outwards was perceived till the third day: from that time it was renewed at intervals, and the tumefaction of the limb, as well as the pulsation, continued augmenting more and more, and MM. Aumont and Depres, who attended the patient, called M. Dupuytren in consultation.

The foot and the leg were violet coloured, swollen, and cold. At the upper part of the leg there was a tumefaction, accompanied by

tension, and a pulsation isochronous with those of the heart. Upon this tumour were seen two openings with unequal edges, closed within a few hours only by clots of blood, which each pulsation appeared to raise up and to threaten to detach. Every thing proved that the ball had pierced one or more arterial trunks; it was evidently impossible to tie the wounded vessels. Amputation appeared the readiest resource to MM. Aumont and Depres, but M. Dupuytren proposed the ligature of the femoral artery, there being a possibility of recurring afterwards to amputation if the condition of the patient became worse. The operation was immediately performed, and had the happiest results. The ligature came away on the twentieth day. During this time the wound of the leg discharged the blood little by little; some portions of the clothing and pieces of bone were brought away by the suppuration, and three months after the accident M. Gombaut walked as well as ever.—*Archives Générales, July.*

From the Lancet.

#### NICOLAS CHERVIN'S RESEARCHES ON THE NATURE OF YELLOW FEVER.

This distinguished individual has been engaged, during nearly the whole of his life, in the study of this formidable disease; neither dangers nor pecuniary sacrifices could change his intention to visit almost every part of America where this calamity reigns. He did not return, until after ten years of incessant study and incredible toil, to his native country, to reap the fruits of his admirable zeal. By the following concise account of his travels, we intend to call the attention of our readers to the work of M. Chervin, which is shortly expected to appear.

In the year 1814, he left Paris for Guadeloupe, which he reached in December of the same year. Before he began his journey, he had eagerly studied all French, Italian, English, and Spanish works on the subject, and from them he was disposed to believe in the contagious nature of the yellow fever; but he endeavoured to make his own observations, free of all prejudices, in order to arrive at a clear result. During the year 1815, he had no opportunity of observing the disease, but in 1816 and 1817, he met with it very frequently, under the most varied circumstances; he then began to doubt the correctness of his former opinion. He then went to Martinique, Antigua, St. Christophe, St. Martin, St. Thomas, and Porto Rico. In August, 1817, he arrived at St. Domingo, where the yellow fever just happened to rage in its most malignant form. Having made many observations there, he went to Jamaica, thence to Cuba, and Port-au-Prince. He adapted his route always as much as possible to the course of the disease. During his absence from Jamaica, the fever had made terrible ravages amongst two newly-arrived regiments; on his return, it was still very violent,

and afforded him the best opportunity of examining the most important circumstances with regard to its contagious or non-contagious nature.

In November he went to Havanna, where he remained till the 12th of February, 1820. At New Orleans he arrived at the period when the fever generally appears; the epidemic was terrible; he witnessed it during six months, and then left for Savannah, being informed that the disease raged there with an unprecedented malignity. He was, however, disappointed; by neglect of the captain, his ship proceeded at once to Charleston, where he was very well received. He remained but a short time, and went to Savannah, notwithstanding the most anxious representations of his friends, and in spite of the information that no less than six physicians of that place had fallen a sacrifice to the fever. When he arrived, in October, the rage of the epidemic had by no means subsided, and he found an ample field for observation. He was, indeed, so deeply engaged in his studies, that he forgot to write to his friends of New Orleans, who were so certain of his death, that in the Medical Society of that town a funeral oration was read to his memory. They were soon, however, agreeably surprised at the news, that M. Chervin, after staying two months at Savannah, had pursued his journey towards the north. He visited North Carolina, Virginia, Alexandria, Georgetown, Washington, Baltimore, Philadelphia, and New York. In the beginning of 1822, he left Boston for Guadaloupe, and having visited Paramaribo, Cayenne, Demerara, Barbadoes, &c. he sailed from Martinique to Spain, where he arrived in February, 1823.

Notwithstanding the dangers which he must have anticipated from the political events in that country, he travelled over the whole peninsula, and came to Cadiz shortly before the commencement of the siege by the French army. After a long stay in that town, he made excursions into the provinces of Malaga and Barcelona, and, at last, in 1824, returned to Paris. Here dangers of a new sort awaited him; he was calumniated, and the most ridiculous political charges were brought against him, but he gloriously triumphed over the envy of his enemies, and exposed their base intentions.

The number of documents which M. Chervin has collected exceeds 800; they consist, mostly, in authentic statements of the physicians and the magistrates of those towns and districts which he visited; and which are subject to the epidemic. Besides the valuable results of the most ample experience as to its treatment, they contain most important materials for deciding the question of its contagious or non-contagious nature.

Chervin has made more than *five hundred* post-mortem examinations. He has often *swallowed* some of the *black fluid* found in the stomach of the deceased; he *rubbed the whole surface of his body with it, and always remained free from infection.*

The following are the general results of his

inquiries:—Of more than 500 competent practitioners, only 48 are in favour of the contagiousness of the yellow fever, 483 being decidedly against it. In those parts of America where it most frequently rages, nobody believes in contagion; the extension of the disease seems entirely owing to the atmospheric constitution, and to local causes; the latter consist, partly, in putrid effluvia; there exists, in no case, a clear proof of contagion having taken place, and all assertions to the contrary are founded either on false testimonies, on defective observations, or on erroneous inferences from correct observations.

When the yellow fever raged epidemically in Catalonia, five French physicians, Mazet, Pariset, François, Audouard, and Bally, were ordered to go to Barcelona, and a Cordon Sanitaire was established by the French army along the Spanish frontiers; the disease was declared highly contagious, and the work of Bally, François, and Pariset,\* tended to confirm this opinion. Chervin being convinced that the measures against the extension of the fever were entirely useless, presented a petition to the Chamber of Deputies in 1825, and afterwards asked for a special committee to examine his documents, and then to decide upon the necessity of the Cordon Sanitaire. Eighteen of the most eminent physicians were elected for this inquiry, and it was not until the 15th of May, 1827, Coutanceau read to the Académie de Médecine the general conclusion of the committee:—"That the documents of M. Chervin contain decisive proofs against the contagiousness of the yellow fever, and, consequently, against the necessity of the Cordon Sanitaire." This report was instantly ordered to be printed, notwithstanding Pariset's opposition; after two days, however, this order was retracted, to give the French physicians, who had been sent to Barcelona, sufficient time for their defence. At the end of 1827, Coutanceau's report, with Pariset's reply to it, appeared; and Chervin himself published a small treatise on the transactions of the committee. He is, at present, engaged with the edition of a large work in four quarto volumes, of 600 pages each, with maps and plans. Till the publication of this important work, we suspend our further remarks on the subject, and refer those of our readers, who wish for a detailed account of the discussions in the Académie de Médecine, to the numbers of the *Revue Medicale*, from May to October, 1827.

From the Transactions of the Medical and Chirurgical Society of London.

THE CATARRHUS ÆSTIVUS, OR SUMMER CATARRH. By J. Bostock, M.D. F.R.S., &c.

In the tenth volume of the Society's Transactions there is an account of a disease which

\* *Histoire medicale de la Fievre jaune observée au Espagne, particulièrement en Catalogne dans l'année, 1821, Paris, 1823.*

I conceive to be of a specific nature, and which from its symptoms, and from its occurring only at a certain period of the year, I propose to name the catarrhus æstivus. In my former communication, I detailed the symptoms as they occurred in my own person; my present object is to extend my remarks to the affection as it occurs in other individuals, to inquire into its cause, and to make some observations on the mode of treatment.

The number of cases which I have either seen, or of which I have received a distinct account, amounts to eighteen, besides about ten others, which are less correctly ascertained. They all agree in the complaint making its appearance at the same season of the year, in its seat being the membrane lining the nose, the fauces, and the vesicles of the lungs, and, for the most part, in the paroxysms being excited and the symptoms aggravated by the same causes.

One of the most remarkable circumstances respecting this complaint is its not having been noticed as a specific affection, until within the last ten or twelve years. Except a single observation of Heberden's,\* I have not met with any thing that can be supposed to refer to it in any author, ancient or modern. I have at various times stated the particulars of my case to some of the most eminent physicians in London, Edinburgh, and Liverpool, and have very gratefully to acknowledge their kindest sympathy and attention; but, until very lately, it was always considered by them as an anomalous train of symptoms, and no one appeared to have witnessed any occurrence of a similar kind, and the same sentiment I recollect to have prevailed in this Society nine years ago, on the reading of my former paper. The first intimation which I received of a contrary opinion, was from the late Dr. Baillie, who, in the summer of 1822, related to me three cases which he considered as similar to my own. Yet, as there appears to be nothing, either in the cause or nature of the complaint, which can induce us to suppose that it is actually a new disease, we are obliged to conclude that it had been regarded as a mere modification of the common catarrh.

The twenty-eight cases referred to above, all agree in the complaint commencing about the end of May or the beginning of June, and continuing from four to eight weeks. Most of them are attended with fulness of the head, stoppage of the nose, sneezing, watering of the eyes, and discharge from the nostrils. In about half of the whole number the respiration is considerably affected, and in three or four instances it is almost the only symptom. Some of the cases are attended with distinct

cough, most of them with irritation of the fauces, and some with a degree of sore throat. Actual inflammation of the eyes is not a very common occurrence, and in some of the cases there is not even the discharge of tears, or the irritation of the eyes. The degree of general indisposition varies very much in the different cases; in some, the patient, during the whole period, is unable to use any exertion, or to continue his ordinary occupations, while, in other instances, he feels no inconvenience, except what arises from the fits of sneezing, and the copious discharge from the nose.

I have not been able to trace any decided connexion between the peculiar symptoms and any circumstance of age, sex, constitution, or mode of life in the patient. For the most part, indeed, I have found, that in very young persons, the first symptoms that are observed are sneezing and running of the eyes, that the chest is not affected until a later period of life, and that, as age advances, the purely catarrhal symptoms decrease, while the pectoral symptoms have a tendency to increase. With respect to age, I have no account of the complaint commencing earlier than it did in myself, at about eight years, nor have I heard of any very old persons being affected with it; for the most part, however, it seems rather to increase with the advance of life than the contrary, and I have no account of any one who has been once affected by it, ever afterwards losing the tendency. It is remarkable, that all the cases are in the middle or upper classes of society, some indeed of high rank. I have made inquiry at the various dispensaries in London and elsewhere, and I have not heard of a single unequivocal case occurring among the poor. A considerable majority of the cases are males, but I have an account of some females, who suffer severely from the complaint. There is no decided evidence of the complaint being hereditary, except that there is an instance where three members of the same family are affected by it.

I have not been able to ascertain with any great degree of precision, whether any specific temperament is peculiarly subject to it. Those cases that have fallen under my own inspection have been generally of a spare habit and liable to stomach affections, but I have met with exceptions to this rule. It does not appear to be confined to any particular situation; it occurs alike in towns and in the country, and I have not heard of any districts, the inhabitants of which are peculiarly subject to it.

The immediate cause of the symptoms seems to be sufficiently obvious; it consists in an increased action of the vessels of the membrane which lines the eyelids, the nose, the fauces, and the pulmonary vesicles, by which it becomes acutely sensible to external impressions, has its natural secretions augmented, and probably its bulk increased; to this last cause I think we may ascribe the very distressing sense of dyspnœa which exists in some of the cases. Although this membrane is continued without interruption over the different organs that are the seat of the affec-

\* "I have known it (catarrh) return in four or five persons annually in the months of April, May, June, or July, and last a month, with great violence." This passage was pointed out to me by Dr. M. Hall: I am also indebted to Dr. Hall for the account of a well-marked case of the catarrhus æstivus.

tion, yet it is observed that the different parts are affected in different degrees. Hence we may divide the disease into four varieties, according as the eyes, the nose, the fauces, or the lungs is the part more immediately affected. It is in the last variety only, that I have observed the constitutional symptoms of fever and the subsequent debility to exist in any considerable degree; and in this case I think we may account for the effect, by supposing that the thickened state of the membrane which lines the vesicles, prevents the oxygen of the inspired air from duly acting on the blood.

With respect to what is termed the exciting cause of the disease, since the attention of the public has been turned to the subject, an idea has very generally prevailed, that it is produced by the effluvium from new hay, and it has hence obtained the popular name of the hay-fever. As it is extremely important to ascertain the truth of this opinion, I have made it the subject of distinct observation, as far as regards my own person, and by minutely attending to the accession of the symptoms, for a number of successive seasons, in relation to this supposed cause, I think myself fully warranted in asserting, that in my own case the effluvium from hay has no connexion with the disease. The following observations will, I think, be sufficient to prove this position.

In consequence of the benefit which I always experienced from fresh cool air, I made choice of Ramsgate as my residence during the summers of 1824, 1825, and 1826. The last two of these years will be long remembered for their excessive heat; but by procuring a house on the cliff, exposed to the German ocean, and commanding complete ventilation, by avoiding bodily exercise, and indeed seldom leaving the house until evening, during the year 1825 I nearly escaped the disease. In the year 1826, I have reason to believe that the disease was much mitigated by the comparative coolness of the situation, but still I had many decided and some severe paroxysms. Now it is well known, that there is not an acre of meadow ground in the whole of the Isle of Thanet, and in the year 1826, in consequence of the great drought, all the little patches of grass, which may be supposed to exist on road sides or elsewhere, were completely burnt up. Nor is this all; during many of the hottest days, the wind blew steadily from the south-east, so that the nearest land to windward of the house which I occupied, was on the French coast, a little to the north of Calais. Yet during this time, whenever I relaxed from my plan of discipline, and exposed myself to the sun's rays, or by any means quickened the circulation, the symptoms recurred in full force.

The last year, 1827, with the exception of a short period in July, was cold. I could not conveniently remove to any great distance from London, and I spent the summer at Kew. This situation might have been chosen for the purpose of the experiment, for almost the whole of that part of the country consists

of hay-grass, which was cut while I was in the neighbourhood. In consequence of the coolness of the season I did not confine myself to the house, but walked out daily, occasionally in the Kew gardens, and was surrounded by many hundred acres of hay-grass in all its different states, yet except during the few hot days, when I suffered as usual, my complaint was in a much less degree than the average.

But although I think the evidence, as far as respects myself, to be quite decisive, I acknowledge that I have received accounts from various quarters, of individuals, who have felt no doubt that the complaint was brought on by the effluvium from hay, and was relieved or prevented by avoiding this effluvium. I will not venture to assert that this opinion is incorrect, but I believe that in most cases we may explain the facts more naturally by supposing, that the patients, at the time when they conceived themselves to be inhaling the effluvium from hay, were also exposed to heated air or sunshine, or had been using bodily exercise. Experience, however, must decide the question, and when the subject is once fairly brought into view, it will not be difficult to collect a sufficient number of facts to enable us to form our opinion.

With respect to the cure or mitigation of the complaint, I regret to say, that except in so far as we are able to avoid those circumstances which bring on the paroxysm, I have been able to obtain very little satisfactory evidence. Most of the patients have tried a change of residence, some from town to country, others from country to town, and some have removed to various parts of the island, or even to the continent. In two cases of considerable severity, the patients have felt convinced that they were better in London than in the country; in another case the patient conceived that he derived great advantage from exposure to sea air, but, in other instances, similar trials have not proved successful.

As far as regards medical treatment, an anxious desire to obtain relief from an annual indisposition of several weeks' continuance, and sometimes of considerable severity, has induced me to try, with the greatest perseverance, every remedy which held out the least prospect of advantage. I think myself warranted in asserting, that, on the whole, the depleting system is injurious, and that some benefit is gained by a moderate use of tonics. This is the only point in which the various accounts that I have received from others and my own experience appear to agree, and in general it would seem that the symptoms proceed nearly in the same way under very opposite plans of treatment, and are very little influenced by medicines of any description.

The experience of many years has taught me not to expect a cure for the complaint, so that I now only aim at relieving any peculiar urgent or distressing symptom. Bathing the eyes in tepid water, and fomenting the face generally, occasionally applying small blisters

to the chest, mild purgatives, small doses of ipecacuanha, Dover's powder, squills, and digitalis, bathing the feet in warm water, a moderate but not spare diet, perfect rest, and carefully avoiding all extremes of heat, comprise the whole of the means that I have found useful to myself. In order to prevent others from making useless experiments, I may remark, that among those things which I have tried without success are bark, iron, opium, mercury, large blisters, topical bleeding, the waters of Harrowgate and Leamington, the baths of Bath and Buxton, sea-bathing, the shower bath, abstinence from wine and animal food, and a more free use of them; each of these having been made, as it may be said, the subject of distinct experiment, and persevered in, until some circumstance rendered it necessary to discontinue them, or until they produced a decidedly injurious effect.

While this paper was in the press, I was informed by a friend, on whose accuracy I could place implicit confidence, that great relief had been experienced in two cases of the complaint, by applying to the eyes and nostrils a very weak infusion of tincture of opium, in the proportion of one or two drops of the tincture to an ounce of water. I regret to say, that in the trial which I have hitherto made, it does not appear to produce the same beneficial effect on my symptoms.

From the London Medical Gazette.

## OBSERVATIONS ON CATARACT.

By M. DUPUYTREN.

M. Dupuytren has recently made some comparative trials of the two methods of operating for cataract; namely, by depression and extraction. Of these we shall take an opportunity in a future number of giving some account; at present we purpose laying before our readers some general observations on the subject, taken from the *Clinique des Hôpitaux*.

Before undertaking to operate for cataract, M. Dupuytren enjoins the minutest inquiry into the general state of the patient, with particular reference to any concomitant diseases. The conditions which he regards as frequently contra-indicating the operation, or at least pointing out the necessity of delay, are, rheumatism, pulmonary catarrh, and derangement of the stomach or bowels; constipation, hemorrhoids, shingles, and many other diseases, may, he thinks, give rise to mischief in the eye, already irritated by the operation. If, for example, rheumatism be present, the operation may occasion its metastasis to the head; the eye and its appendages then become painful, and ophthalmia is excited, which often proves extremely severe. Whether this phenomenon is to be attributed to the rheumatism or to irritation, is of little importance; the fact remains the same, that it is not prudent to operate in such cases, experience having shown the evils which result from so doing. It is necessary, then, in the first place to at-

tack the rheumatism, and if it is determined to operate, whilst some degree of pain still continues, it is prudent to apply a blister to some part at a distance from the head. If pulmonary catarrh be present, besides the injurious effect of the cough on the circulation of the head, we should fear, if the operation of depression had been performed, lest the cataract should resume its former place in consequence of the succussions communicated to the head during the paroxysms of coughing. If there be any affection of the stomach, not only have we to dread the same mechanical inconveniences which result from the cough, and which in this case may be produced by vomiting; but, also all those complications, which must necessarily result from the sympathy between the stomach and the eyes, since there are some affections of these which depend entirely upon derangement of the digestive organs; and, moreover, if the operation has been performed during the existence of disease of the stomach, even although but slight, it is requisite always to place the patient during a longer period on regulated diet, and the difficulty of accomplishing this, either with children or persons advanced in life, is well known: indeed, with respect to these last, low diet is not always free from danger. In some persons it produces a nauseous odour, perceptible to the smell when the curtains are opened: it also causes loss of appetite, the tongue becoming at the same time large, pale, and loaded.

The presence of diarrhoea obliges the patient to get up frequently, and thence arise displacements of the cataract. Constipation may have many of the disadvantages which attend cough, and may occasion sympathetic effects besides. The presence of bleeding hemorrhoids contra-indicates the operation; and although it may be practised when the flux ceases, still we must, under such circumstances, always be on our guard against congestion about the head, and combat the slightest symptoms of this by the application of leeches to the anus. When the patient has any herpetic eruption, the operation may determine the eye as the seat of irritation, thus giving rise to serious disease of the organ.

After having combated the diseases with which cataract may be complicated, (all of which M. Dupuytren states that he is far from having enumerated,) there remains for us to choose between the two methods of operating; for nothing can be less rational than to adopt either universally, and without reference to the circumstances of the individual case. In surgery, as in medicine, the same methods of treatment cannot always be adopted in order to accomplish the same end: thus in cataract, the age of the subject, the form and size of the eye and its appendages, and various other circumstances, may compel the surgeon to have recourse to one form of operation in preference to the other. With regard to age, if we consider the state of the absorbent function, it will be apparent that we should prefer depression in children,

and extraction in elderly people. In the former, the vital functions are in all their energy—composition and decomposition are performed with astonishing rapidity—the absorption of the chystallin commences almost the moment that it is detached; besides which, it is never so hard at this period of life as in old age, and thus is less disposed to resist the powers of absorption. In old people again, the acts of composition and decomposition are sluggish; absorption, in particular, appears to have lost its energy, and the chystallin is of remarkable hardness, and, of course, more slowly acted upon by the absorbents. M. Dupuytren states that he has known the lens perfectly untouched, although displaced for more than two years in elderly persons, who had died of complaints unconnected with the cataract.

There are yet other considerations besides those above mentioned which are in favour of depression in children. They are rarely so docile as to refrain from all movement or struggling during the operation, a circumstance which renders extraction difficult, and which may cause the escape of the vitreous humour. In old persons the eye is deeply imbedded in the orbit, in consequence of the absorption of the adipose substance from the bottom of the cavity: under these circumstances extraction is extremely difficult. Besides, we meet with individuals of all ages in whom, from some preternatural movement or conformation of the ball of the eye, this last method is rendered inexpedient; and without speaking of those who have the eye constantly in a state of agitation, from rapid and convulsive movement, it is a general observation, that as often as an individual is deprived of sight for some time, he seems, with the habit of seeing, also to have lost the faculty of fixing the eye, the motions of the globe not being under the control of volition—a circumstance which much increases the difficulty of extraction.

After these general and comparative remarks on the choice of the two methods, M. Dupuytren described the manner of operating in both. According to him, two instruments suffice; for extraction the knife of Richter—for depression the needle of Scarpa, modified. Richter's knife appears to him preferable to that of Lafaye, because it acts principally by *sawing*, while the other acts rather by *pressure*. The methods themselves are too well known to require description, and we shall only draw the attention of our readers to one point in the operation of depression which M. Dupuytren has illustrated. Scarpa was originally of opinion that all cataracts ought to be broken down. It will easily be seen how much the illustrious Italian was in error, if we consider that, in order to offer a sufficient resistance to the needle, the chystallin would require to be of much more considerable size. On the other hand, the cataract is fixed by bands of the utmost fragility. The parts against which the needle is carried offer much less resistance than the vitreous humour; and

if, along with the softness of this last, we take into consideration the difficulty with which some cataracts are broken between the fingers even after their extraction, we cannot but be surprised at the opinion of Scarpa. There are, however, some cataracts which ought to be broken, and which, indeed, it is impossible to depress. Such are those, the cohesion of which presents no resistance to the instrument. After the operation, we ought to be on our guard against determination of blood to the head. In young subjects, the most active antiphlogistics ought to be employed; but in older persons, and where the temperament is not sanguine, these measures ought to be used with moderation. A simple white bandage, with a green or black one over it, suffices to cover the eyes. It is useless, and even hurtful, to apply charpie, which tends to increase the danger of ophthalmia; and from the pressure necessary to keep it in its place, may even occasion the evacuation of the vitreous humour where extraction has been practised.

From the Journal General de Medecine, &c.

#### SUR L'EMPLOI DE L'IODE DANS LE TRAITMENT DE LA GOUTTE. Par A. N. GENDRIN, *réd.*

The virtues of a newly discovered medicine in the treatment of a serious disease, can be accurately determined only by multiplied observations, repeated under varied circumstances, by different physicians. It was in the conviction of this truth, and in order that I might not expose myself to the imputation of having deduced an unwarranted conclusion from a too limited number of facts, that I invited the attention of physicians to the advantages which I had obtained from the employment of iodine in the treatment of gout. These advantages have been confirmed in my own practice. I have varied in divers ways the administration of the remedy, both externally and internally; it has been used in frictions, baths, vapour, tincture, alkaline solution, and enemata, and I have never observed any unpleasant effect produced by it. In all cases where the patients were not cured in a few days, their condition has been rapidly changed for the better. If these cases, which now amount to twenty-six, afford sufficient encouragement for further trials, they are not sufficiently numerous to justify us in attributing definitely to iodine the power of curing gout in all cases. It is for this reason that I have not yet given a detailed account of them, because I am desirous rather to have them corroborated by the observations of others, than to present them as alone sufficient to resolve the question. I shall first, therefore, publish in this Journal, the facts, favourable or otherwise, which may be communicated to me by my *confrères*, and add to them such reflections as they may appear to me to require. In this manner I hope to prevent any undue prepossession that I might otherwise conceive for a medecine, which up

to the present moment I have so successfully employed. I renew, therefore, the invitation which I addressed to the members of the profession generally, requesting them to verify the results which I have obtained, and to transmit to me their observations, that I may lay them without delay before the public through the medium of the *Journal Général*. I begin to acquit myself of my promise, by publishing the two following letters.

Letter from Dr. Louis Valentin to the Editor of the *Journal Général*, &c.

Nancy, June 15, 1828.

MY DEAR SIR,—It is with much pleasure that I reply to your invitation, published in the *Journal Général* for April last, relative to the efficacy of iodine in arthritic affections. The appeal which you make to the profession, will doubtless give rise to many communications. In the meantime I send you the following, which dates from a remote period.

In the interval between the years 1784 and 1790, a great number of persons affected with goitre came under my care in the city of Nancy and its vicinity; the soldiers belonging to the infantry composing the garrison, who mounted guard during the night, were often affected with this tumour. Having made many experiments on this occasion, particularly in the composition of the remedies which I employed, as well upon the soldiers as upon the citizens and inhabitants of the country, I observed that those who had nodes, contractions of the fingers, and enlargements of the joints, the sequelæ of gout, were cured, or at least more or less relieved of these affections. Of all the remedies which I employed, sponge, calcined or only burnt, formed a component part. I gave it also to arthritic patients unaffected with goitre, and with good effect. But another kind of *anti-strumous* powder which I prepared with the purified soda of the sponge, acted still more promptly in cases where both diseases were combined, restoring the mobility and suppleness of the articulations; I made use of this composition, and often with success, in patients who were affected with gout alone.

Passing through Geneva in returning from my first journey to Italy in 1820, Dr. Coindet informed me of his plan of treating goitre, by means of iodine; and gave me some of this substance and of its tincture. The odour of the iodine when rubbed, appeared to me to resemble that of sponge, a circumstance which I remarked at the time, and told the doctor that on my return to Nancy I would have the sponge analysed, strongly presuming that it would be found to contain iodine, which would afford an explanation of its success in the treatment of goitre. The analysis was made by two eminent chemists, and iodine was discovered, but a greater quantity was obtained by one than by the other.

I have never employed burnt sponge or iodine externally, in uncombined arthritic affections, but since my first observations I have often prescribed during the chronic stage of

gout, or with a view to prevent its return, the carbonate of soda in conjunction with a bitter powder, or with an opiate; most commonly I have directed it to be dissolved in an infusion of the roots of *calamus aromaticus*, or of *gentiana lutea*. For a long time I have not given the sponge to arthritic patients, unless they were at the same time affected with goitre.

If any one should be inclined to doubt the date of my little discovery, he may consult my manuscript upon goitre, sent to the Royal Academy of Surgery in 1789, for which a gold medal was decreed to me by that body. If sponge contain iodine, then I discovered forty-three years ago, that this substance, which was not known till 1813, is useful in arthritic affections. I am, &c.

The salutary effects resulting from the employment of calcined sponge in the treatment of gout, with which the patients of Dr. Valentin were accidentally affected, evidently confirm, as this able practitioner supposes, the results which I have obtained from the use of iodine in gout, for it is certain that calcined sponge does contain iodine, but it also contains a considerable proportion of alkali, and this latter substance is even in greater quantity than the iodine, so that it also, may lay claim to a participation in the success. It has indeed long been known that alkaline beverages have affected a cure in some cases of chronic gout, especially when there existed swellings of the joints; this consideration detracts much from the importance of the facts communicated by M. Valentin, which moreover were evidently not of a nature to lead to the employment of iodine, which I was solely induced to use from the fact of its generally recognised utility in chronic engorgements of the glands and articulations.

Letter from Dr. Godier to the Editor of the *Journal Général*, &c.

Paris, June 22, 1828.

SIR,—I have the honour to communicate to you the commencement of a case confirmatory of the anti-arthritic virtue of iodine; the success has been too prompt to permit me longer to withhold the details of the case, although the cure is still incomplete.

Delaunoy, a woman about 60 years of age, of a sanguineous temperament, and living in an unhealthy street, was obliged, from her occupation, to have her hands frequently immersed in water; she had a paroxysm of gout, which, although the first which she had had, attacked successively all the articulations. The toes of both feet were red and tumefied; the aponeurosis plantaris at its attachment to the os calcis was swelled, but without redness, and was extremely painful, as were all the affected parts; the joints of the fingers were likewise red, tumefied, and painful. I directed absolute repose, a sudorific ptisan, and the application of flannel to the suffering parts; no change resulting after the lapse of several days, I had decided to have recourse to leeches,

when I read in the *Journal Général, &c.* your observations on the anti-arthritic properties of iodine. I determined to make trial of the remedy, and the same evening prescribed frictions upon the affected parts, with an ointment composed of one part of iodine and twelve parts of axunge. Four days afterwards, the tumefaction began to diminish, together with the pain; the frictions were employed night and morning, and to-day, 22d June, she informs me that she is almost entirely free from pain, and that she can use her needle, which before the employment of the ointment she could not do; she is still not entirely cured, but such a surprising effect, obtained in the space of fifteen days, by the external use alone of iodine, has induced me to request a place in your *Journal* for the commencement of a case, the conclusion of which shall be forwarded to you as soon as the cure is complete.

I am, &c.

GODIER, D.M.P.

*Médecin du Bureau de Charité du Premier Arrondissement.*

P. S.—June 29. Since my last communication, the patient has been alternately better and worse; she is worse in stormy weather particularly. For the last three days I have given iodine internally, and she is improving daily. I shall avail myself of another opportunity to communicate to you the sequel of this case, together with several others, all favourable to the use of the remedy.

The success obtained by M. Godier is the more remarkable, since the quantity of iodine applied externally was so small; it is not to be wondered at that the symptoms should re-appear, when the immediate effect of the medicine has been somewhat diminished by habit; and to complete the cure but two methods remained, either to administer the remedy internally, as was very properly done by M. Godier, or to increase the proportion of iodine in the ointment. In the latter case there is one remark which my experience leads me to make.

When the proportion of iodine in the ointment exceeds one-eighth part, it is too irritating for the skin of most patients, especially when the frictions are always made upon the same place; in such cases it will be advisable to apply them alternately to the inner part of each thigh and arm, alternately, or the ointment may be made less irritating; this may be accomplished by using as an excipient for the iodine, *baume tranquille*, mixed with an equal part of axunge. It is under this form that I now apply the remedy to joints affected with acute gout.

From the London Medical and Surgical Journal.

#### FUNGUS HEMATODES—LIGATURE OF THE CAROTID ARTERY. By M. LISFRANC.

Josephine Lenoir, æt. 18, of a lymphatico-sanguine temperament, enjoyed perfect health

until she arrived at the age of twelve. At this period there appeared in the situation of the right parotid gland a tumour, accompanied with slight cephalalgia, throbbing in the right lateral region of the head, and palpitations of the heart. This tumour was supposed, by the practitioners who were consulted, to be engorgement of the lymphatic vessels of the neck. From the first appearance of the malady, until 1827, hemorrhage took place at various times from the meatus auditorius of the side affected, which appeared to ease the pain from time to time, without arresting the development of the tumour, the pulsation in which becoming more manifest, inspired the friends of the patient with serious inquietude. Resorting to Paris to obtain the succours of the surgical art, she entered the hospital of la Pitié on the 11th of January, 1827, in the following state:—From the angle of the lower jaw to the lobule of the ear, the tumour is soft, even, round, without change of colour at the base, of the size of a hen's egg, diminishing on pressure, exhibiting pulsations synchronous with those of the heart, which pulsations, without quite ceasing, are less perceptible during the compression of the primitive carotid artery; pulsations otherwise stronger, more frequent, and extensive, than in the natural state. When the tumour was compressed, no effect appeared to be produced on the brain. All these circumstances gave rise to an opinion that this was an aneurism of the external carotid artery. (*V. S.* 3viii., and low diet from 12th January to 10th April.) Ligature on the common carotid, which was intended to be had recourse to, was deferred, owing to the lowness of the temperature of the atmosphere and other circumstances, which left time to observe the disease with more attention. Several distinguished practitioners were consulted on the question, whether or not the heart was healthy? It was their unanimous opinion that this organ was in the natural state. The beating of the common carotid, which appeared to extend to about two inches and a half in all directions below the tumour, rendered it rather suspicious that this vessel was also affected: in fine, an abundant discharge of blood from the meatus auditorius of the same side gave rise to some inquietude. On examining with attention the interior of the ear, a small, soft, reddish tubercle was discovered, formed of an erectile tissue. This gave rise to a suspicion that the tumour, regarded before as an aneurism, was a fungus hematodes. As it appeared evident, from the circumstances attending the case, that, if abandoned to the efforts of nature, it would lead to certain death, M. Lisfranc thought that some chance would be afforded to the patient by applying a ligature on the carotid artery. In a case so serious and complicated he, however, not wishing to rely entirely upon his own views, presented the patient to the Académie Royale de Médecine (Section of Surgery,) the members of which were of opinion that the operation ought to be had recourse to. The greater

number of the distinguished surgeons who saw the case thought that the tumour was an aneurism, and not a fungus hematodes.

April 10th, in the presence of several members of the academy, the operation was performed in the following manner:—The patient, lying on her left side, contracted the sternomastoid muscle, in order to render its anterior edge more distinct, along which M. Lisfranc made an incision of three inches long, in the middle of the neck. The skin, the sub-cutaneous cellular tissue, and the fascia cervicalis were incised with precaution; a very considerable venous plexus, observed under this tissue, was moved aside with great care, and held by the finger of an assistant towards the upper angle of the wound. A sponge squeezed out of cold water was now applied for a few minutes to the wound, by which means the bleeding of the small vessels was stopped, and the surface of the wound looked as clean as if it had been on the dead body. The sheath of the artery was divided with great care according to the method of Scarpa. The cellular tissue surrounding the vessel was moved aside with the fingers, and the sternohyoidien muscle was pressed inward and downward out of the way. The jugular vein, known by its colour, its softness, and the augmentation or diminution of its size, as the patient exerted herself or not, was more inward. A large vein was discovered passing across the upper part of the wound; the cellular tissue surrounding it was removed with great precaution, and two ligatures were applied; the vein was then divided in the interval. The internal jugular vein, moved outward, permitted a ligature to be carried under the artery, from without inward, with great facility. The operator, having satisfied himself that the artery, and nothing but the artery, was included in this ligature, tied the vessel very tightly. The pulsations, contrary to the opinion of some authors, did not completely cease in the tumour, the size of which had already diminished one half. The pneumo-gastric and the great sympathetic nerves were observable at the bottom of the wound. The extremities of the ligature were allowed to hang out at the lower angle of the wound. The edges of the wound were now brought together by two strips of adhesive plaster, and a compress and bandage applied.

The pulse, examined by M. Moreau during the whole of the operation, did not undergo any change. The patient, in every respect, showed uncommon courage. From ten o'clock until one in the afternoon, she felt remarkably well; the throbbing in the head less painful; countenance smiling; pulse as natural as before the operation; slight pain only in the wound. (*Gum water; absolute abstinence.*) At one o'clock, pulse frequent, quick, full, hard; face flushed; injection of the conjunctiva; eyes full of tears; cephalalgia. (*V. S. 3vij. from the arm; application of ice to the tumour.*) At three o'clock, the patient felt a little pain in the throat. (*The ice to be discontinued.*) At six o'clock, some slight shi-

verings; perspiration; feeling of syncope; severe pains in the epigastric region and towards the sternum; nausea; cephalalgia; pulse full and accelerated; countenance pale. At eight o'clock, the countenance looked as before the operation; the epigastric and substernal pain less severe. (*The same drink.*) She passed the night very quietly. 11th. Difficulty of deglutition diminished; epigastric pain continues; pulse frequent; face flushed and swelled; injection of the superficial veins; cephalalgia relieved by three bleedings from the nose, which took place between nine o'clock in the morning and noon. At this moment, throbbing in the head; cephalalgia; dyspnœa; and a sense of suffocation. (*V. S. 3vij. from the foot, followed by great relief.*) The wound dressed morning and evening. Towards the evening the symptoms returned. (*Eight ounces of blood taken from the foot.*) Marked relief; two hours' sleep during the night. 12th. Pulsation weaker in the tumour, which appears a little diminished in volume; pain in the throat quite gone. (*Vegetable lemonade for drink.*) In the course of the day, the head became affected again with severe pain; pulse full and frequent; skin hot and moist; palpitations. (*V. S. 3vij. from the foot, followed by a remarkable amendment; dressing the same.*) 13th. Substernal pain less acute; the skin continues hot; pulse frequent and full; cephalalgia persists. (*V. S. 3vij. from the foot.*) Complete cessation of the pains. The aspect of the wound looks favourable; it is dressed twice a-day. 14th. Slight sleep during the night; the pain of the stomach and chest is now only intermittent. (*Emollient drink.*) In the day pulse frequent; face flushed; ardent heat of the skin; veins of the face much injected; sense of general weakness and of constriction in the chest. (*In the evening, V. S. 3iv. from the foot.*) This was followed by slight syncope, though with sensible amelioration. 15th. Pulse less frequent and less full; tumultuous throbbing in the precordial region; general illness; anxiety. (*The same drink; antispasmodic mixture.*) 16th. Pain the neck, attributed to the constant position of the patient on the back; considerable debility. The wound looks well, and the suppuration is of a good quality. 17th, at four o'clock in the morning, painful oppression; pain in the right side of the thorax; intense cephalalgia. (*V. S. 3vj. from the foot.*) This was followed by inexpressible relief. At eight o'clock, in the same state. (*Few spoonfuls of buillon; diluent drink; antispasmodic mixture.*) On dressing the wound, a point near the ligature looked very angry, which gave rise to a suspicion that a coagulum had not yet formed. In the evening, pain in the throat; prostration of strength; pulse less accelerated; contractions of the heart less violent. 18th. No difficulty of deglutition; the same state of the heart and pulse. At five o'clock in the evening, countenance thin; swelling of the veins of the forehead; slept for an hour. At this time the patient wished to drink; in lifting

her head to do so, she felt slight pricking in the wound, and dreadful hemorrhage immediately ensued. The dressing was immediately removed, and the finger of the *religieuse* pressed in the wound was not able to arrest the flow of blood. The house-surgeon immediately arrived and found the patient in a state of syncope; the hemorrhage had stopped; inspirations deep and rare; pulsations of the heart slow and feeble; pulse small and very compressible. (*Aspersions of the face with vinegar and water; friction on the precordial region and of the limbs.*) The pulse became insensible; the heart ceased to contract; at a quarter before eight all attention was now useless; the patient was no more. The tumour had diminished one-half.

*Secio Cadaveris, forty-five hours after death.*—Embonpoint ordinary; slight rigidity of the limbs; skin blanched. *Head:* The base of the cranium presented a very remarkable alteration. The petrous bone of the right side had tripled its natural size; its superior edge was on a level with the small wings of the sphenoid. Its tissue was soft, friable. On detaching the dura mater, fragments of this bone came away adhering to the membrane. The interior of this bone was spongy, and of a reddish colour, resembling the cavernous portion, slightly macerated. The cavernous sinus of the same side was flaccid at the internal extremity of the petrous portion. This portion extended to the posterior clinoid process, both of which appeared to form only one piece of bone. The disorganization of the bone in this part was such that a probe could be run in many points from the interior of the cranium to the cervical region through preternatural apertures. The thyroid gland was apparently healthy. The common motor oculi, the pathetic, the trifacial, and the external motor oculi nerves, did not present any sensible change of structure; but the acoustic and facial nerves were at least twice their natural size at the point before entering the petrous bone. The dura mater was much thickened in the right temporal fossa, and so adherent to the petrous portion as to render it impossible to separate the two without tearing off fragments of the bone. All the sinuses were enormously dilated, particularly those of the diseased side. The superior and inferior petrous and the cavernous sinuses of that side were filled with an erectile, spongy substance, divided into an infinite number of small cells, all of which communicating with each other; by insufflation, they could be distended into a polygonal form: they contained hardly any blood. The arachnoid and pia mater were thickened and adherent to each other over the lower surface of the middle lobe of the brain on the diseased side. *The brain did not entirely fill the cranium.* An interval of a quarter of an inch separated the cranial, from the cerebral, arachnoid. The brain was otherwise in the natural state, its colour only appearing rather paler. *Neck:* The fungous tumour was situated in the hollow between the posterior edge of the lower jaw, and the

mastoid process, extending to the lower surface of the petrous portion of the temporal bone and the auditory conduit, to which it adhered by strong prolongations. Between the ramus of the jaw and the tumour was observed the superficial temporal artery and vein; the facial nerve enveloped in a hard cellular tissue; several veins in a remarkably enlarged state; the stylo-maxillary ligament, and the cervico-facial branch of the respiratory nerve. The internal carotid artery traversed the substance of the fungus, in which it gave off three thick branches; to the jugular vein, which contained within its canal a considerable prolongation of the tumour; to a great number of accidentally developed veins; to the pneumogastric and to the superior and middle cervical ganglions. The parotid gland was found in a state of great atrophy, reduced to a very small size. The skin over the fungus was healthy. The tumour, since the hemorrhage, had diminished very considerably. Divested of its cellular covering, and of the surrounding vessels, its size was not larger than that of a hen's egg. Its form and appearance might have been compared to those of the heart of a three-months foetus. Its structure was soft, spongy, of little consistence, diminishing under the pressure of the fingers, of a dirty red or brownish colour. It was formed partly of aneurismatic vessels and partly of erectile tissue; it had no proper capsule. Its cells communicated with each other, and appeared to consist entirely of arteries and veins in a dilated state, which formed the chief constituents of the fungus. The upper two-thirds of the wound resulting from the operation was cicatrized; the rest was filled by a fleshy substance. There was no abscess or purulent secretion any where surrounding the part. The ligature still embraced the artery; and, what was remarkable, the rupture had taken place fifteen lines below the ligature, and a coagulum filled the caliber, otherwise healthy, of the vessel, from the ligature to the opening where the hemorrhage had issued. *Thorax:* In the posterior mediastinum, some of the lymphatic glands were found softened, containing caseous matter, which had no connexion with the wound of the neck. All the other parts were healthy. *Abdomen:* All the abdominal viscera were in the natural state; a great quantity of gas had formed in the digestive canal and in the peritoneal cavity.

We have translated this very interesting case nearly at full length. It is very probable that, had hemorrhage not taken place, the patient must have soon died of the disease of the brain and of the bone. The case is altogether one of great interest.

From the Gazette de Sante.

OBSERVATION SUR UNE HYDROCEPHALE GUERIE PAR LA FORMATION SPONTANEE D'UN ABCES. Par M. AUGUSTE LARREY, D. M. à Toulouse.

June 20, 1827, I was requested to visit

the child of M. Pialh  s,   t. 14 months, who had been much indisposed for the preceding five or six days, up to which period she had enjoyed excellent health. Previously to sending for me, a variety of anthelmintics had been given, on the supposition that she was troubled with worms, but without bringing any away. The following were her symptoms when I saw her; pulse strong and quick, profound coma, occasional screams, frequent convulsions, respiration natural, abdomen soft, and not at all tender upon pressure. The parents assuring me that the child had not been exposed to the heat of the sun, nor had in any way injured her head, which, notwithstanding, appeared to be the seat of the disease, I was, at first, unable to determine its character; acting, however, upon existing symptoms, I directed the application of six leeches behind each ear, an exclusive milk diet, and a few drops of orange flower water, to mitigate the extreme thirst with which she was harassed.

The above symptoms continued unabated on the 21st, and others now made their appearance; every thing swallowed was immediately ejected from the stomach, the alvine and urinary evacuations were suppressed, constant stupor, convulsions more violent than before, pupils dilated, and the head drawn backwards. A blister was directed to the back of the neck, ice to the head, and sinapisms to the legs; in the evening a blister was applied to the right arm, as a substitute for that on the neck, which it had been impossible to retain in its place.

No change in the melancholy condition of the patient, was perceptible on the 22d; her pulse was softer, and the abdomen appeared tense; emollient fomentations were ordered, the blister kept open, and the other remedies continued.

On the 23d, there was an aggravation of all the symptoms; the convulsions were more frequent and of longer duration, the vomiting continued, and the child, when allowed a moment's repose, appeared as if lifeless. The nurse now confessed that about the beginning of the month, the child had struck its head by a fall from her arms upon the ground. A blister was applied upon the head, and frictions with camphorated oil upon the abdomen; the patient readily took the breast, but immediately rejected what it had swallowed.

Nothing occurred worthy of notice on the following days; the blister on the head soon dried up, and one on the left arm was substituted. On the 28th, the mercurial treatment was commenced; calomel was given internally morning and evening, and frictions with *Ponguent napolitain* were made regularly once a day, both along the spine and behind the mastoid processes, at first in quantity of a quarter of a drachm, afterwards of a drachm. By the 9th of August, twelve days afterwards, she had consumed five drachms of the ointment and 18 grains of calomel, without any marked effect being produced, except that the convulsions and vomiting were not so frequent as

before. The above treatment was suspended during four days; at this time the unfortunate patient had become so greatly emaciated, that I did not think it necessary to trouble her farther with medicine. The mercurial treatment was recommenced on the 17th, and in the space of seven days, six drachms of the ointment were consumed.

Perceiving the inutility of all our efforts, and that the only remaining resource was the hope that, from the protracted character of the disease, nature was about to establish a crisis, every thing was discontinued with the exception of emollient fomentations to the abdomen, and death was daily expected to terminate a life of so much suffering.

Finally, on the 31st July, 41 days from the invasion of the disease, and about two months after the fall, an oblong tumour, about the size of a pigeon egg, was observed on the inner and upper part of the left arm; it was painful to the touch, and attended with inflammation of the skin; the day following it had increased to twice its former size, and when opened, on the 2d of August, about eight ounces of a white and very thick pus were discharged. The wound was dressed in the usual manner, and by the 7th, was completely healed. From the day on which the abscess was opened, the patient appeared as if restored to life, she called urgently for food, and greedily devoured all that was set before her. This appetite continued without any unpleasant consequence for the space of ten or twelve days, when convalescence was finally established, and after the lapse of a month she was entirely cured.

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From the Glasgow Medical Journal.

**CASE OF LITHOTOMY, COMPLICATED WITH OBSTRUCTED BOWELS,** *from the pressure of an Osseous Tumour in the Mesentery.* By JOHN MACFARLANE, M.D. Lately one of the Surgeons of the Glasgow Royal Infirmary, &c. &c.

Robert Cameron,   t. 67, weaver, was admitted into the Royal Infirmary, on the 23d of January, 1827, on account of stone in the bladder. Complained of severe pain about the neck of the bladder, and at the glans penis, coming on frequently when at rest, and without evident cause, but always urgent during micturition, when at stool, or on the slightest motion of the body. The calls to void urine varied in frequency. For whole days he required to pass it every quarter of an hour, generally in a small stream, frequently obstructed, and accompanied by painful tenesmus. Had slight pain on pressure in the situation of the right kidney, and had several times voided small calcareous particles about the size of a pin head, and twelve months before, a hard smooth yellow stone, of the size and shape of a kidney bean. A large sound was readily introduced into the bladder; and from the irregular feeling and rattling noise

communicated, it was evident that there were several calculi. The urine was of a natural colour, but on standing deposited a small quantity of flaky sediment. The bowels were obstinately costive, and the abdomen somewhat tympanitic. These symptoms, commencing about 19 months before his admission, and gradually increasing for the last six weeks, had forced him to give up his employment.

On attentively examining this patient, to ascertain if any other disease existed likely to militate against the success of an operation, it was found, that with the exception of a slightly enlarged prostate, and flatulent distention of the bowels, no other morbid manifestation could be discovered; and these were not such as to forbid an operation, to which he was anxious to submit. Although advanced in years, he still exhibited a healthy and robust appearance, and possessed sufficient vigour to sustain the shock of an operation, and afterwards to establish an efficient process of renovation. The straining on going to stool, and while micturating, was excessively severe, and resembled much the propulsive pains of parturition; but it was believed that the obstinate constipation, and the irritation from the calculi and enlarged prostate, were sufficient to account for this unusually urgent symptom. The prostate gland had not the globular shape usually observed; it was, however, somewhat enlarged, firmer than natural, and so flattened, that the finger could not reach the bladder. When this part is in a state of simple chronic enlargement, we cannot reasonably refuse our patients the chance of an operation, should there exist no other unfavourable combination. I have twice operated with success in more extensive enlargements of the prostate; and although the wound did not heal so speedily as when no such disease existed, the result was still sufficiently fortunate to justify the operation, and in one of the cases, the gland diminished considerably afterwards. The existence of several calculi in the bladder would, by requiring the frequent introduction of the forceps for their removal, somewhat protract the operation, but this could afford no ground of apprehension, it being acknowledged that there is more danger to be dreaded from the extraction of one large, than of many small calculi.

The patient was subjected to the usual treatment for a few days. He was repeatedly purged with castor oil and enemata, and an immense quantity of scybala evacuated, but without any marked reduction of the flatulent distention of the belly; the bladder was soothed by the warm bath, anodyne clysters, and frequent doses of supercarbonas sodæ.

On the 29th, the lateral operation was performed, by running the narrow probe-pointed knife along the groove of the curved staff, and six entire, and three broken calculi extracted, the largest being oval, and about the size of a walnut. From the enlargement of the prostate, and depth of the peritonæum, some difficulty was experienced in feeling

with the finger the whole internal surface of the bladder, to ascertain that all the calculi were extracted. To remove all doubts in such cases, the introduction of a sound, either by the penis or the wound, should not be neglected, as it affords the most correct means of ascertaining that this necessary object has been effected. The prostate gland felt hard, almost like cartilage, but when the finger or forceps were withdrawn, it still retained so much elasticity, as to close the wound into the bladder as if by a valve. Three arteries were observed to bleed freely, two superficial branches were tied, and a large deeper-seated vessel, evidently the transversalis perinæi, threw out its blood per saltum, and in considerable quantity. This artery was much enlarged, but from its deep situation, and its being divided near to the ramus of the ischium, it was found impossible to secure it by ligature; it was, however, easily commanded by pressure. An elastic tube was introduced into the bladder; the patient was placed on his back in bed, the thighs separated, to facilitate the escape of blood, and he was lightly covered, and kept cool. On visiting him at 8 P.M. the urine was passing freely along the tube, and he was free of pain, except when occasionally attacked by a strong bearing-down sensation, accompanied with a desire to void urine and go to stool, which being similar to what he experienced before the operation, was ascribed to flatus. The finger was passed into the bladder, which was found empty. Pulse 72, soft. Complained of thirst, and slight rigours. *Anodyne enema.*

30th. Has had some sleep during the night, and feels easy, except when affected at intervals with severe spasmodic pains in the abdomen. In the evening, as he had pain on pressure above the pubes, and his pulse was accelerated, 24 leeches were applied to the hypogastrium, and a *large enema* ordered, which dislodged a quantity of hardened fæces.

31st. Continues to complain of violent expulsive efforts, and of fixed pain above the pubes. Pulse 74—tongue clean—no stool. The tube was withdrawn. *Castor oil. Leeches to the hypogastrium. Anodyne enema at bedtime.*

1st February. Six stools from the oil, but continues to complain of fixed pain in the hypogastrium, and of general uneasiness in the abdomen from flatulence. *Leeches and anodyne repeated.* A large elastic tube was introduced for several inches into the rectum, to facilitate the escape of air from the bowels, but only a small quantity was discharged. This practice is sometimes successfully adopted, when the natural peristaltic action of the intestines has been impaired by flatulent distention; but if the gas is confined in the small, or high up in the large intestines, beyond the reach of the tube, no benefit can be expected from its introduction.

2d. Passed a comfortable night; but an hour before the visit had a small rigour, followed by increase of pain, thirst, and nausea. Pulse 84, small and sharp. Tongue dry and

furred; *Calomel and opium. Fomentations. V. S. to 3vij.* Blood cupped and buffy.

3d. Pain on pressure, swelling, and tenesmus greatly abated. Voids his urine through the wound at intervals, by contraction of the bladder. Pulse 72, soft and compressible.

5th. Flatulence and tenesmus increased, coming on in frequent and violent paroxysms, but with little or no pain on pressure. Has had several stools, containing scybala, accompanied with excruciating pain during their evacuation. Complains of the feeling of a large hard body, fixed in the upper part of the rectum, which excites violent expulsive efforts. Pulse 70, rather weak. Tongue dry and furred. Wound sloughy. *Dose of castor oil, and anodyne enema after its operation.* These symptoms appeared to depend on abdominal irritation, the consequence of an impacted state of the colon; but did this condition of the bowels afford an adequate explanation of the violent bearing-down efforts, which had annoyed him more or less for a whole year?

From this report till the 10th, there was little change in the symptoms. The spasmodic pains affected him violently, and the wound was lined with an ash-gray tenacious secretion. The stools still contained scybala; his countenance was pale, and had an exhausted expression, on which account purgatives were more sparingly administered, and the bowels unloaded by frequent and copious injections, thrown freely up by the patent enema syringe.

13th. Paroxysms of pain continue unabated, but the stools are now of a natural colour and consistence. Pulse 68. *Anodyne enema. Beef tea. Arrow root.*

15th. Had a violent attack of pain this morning, chiefly referred to the rectum, and he describes it as exactly similar to what he experienced on going to stool previous to the operation. The finger was passed into the rectum, but neither hardened feces, nor any other obstruction, was discovered. As the bowels were now acting more freely, and the flatulence had diminished, and as he complained of burning heat about the prostate and bladder, it was judged proper to sooth the recto-vesical irritation by a pill every six hours, containing *Extract. hyosciami gr. iij. and camphor gr. ij.* *Warm bath. Anodyne enemata. An occasional opium suppository.*

For the following three days, the attacks of pain were less violent, and he appeared to improve in strength and spirits; he still, however, complained of severe straining at stool, from the feeling of a foreign body in the gut, which he was ineffectually excited to expel. On the 20th, his appearance was less languid; his pulse was about 80, and of moderate strength, the tongue clean and moist, and the wound florid and granulating. The thighs were secured together, to accelerate its closure, and the urine passed afterwards by the penis. His appetite was improving; and although at this period he was considerably exhausted, there existed no prominent indication of a suddenly fatal result. On the morning of

the 22d, he was seen by the nurse at six o'clock in his usual state, and when visited again at eight, he was found dead in bed.

*Dissection.*—On opening the abdomen, a hard tumour was discovered lying over the last lumbar vertebra, between the laminae of the mesentery, near the inferior part of the ilium, and which pressed on the sigmoid flexure of the colon, where it is about to become rectum. The surrounding mesentery exhibited no thickened or diseased appearance, and only adhered to the surface of the tumour by loose cellular attachments, easily destroyed by the finger. It was about the size of a small lemon, of a hard bony feeling and appearance, and very irregular shape. When sawn through, the exterior part was evidently bone, and varied in thickness at different parts from a quarter to half an inch, while the centre was filled by a yellowish white substance, in appearance and consistence like adipocire, intersected in various directions by spiculæ of bone. Two small cavities in the centre were lined with innumerable transparent, needle-like crystals, which, however, disappeared after the tumour was dried, and before I had an opportunity of submitting them to chemical analysis. The mucous coat of the bladder was considerably thickened, of a dark vascular plaited appearance, especially about the neck, and coated by a muco-purulent secretion. There was a tumour at the fundus about the size of a small marble, containing purulent matter, which issued into the cavity of the bladder, through two fistulous openings in the mucous coat at that part. The prostate gland was enlarged, and firmer in texture than natural, but without the fibrous appearance of scirrhus. The mucous coat of the rectum was highly inflamed, and there was considerable induration and thickening of parts between this gut and the base of the bladder.

This dissection afforded a satisfactory explanation of what had been previously only matter of speculation. The long-continued and painful tenesmus was obviously to be referred to the pressure of the osseous tumour, on the commencement of the rectum, producing an impediment to the regular discharge of the feces, tympanitic swelling of the abdomen, and great irritation. From the situation and connexions of this tumour, it would appear, that when the diaphragm and abdominal muscles were called into action in expelling the feces, it would be forced back on the termination of the colon, by the pressure of the surrounding parts, and not only impede the feculent evacuations, but also, from its extreme hardness and inequality, irritate and injure the bowel in no small degree.

In scrofulous habits, the mesenteric glands are sometimes filled with calcareous matter, but bony depositions are stated by Dr. Baillie (*Morbid Anatomy*, p. 134.) to be of rare occurrence. The few recorded cases of this disorganization, which I have had an opportunity of examining, appear to have originated in disease of the glands of the mesentery, and

to have been complicated with organic disease of the bowels. Dr. Donald Monro narrates a case in the *Medical Transactions* (vol. ii. p. 361,) in which all the mesenteric glands, varying in size from a pea to a walnut, were hardened and ossified. They were not, however, as in the case of Cameron, made up of one large firm osseous tumour, but, "like spongy carious bones, they were composed of a number of small pieces, joined together by membranes."

Cameron's death cannot be attributed to the operation; he lived for 23 days after its performance; and although the bladder was partially diseased, yet he was exhausted and carried off by an unusual and unexpected occurrence. It was a combination that could not have been detected during life, otherwise no operation would have been performed; and although it had been discovered, it was irremediable. The flatulent distention of the belly of course prevented its being recognised by an external examination, and it was too high up to be reached by the finger in the rectum.

I am indebted to Professor Thomson for the following analysis of the calculi and tumour.

The calculi consist chiefly of uric acid. But there is present in them also a small quantity of matter, which has a light yellow colour, and dissolves with ease both in nitric acid and in caustic potash. But it did not crystallize with either, nor form the pink coloured matter with nitric acid. It may be new; but the quantity upon which I experimented did not admit any farther trials. The bone is very solid externally, and is surrounded by a periosteum in the usual manner. It becomes more and more porous towards the centre. The specific gravity of the whole mass is 1.219. But it was so full of cavities, that this specific gravity is doubtless below the truth. The matter in the middle of the bone is soft, but compact. It cuts like cheese, and is partly buff coloured, partly white. It was not in the least soluble in boiling alcohol, and therefore was not adipocire. It was insoluble in acetic and muriatic acids, and therefore was neither muscular nor ligament. But when digested in caustic potash, a little fat was separated. It melted when heated, and behaved like cartilage.

From the *London Medical Gazette*.

#### CASE OF ENLARGED BLADDER. By J. B. ESTLIN, F.L.S.

*To the Editor of the London Medical Gazette.*

SIR,—As the following case of enlarged bladder may prove interesting to some of my professional brethren, I have much pleasure in giving it publicity through the medium of the *Medical Gazette*.

A gentleman, 54 years of age, consulted me in October 1827, in consequence of constant nausea and loss of appetite and strength. His tongue was foul and his bowels confined. The

pulse indicated no morbid symptom. I ordered him some cathartics with calomel, and when he visited me two days afterwards he was somewhat better. I then prescribed for him an emetic and a bitter aperient infusion.

October 8.—Not much better. He informed me that for many months he has had some difficulty in passing his water; that a considerable quantity comes away in the day and night, but in small portions at a time, and often involuntarily and without any force. He assured me (and I place full reliance on the declaration) that he had never laboured under gonorrhœa or any other form of venereal complaint.—Repeat the cathartics.

15th.—No better. Being anxious to ascertain the state of the urethra, I introduced a middle-sized bougie, which met with a degree of obstruction at six inches from the orifice that moderate pressure could not overcome; and as much pain was occasioned by the attempt, I desisted from it for the present.

18th.—I introduced a silver catheter, and found it pass into the bladder without any obstruction. A pint of urine was drawn off—a quantity much exceeding what he has passed at one time for many months.

19th.—He suffered much pain after the introduction of the catheter, and experienced not the least relief from the quantity of water removed from the bladder.

It was my intention to have passed the catheter again to-day, principally with the view of ascertaining if there were any calculus in the bladder impeding the passage of the urine into the urethra, but the canal remained in a very uneasy state from the employment of the instrument yesterday; and as he was under the necessity of going a journey on business in a day or two, I thought it better to delay the attempt.

30th.—He returned from his journey last night, in all respects worse. He has constant nausea, and he frequently passes urine involuntarily.—Cap. pulv. ipec. comp. gr. xii. h. s.

31st.—Slept. Vomiting came on this morning and continued through the day. Bowels confined. Calomel, with other aperients, was prescribed.

Nov. 1st.—Vomiting very frequent. Bowels do not act. Calomel and opium given.

2d.—Vomiting incessant: the quantity brought up from the stomach is far more abundant than the fluid he swallows: the rejected matter is of dark colour and coffee-ground appearance. He has some slight alvine evacuations of similar fluid. A few ounces of blood were drawn from the arm: it was buffy. No relief experienced from the bleeding.

3d.—He becomes worse: the vomiting is unabated, and the ejecta are darker. The urine flows involuntarily, from two to three pints apparently in the twenty-four hours.

From the commencement of the vomiting he has had no power of taking food. Various liquids have been tried: soda water remains longest on the stomach.

Yesterday or to-day he directed my atten-

tion to a swelling in the abdomen, which had escaped my notice when I felt the epigastric region, and when I daily pressed the bowels to ascertain if any tenderness existed. I examined the tumour, and found it to be of an oblong form, situated in the right hypochondrium, about the outer edge of the rectus muscle, extending nearly from the eleventh rib to the right side of the symphysis pubis, and being particularly prominent about the situation of the inner abdominal ring. It somewhat distended the integuments so as to be perceptible to the eye, and might be considered to be about three inches in width.

His account of this swelling was imperfect, but he believes that he first discovered it last week, while he was absent on his journey. I was unable to satisfy myself as to its nature. It did not answer to the description of any kind of hernia. It was not elastic, nor could any fluctuation be discovered: it seemed to possess considerable solidity. No inflammation existed, as pressure did not detect any tenderness, nor was there any unusual tension over the rest of the abdomen. Turpentine injections were administered, and cathartics and opium taken by the mouth. The stomach rejects every thing, and the bowels are but slightly evacuated.

4th.—Worse in all respects; pulse 100; countenance bad; was bled again. Injections continued; no fecal evacuations; urine flows plentifully, but generally involuntarily.

5th.—Vomiting incessant. His strength appears to be rapidly giving way. No sustenance can be retained. Tongue brown. Pulse small. The tumour is larger, or the parietes of the abdomen, by sinking in, in consequence of his great emaciation, make it more apparent.

6th.—I was desirous of having another opinion on the case, and he was visited by my friend Mr. J. C. Swayne, surgeon of this city. Upon an attentive examination, as far as we could come to any conclusion, the tumour appeared to be a mass of internal disease, agglutinating the contiguous parts, pressing upon the bladder, and impeding the action of the intestines. By both of us the patient's speedy dissolution was expected. To his friends and himself the same event appeared so certain that he made a final settlement of his affairs with considerable effort. For the last two or three days he has spoken as if he anticipated a fatal termination. Small but frequent doses of cathartic extract, with opium and purgative injections were ordered.

7th.—He becomes still worse; some delirium; urine continues to be evacuated, and there is no swelling immediately above the pubes. With the view, however, of exactly ascertaining the state of the bladder, and of assisting, by drawing of the water that might be there, the action of the bowels, we resolved upon introducing the catheter. So near did his death at this time appear to his friends, that they earnestly entreated he should be subjected to no further inconvenience, but allowed to have an undisturbed release. These objections were of course overruled, and I introduced the ca-

theter. It passed without any difficulty, and a forcible flow of urine through it occurred. The tumour immediately began to subside, and by the time about three pints of water had been drawn off it entirely disappeared.

The general nature of the disease was now apparent. It could not be doubted that the tumour was a preternatural enlargement of the bladder, and it seemed most probable that the elongated part was the internal coat protruded through the muscular coat; in consequence of which, the natural efforts of the bladder to expel its contents forced them into this cavity, instead of overcoming the cause of resistance at the neck of the bladder. To what extent any morbid impediment existed at the neck of the bladder it was not easy to determine. The catheter passed without obstruction, and examination per anum detected no disease of the prostate gland.

In a few hours, when the tumour began to form afresh, the urine was again drawn off; the vomiting lessened, and the pulse in the course of the day became firmer.

8th.—Vomiting less frequent; urine drawn off night and morning; the vesical tumour is formed some hours before the introduction of the catheter; some feculent evacuations followed the enema.

9th.—Vomiting nearly ceased; feculent discharges after the enemas; no power of voiding the urine, but it flows involuntarily upon the re-appearance of the swelling. He takes nourishment.

13th.—No vomiting; good alvine evacuations from the injections. He was taught to introduce the catheter himself, and directed to empty the bladder every five or six hours, so as to prevent the formation of the tumour.

20th.—Continues to improve. There is no involuntary discharge of urine, nor can he void any excepting by the assistance of the catheter. Mild alvetic pills act favourably upon the bowels.

His convalescence was slow but regular, and he is now (August 1828,) returned to his usual state of health, excepting that he feels less strong than he was before his illness. He never allows the bladder to become so full as for any involuntary discharge to take place, or for the tumour to become perceptible. No voluntary power over the bladder has returned. Pain along the urethra is the indication of the necessity to introduce the catheter, and this generally occurs every five or six hours. He is able to walk about and use his accustomed exercise.

It is probable that some of your readers may feel surprise that the nature of this gentleman's complaint was not sooner detected. Without any attempt to dispute their penetration, or to justify my own want of it, I give the case just it occurred in practice, with the hope that it may prove useful to others. Late as the knowledge of the disease was obtained, it was a source of great satisfaction to me that it was procured in time to relieve the patient, instead of being discovered by a post-mortem examination—a period to which alone at one

time I looked for an explanation of the symptoms.

When the nature of an obscure disease has been unravelled, there is often but little difficulty in deciding upon the course that should have been pursued: but they who have been longest accustomed to medical practice, can best estimate the difficulties with which the path of a practitioner is beset in cases of an ambiguous kind, where a valuable life is at stake, and where the hopes and fears and interests of anxious relations are contributing to perplex his mind, and to increase his diffidence of his own judgment.—I am,

JOHN BISHOP ESTLIN,

Member of the Royal College of Surgeons,  
London, and of the Royal Medical Society, Edinburgh

Bristol, August 16th, 1828.

From the Medico-Chirurgical Review.

AN ESSAY ON THE REMITTENT AND INTERMITTENT DISEASES, INCLUDING, GENERICALLY MARSH FEVER AND NEURALGIA; *comprising, under the former, various Anomalies, Obscurities, and Consequences; and, under a new Systematic View of the latter, treating of Tic Douloureux, Sciatica, Headach, Ophthalmia, Toothach, Palsy, and many other Modes and Consequences of this Generic Disease.* By JOHN MACCULLOCH, M.D. F.R.S. &c. &c. Physician in ordinary to His Royal Highness Prince Leopold of Saxe Cobourg. In two Volumes, 8vo. 1828.

In some late Numbers of this Journal, we have given a very full account of Dr. Macculloch's work on Malaria, as the cause of the various (at least variously denominated) diseases in the two volumes now before us. It will require three or four articles to convey any thing like an analytical delineation of these volumes; for, although it will be readily perceived that our learned author has a hobby-horse, on which he has quietly rode for more than 20 years past, pursuing the bent of his own inclinations, investigations, and meditations, yet we do not think that his speculations are all visionary, though some of them are probably seen through a coloured, if not a distorting medium. Dr. Macculloch is a great philosopher and logician—and he wishes to see all medical inquiries carried on according to the strict rules of philosophy and logic. That he is frequently a little ruffled in his temper, on finding that physicians have not generally conducted their investigations on these *beau-ideal* models, will be sufficiently apparent as we proceed. That he is sometimes pretty sharply critical—we had almost said *cynical*, will probably be inferred from the following passage in the preface to these two volumes. After telling us that science begins with conjecture and assertion—that its infancy is the reign of the imagination—that “physic is yet in that very infancy, wandering about its own fairy land”—that this science

abounds in bad observation, in imaginary experience, “and even in positive *mala fides*”—“that the laws of philosophy and logic (what will Sir Gilbert Blane say to this?) have scarcely yet found their way into it,” our author winds up with this terrible Philippic.

“The language of truth is simple and brief, but that is not the language of physic. Its words have meanings, and the same words have always the same meaning: but this is not the language of physic. The language of error is multitudinous, variable, vague and unsteady: and this is the language of physic. If there be a philosophical reader who doubts this, if there be a logician, a man accustomed to evidence, who has not read medical books, let him read even the most celebrated, and be satisfied.”

These are hard words master! Perhaps, if the learned author had toiled, during the said 20 years, at the bedside of sickness, he would have been more merciful to his brethren. It is not in books alone that we see the most clashing testimonies, contradictions, and contrarieties. He who has seen most of human nature, and especially of human maladies, is most convinced of the existence of these contrarieties, and of the impossibility, in the present state of our knowledge, of reconciling them by means of philosophy and logic. We make allowance, therefore, for Dr. Macculloch, because we believe he has been in the habit of viewing things rather as they *should be*, than as they *are*. The philosopher forms an imaginary picture, in his mind, of what the order of things ought to be in this world—but when, like Parnel's hermit, he comes out among men, he discovers a terrible difference between the closet and the crowd! The disappointment of the worthy Doctor, at not finding physicians to be philosophers, brings up the rather ludicrous reminiscence of Sir Joseph Banks' disappointment, at finding certain little animals remain black, after undergoing the severe discipline of ebullition,

“Fleas are not lobsters, d—— their souls.”

Dr. M. observes that—“he who is now the philosopher in physic, is also a Pythagoras, while he does not *perceive* it.” We confess that we cannot clearly *perceive* the drift of this occult passage—nor that of the following:—“The physician is like Nebuchadnezzar—he dreams, and death is the sentence of him who cannot divine what that dream was.” We humbly propine that, when the physician dreams, he also sleeps—and we have a notion, that Death is not always most active when the doctor nods. But, leaving all prefatory matters aside, we come to the philosophy and logic of the work. The first of these two volumes is entirely occupied with the subjects of remittent and intermittent fevers—dysentery—and cholera. The first of these topics will afford ample materials for this article.

ORDINARY REMITTENT OR MARSH FEVER.

Dr. M. must be an extremely discontented man. He sets out by informing us that, on no

disease in the whole circle, has he derived less information from books than on this. "He must labour with no small discrimination, who would, from medical works, extract any rational account of the immediate causes of the disease, or of the real condition of the system under it—who would discover any intelligible and consistent method of cure—who would even be always certain that it is of this disease, and not of contagious fever, that he is reading." What if we were to tell Dr. M., that the causes of this fever are not always precisely of the same kind—and that this observation might apply to the conditions of the system—and, consequently, to the methods of cure! Nay, we would have little hesitation in going a step farther, and saying, that Dr. M. himself would not always be able to discriminate between marsh fever, under particular circumstances, and contagious fever, if it do not actually take on a contagious character.

The object of our author, in this essay, is to class together some disorders which appear to him to have been misunderstood, as well as misplaced, in relation to their causes and characters—and, consequently, whose treatment has been erroneous. At the head of this class, as the offspring of malaria, stands remittent fever—the most important link in the philosophical chain—and the cause of many other diseases. Dr. M. properly observes, that the more perfect forms of diseases, as described in books, are much more rare, in nature, than the ill-defined forms, and, hence, the unreasoning, and, indeed, the inexperienced practitioner, is daily at a loss to name or classify the multitudinous forms that present themselves to his view. It is our author's object, therefore, in this work, chiefly to notice that which is obscure in itself, or least generally known, in marsh fever; thus making the essay a kind of supplement or addition to the systematic descriptions already existing.

Although there can be no reasonable doubts, that the general cause of remittent fever, in its perfect form, is the application of malaria; yet our author acknowledges that various other causes do produce it, as heat, errors of diet, fatigue, cold, mental anxiety, &c. "But every one of these is an accessory cause of many other disorders;" and if malaria be more copiously generated, or more widely diffused, than is usually suspected, it may still be the real productive cause, where the causes above-mentioned are only auxiliaries, though the only ones that are apparent. The subject of the malaria itself has been amply discussed in a former volume, and all that he can permit himself here to say is—"that he has attempted to prove that all the fevers of any moment, which are not produced by contagion, are the effects of malaria, very often, or, perhaps, very generally, overlooked." All other fevers, arising from other causes than malaria and contagion, are comparatively trifling in number or power. Our author recognises one exception to the rule respecting the cause of remittent fevers, namely, HABIT. It is well known that agues and remittents are some-

times reproduced, where the original cause cannot be in operation. It is probable, however, that, in all these relapses, some of the auxiliary causes have been exerting their influence on the constitution.

After some observations on the comparative susceptibilities of natives and strangers, in malarious countries, Dr. M. touches on the *time* which intervenes between the reception of the poison and the manifestation of its effects.

"If my own frequent observations show that fever may be induced within half an hour after exposure to malaria, and that a single inspiration, or the space of a very few seconds, is amply sufficient for the purpose, this is also an opinion most decidedly stated by many French and Italian physicians whose experience and acuteness will not be questioned. It is equally the opinion of other observers, not physicians, and, therefore, without the bias which might be suspected in such cases: of military, and chiefly of naval men, whose observations have been founded on the momentary and transitory effects of a breeze of wind, and especially of a land wind blowing off to sea. In France and in Italy, to confirm this, instances are known and recorded, of labourers dying instantaneously from merely sitting or lying down on the ground, and of others who from looking into a ditch or drain, have been struck dead by that poison which, of course in a minor degree, would have merely produced a fever. Lind, also, whose authority stands high, describes the instant seizure with nausea and delirium, as many others have done; so that respecting this part of the question there needs be no dispute."

That the miasmal poison may, in certain states of uncommon concentration, be capable of producing an instantaneous, or even fatal effect on the nervous system, we do not deny; but that a regular fever is so quickly produced, we are inclined to doubt. It seems, in general, to require a certain period for concoction in the system, before fever is evolved. The extreme length of the interval next engages our author's attention, and after a minute examination of evidence, he appears to conclude, that 15 or 20 days from the utmost limit—perhaps more than the limit. Thus a ship's company became affected with remittent fever, on the coast of Africa—the ship put to sea, but some of the men continued to fall ill with the fever till the 20th day, after which no more cases occurred. Here, too, Dr. M. says there may have been fallacy. There may have existed a foul hold, from whence the febrific miasm continued to ascend—or the fever might have become contagious. "A remittent will become, or perhaps produce, in any given individual, a contagious typhus, under confinement." But independent of this hybrid, or contingent disease, we think there are sufficient facts on record to prove, that miasmal fever may take place after a longer absence from the source of the miasm.

"Many physicians or surgeons, both English and French, have said, that even after six

months, many soldiers who had been at Walcheren and had escaped the fever there, were seized with the same disease in other countries; asserting also, that the poison had remained during the time dormant in the constitution. Pym is one of those who thinks thus, and so I imagine does Blane; while Bancroft believes, that the intermittents of spring are the produce of malaria received in the previous autumn. Baumes, resembling Lind, limits the term to fourteen days; but Ferrus, coinciding with the former, relates in proof, a case of a soldier who having escaped at Walcheren, was affected with this fever six months after, on the Niemen; as there is also a case quoted in evidence, where an English regiment became attacked in the same circumstances in England, after eight months."

An objection might be readily raised to the latter fact, that a fever produced *on* the banks of the Niemen, may be produced *by* the banks of the same river. And so an English regiment serving in Spain, after the expedition to Walcheren, might very readily pick up some malaria in the former place, without any necessity for carrying it from the banks of the Scheldt. In a physiological point of view, it is difficult to conceive how a poison of this, or of any other nature, should lie dormant so long in the system. It is easier to conceive the reception of a new poison, and the production of a new disease.

Dr. M. very properly passes over, by a reference to a host of authors, the symptoms of remittent fever, both in its simple and complicated forms—together with the almost endless modifications produced by climate, epidemic influence, and various other causes, including differences in the miasma itself.

"But I may mention, that while an undue, and apparently a morbid secretion of bile, is the most conspicuous and common local affection, producing sometimes what is emphatically called the yellow fever; so the brain and other organs, and above all, the stomach and the bowels, are often found affected by inflammations, modifying materially the symptoms, and also demanding important modifications in the practice."

In referring to systematic writers for general descriptions and local peculiarities, Dr. M. feels himself compelled to notice that characteristic symptom, whence the disease derives its name. This is the remission or diminution of the intensity of the febrile symptoms, occurring once in the 24 hours, however variable in the period of its arrival, its duration, and degree. Sometimes this remission is so slight as not to be perceived, though errors are often committed here, especially when the remission occurs in the night, or in the absence of the medical practitioner. For our own parts, we can hardly say, that we have ever seen a decidedly continuous fever. There is an evening exacerbation and morning remission in almost every case. It is hardly necessary to say, that it is by a gradual prolongation of this interval of remission, that the so called continued fever of malaria becomes an

intermittent—and this leads our author to speak of the terminations of the disease. There is, no doubt, a disposition in most fevers to resolve on certain days termed critical, though these are now but little observed.

"Three weeks may probably, however, include the much greater number of terminations in recovery, when the disease submits to the law of the critical days; while it is not uncommonly protracted to six weeks; and even in cases where its extreme mildness might have led us to expect an earlier solution."

Where this law of crisis does not seem to exist, we can assign no period for the recovery or the fatal termination. The favourable termination is often perfect, in all climates; but it not unfrequently changes to an intermittent, easily, in general, removed by proper remedies—but often peculiarly inveterate and inimitable, as those who have witnessed Walcheren, some parts of China, Greece, Italy, Spain, Moldavia, and many other countries, can testify.

But remittent fever not only terminates in ague—it frequently produces, or ends in, "the local and painful affections of the nerves which may be ranked under the general term NEURALGIA." After alluding to the production of "paralytic affections," by remittent fever—or the termination of this fever in such affections, Dr. M. makes the following observations, which bear on a point of pathology, or etiology, recently mooted in one of our medical societies.

"That marsh fever does act directly, itself, or its generating poison, on the nervous system, is proved by the state of apoplexy or profound coma with which the attack is sometimes ushered in; a fact common in Italy, and known by the name of *febbre larvata*; though, in this case, mere intermittent may also be the supervening disease, instead of remittent. That, in these fevers, the affections of this nature have been attributed to local diseases of the brain, I know; and such events may doubtless occur. But this does not explain the cases in question, where the affection of the brain is instantaneous, following directly the application of the poison, even before fever is produced, and resembling that which occurs from the application of other poisons, whether to the lungs or the stomach.

"Farther, as it is the effect of Malaria to produce the local affections of particular and single nerves, either with supervening or present palsy, or without either, while the brain is not affected, and while no local inflammation or other disease of that organ can be supposed to exist, from there having been no previous fever, it is plain that Malaria does exert a power of some kind on the nervous system directly; on the whole, or on more or fewer of its parts, even to a single point in the minutest nerve."

Thus the larger paralytic affections, as hemiplegia, or paralysis of a leg or arm, consequences of an intermittent or remittent, may, he thinks, result from "direct action on the

nervous system," and, in this way, he also thinks, may be explained that diminution of intellectual power, proceeding often to perfect idiotism, which sometimes follows long-continued intermittents. Dr. M. admits, however, (what we think is the more probable solution,) that there may, in such cases, be organic disease in the brain itself, the product of local inflammation there. He does not consider this, indeed, as the more common source of the paralytic or cerebral affection.

"But if malaria does produce direct apoplexy, as it also often brings on a comatose state which is exceedingly durable, both in remittent and intermittent, and if also it produces, not only local and similar effects on single nerves, but complete hemiplegia, it will be most necessary to inquire whether some of the cases of paraplegia or other palsy, especially as occurring in certain climates and in campaigns, are not instances of the same nature; since, whether our practice in such a disorder should be different or not from the treatment of palsy produced from simple cold, the philosophy of physic cannot fail to be improved by discovering causes and assigning distinctions."

The subject of visceral or glandular disease is next taken up. Most authors have looked upon these as the effects of the fever which preceded them—though a few writers have considered the visceral affection as the primary, and the fever as the secondary link in the morbid chain. Dr. M. appears to think that both hypotheses may be occasionally right. Thus, some physicians have maintained that the hepatitis of India is the direct effect of a morbid miasm—and, if so, other glands and organs may be the primary seat of action when malaria is received.

"Thus also, through France, Italy, Sicily, every where, it is common to find, not merely single instances, but a whole population, suffering from glandular diseases in their worst forms; while no fever is present, and while also, in many cases, it seems to be ascertained that no fever has preceded, or that there has at least been no severe remittent or intermittent as the cause."

Our author concludes this chapter with an expression of his disapprobation of those wild theorists who disbelieve in contagion altogether. No doubt the great mass of fevers in this country are devoid of contagious character, because comfort and cleanliness are observed—"but to assert that there is not such a thing as contagious fever, is to discredit evidence as numerous and incontrovertible as science or human affairs have ever produced." The fevers not contagious, he thinks, must fairly be ascribed to malaria.

The second chapter is on the chronic or relapsing, and obscure or anomalous remittent fever. This, Dr. M. observes, is a modification which is often but little noticed, while it is a source of great distress.

"If this peculiar variety is sometimes sufficiently severe and marked to be esteemed a fever, it is far from uncommon for it to be so

slight as to pass for hectic, for what is called debility (a term without meaning) or for ill health, or delicate health, terms equally convenient to cloak ignorance; while not unfrequently also, it is characterized by the no less convenient phrase nervous, or even brings on the unlucky patient the charges of hypochondriasis or affectation."

To be more particular; there is, says Dr. M. a fever not uncommon among us, to which the popular name of nervous fever is not inaptly applied, when its symptoms are not severe. Dr. Cullen was unpardonable, he says, in confounding this with contagious fever, under the name of typhus mitior. It is not contagious, nor produced by contagion, as far as his own observations have gone. "Durability, or the property of prolongation, seems to be a peculiar character of marsh fever, under all its forms; and, until an unequivocal case of contagious fever, thus mild and thus durable, is produced, I must continue to believe, that all long-continued or often-relapsing fevers belong to the disorder under consideration." The same remarks apply to what Cullen denominates synocha or synochus. By this, however, Dr. M. does not mean to deny that there can be such a thing as a pure inflammatory fever, produced by cold, or the other causes usually assigned; but he believes that the disorder so called is very frequently "a fever of the remittent family, and produced by the same causes." Many plausible arguments are adduced in favour of this position, for which we must refer to the work itself. If this "low fever"—"fever on the spirits"—"fever on the nerves," (by all which names it is known,) be not a modification of remittent, our author knows not how or where it is to be classed. Physicians must erect a new genus for it—since it does not belong to one or other of the two leading classes.

"And it is far from unimportant that this point should be clearly understood; as it is only thus that our practice can be justly regulated: while it is most certain, that by mistaking it for other diseases, the sufferings of the patients have often been, and are daily and every where, materially aggravated. And if the cause, the original one, be malaria, as in the case of acknowledged remittent, whatever the causes of the relapses may be, we thus acquire the means of prevention; of which, as long as we mistake its nature, we cannot avail ourselves."

Among other arguments in support of the said position, Dr. M. adduces this one, that the disorder in question is among those habitual complaints included under the vague term "ILL HEALTH," which are the produce of low and wet situations, or of some of the soils formerly described as productive of malaria.

"Another argument is, that its relation to the marked or severe and terminating remittent, in slenderness of symptoms and in the frequency of its recurrence, is precisely that which intermittent, equally slender and equally returning, bears to a limited and severe intermitting fever, while I may lastly add, as a

proof of its cause and return, that if it is especially subject to relapses in low and wet situations, as well as indebted for its very existence to those, so it is best cured, and especially when relapsing, or repeated, or chronic, by change of air; that is, by change to a drier air as it is usually termed, or, what is the fact, by removal from its causes."

Dr. M. thinks it probable that the disease in question, is that which Dr. Haygarth has described in the College Transactions, as inexplicable—"a peculiar state of permanent debility, enduring even for years, and without very marked disease of any kind." Unless it be attributable to the abuse of purgation and bleeding, Dr. M. is unable to account for this complaint, except by assigning some morbid exhalation from the earth as its cause. We must now allow our ingenious and indefatigable author an opportunity of describing the disease under consideration.

"This disorder may be found, and not unfrequently, with scarcely any marked symptom except mere muscular weakness; a debility on any attempt at exertion, which seems unaccountable, inasmuch as it occurs in persons, even in youth, and apparently strong, and is not very obviously accompanied by any proper febrile symptoms. At times, not even the appetite seems affected; and here, almost necessarily, the result is, to suspect the state of the patient's mind, or his moral dispositions, rather than his health; to suppose, for example, as I have often seen, that a soldier is 'shamming,' that an opulent female is indolent or affected, or a studious or professional man hypochondriacal.

"Yet, let an acute physician watch this disease, and he will be convinced that it is a disease, and moreover a fever. It commences and terminates like the remittent when best marked; and when it appears to be prolonged for months or years, as is sometimes the case, it will be easy to see that it has had intervals of cure, generally of self-cure, and relapse; and that, to each relapse, there is a period of weeks, not very uncommonly of six, while the intervals vary from one or two to any given number. Further, either the patient or the physician, or both, must be very inattentive if they do not discover that the paroxysm of extreme debility is fixed; that it is, in fact, a paroxysm, let its length be what it may, and that there is a diurnal period when it diminishes, or where the patient, who, possibly, could not stand, on getting up in the morning, is enabled to exert, and even to enjoy himself at night.

"Hence, as to some cases, at least, the truth of, as well as the reason for, a very common remark, that midnight is the nervous patient's holiday; though there are unquestionably many cases of nervous affection, and even of periodical returns and intermissions in this complicated class of disorders, which do not appertain to a remittent type of fever, or perhaps to any fever. The particular case here quoted, is one, of course, where the paroxysm attacks in the morning and the remission is at

night; but while the periods are necessarily various, so are the results, as to the complaints, appearances, or sufferings of the patient. I shall presently trace some others of the more marked of these modifications."

Between the above and the unequivocal forms of remittent fever, there are, of course, a great number of shades, in which the obscure symptoms gradually multiply and become more conspicuous, indicating, as they advance, a more severe disease. It would not be difficult, Dr. M. adds, for any attentive physician, in tolerably extensive practice, "to collect a series of cases rising in exact gradation, from the simplest debility to the most perfect form of remittent fever." The appetite, though sometimes apparently unaffected, is generally irregular or capricious—vanishing in the paroxysms, and returning in the intervals. The tongue and the secretions will usually show indications of disorder, but none peculiar or pathognomonic. These disorders of the *primæ viæ* Dr. M. looks upon, of course, as *effects*, though they are generally viewed as *causes* of disease.

"In the whole catalogue of ordinary practical errors, I know few indeed more common than that which views a sluggish state of the bowels as a primary disease, sometimes also a consequence of theoretic disorders of the liver, instead of considering it what it really often is, the produce of a febrile state, belonging, either to this fever or to some other initiative and similar cause. Nor is it difficult to account for this error, vulgar as it is common; since it is the consequence, partly, of seeing, in a disorder, nothing but obvious symptoms, and partly of that empirical practice for which England is so celebrated, and which, while it tends to blind the judgment, can, from its facility, be conducted by any one."

This will not be a very palatable doctrine to the routine practitioner, and especially to the disciples of Abernethy, who have had halcyon days of it, for many years past, in consequence of the great simplicity, or rather uniformity to which practice was reduced, as a result of the doctrine that made but one cause for all disorders. We are disposed to think that the moral world will not *alone* suffer from those disturbances occasioned by the "march of intellect"—the physical world, or at least the world of physic, will come in for a share of revolution.

But to return. Our author has no hesitation in averring, that fevers of malarious origin are a very general cause of the chronic and common derangements of the digestive organs, to which so much attention has, of late years, been paid. In respect to the state of the pulse, Dr. M. justly observes that, "there are persons who cannot conceive a fever without an accelerated pulse; whereas, even in severe cases of remittent, the pulse often gives no indications of any disorder, or the very reverse of what such practitioners would have anticipated." The fact is, that in these forms of complaint, the pulse may be natural during the greater part of the day, but considerably

accelerated for an hour or two, during the obscure paroxysm which marks the disorder. This often, perhaps generally, takes place in the evening or in the night, and consequently escapes the observation of the medical practitioner.

"The diseased state of the mind may, however, exist at two distinct periods of the paroxysm, and under two different states of the pulse. Under the accelerated or contracted one, it is a state of peevishness or irritability, attended by the feeling of despondency or not: or it is a modification of the great leading passion anger, which, together with fear, the equally inclusive and principal passion, forms those deranged states of mind appertaining to hypochondriasis, which appear under so many modes and modifications. And if under the full and slow pulse, it is commonly simple or passive despondency, or, in extreme cases, despair, so if the opposed condition or passion, irritability or peevishness, belongs to the accelerated pulse, that, in similarly extreme cases, may amount to anger, or to a tendency to that fundamental passion, easily excited by trivial causes; not seldom, difficult to restrain, even when no external cause is applied, or proceeding to causeless conduct, even in solitude, unaccountable to the patient himself."

This symptom of mental derangement may be the only one apparent to a bad observer. Despair and fear, especially the latter, appertain peculiarly to the class of marsh fevers—so much so that, in some parts of the Mediterranean, when these fevers are endemic, the only name by which they are known to the common people, is *scanto*—fear or fright. This circumstance is adduced by our author as corroborating his views of the connexion between hypochondriasis and malaria. He cautions the reader, however, against concluding that he is endeavouring to draw a sweeping conclusion that all cases of hypochondriasis are dependent originally on miasmatic disorders. Dr. M. makes many judicious and ingenious observations on the effects of this slow or obscure remittent fever on the intellectual faculties, but we cannot so far disentangle them of the monstrous load of verbiage under which they lie buried, as to exhibit them here. The reader must plod, and labour, and unravel for himself. The two principal mental conditions induced in this way, are torpidity and excitement.

"The state of torpidity or inability accompanies that condition which must be considered as the cold stage, or which is the commencement of the diurnal paroxysm; being noticed, of course, only when the attack commences in the day and in the hours of labour, and therefore often passing without remark. And in every fever, this is the period of peculiar mental inability; the one observation confirming the other.

"The period of excitement, or of increased, if of hurried mental power, on the other hand, is the hot fit, or that which is here its substitute; a period of partial delirium: and here also, that condition of mind which is useful in

moderation, is illustrated by the other, or by that excess which causes the imagination to run wild."

Other bodily symptoms are next investigated by Dr. M. Headach, though not always present, is much more common in women than in men. In the former, debility and headach often constitute the whole of the obvious disease. "With these symptoms alone, or apparently so, the disorder will sometimes run a course of six weeks, and with such severity as to confine the patient to bed." To the above symptoms may be added fits of restlessness or lassitude—occasional pains in the limbs—drowsiness, sometimes irresistible, and very remarkable in the chronic remittents of Italy and other malarious countries—natural sleep disturbed or irregular, being absent in the early part of the night, and coming on at the time when the patient ought to get up. The following is a curious instance of this class of disorders haunting an individual for 30 years!

"In this, the patient had, for thirty years, been subject to nearly all the diseases in rotation which I here rank under those of malaria, namely, to remittent, to intermittent, and to almost every known variety of neuralgia; having apparently acquired the incurable habit of these disorders at an early period of life. In several long intervals among those more marked ailments, the same person had also been affected, for long periods, with simple coma or drowsiness, and further with nocturnal awaking in the state of partial delirium just described: and it was his invariable remark, that the hour of awaking in this manner, was always precisely the same as that which marked the paroxysms of the intermittent and those of the neuralgia, indicating their joint dependence on one cause and one habit; while that hour scarcely ever had varied by many minutes during the whole of his life of disease."

A disordered state of the digestive organs is so inseparably connected with the complaint in question, that our author seems doubtful whether to consider it a constituent part, or a consequence of the general malady resulting from malaria.

"Let physicians watch their own cases of this disease with this new light, and they will scarcely fail to find evidences of their own which will be much more satisfactory. And they will recollect also, when they reflect on their practice, how often they have found dyspepsia periodical under diurnal returns of various kinds, how often periodical and dependent on seasons, how often they have seen it cured by merely altering the hours of eating, how often by bark, or by arsenic, or by the other tonics that cure the remittent and intermittent diseases; and how often by change of air, and also by mental affections, or causes operating on the imagination, such, among others, as the change of physicians; all of them remedies for the intermitting diseases in question."

The author observes that, in the various works which have been written on dyspepsia,

he has found no author who has taken this view of the complaint—who has looked to malaria as the cause. It is true that Dr. M. is original in making miasma so prominent a cause of dyspepsia, but if he will examine the works of authors again, he will find that “*impure air*”—which surely must be synonymous with *malaria*, has not been overlooked in the etiology of dyspepsia.\* But malaria is only one of the thousand *physical* causes which disorder the digestive functions—and for one physical cause of this disorder, there are, at least, two moral causes! It is but fair to say that Dr. M. considers the catalogue of real causes of dyspepsia too long for insertion in his work.

“Of the very numerous real causes of this common disease, I cannot pretend to give even a catalogue, as that would be to transgress my plan; nor could I therefore enter upon any comparative view of the predominance of these several causes, or attempt to suggest what place the one here proposed may deserve among them.”

Hysteria is considered as one of the symptoms attending this class of complaints—“though rather an incident than a portion of the disease.” In some of the more strongly marked forms, however, of the low remittent, “the occurrence of the hysterical paroxysm, which is rarely more than a fit of crying, is commonly as regularly periodical as any other portion of the disease.” Dr. M. has generally observed that it attends the subsidence of the pulse, or, in other words, the termination of the excitement. It is curious, that where the disease altogether is so slight, that the patient, if courageous, bears up against it, and does not complain, this hysterical weeping or feeling, is the only part of the disease which cannot be resisted. “A long-continued attempt at exertion, or at concealment which is exertion, is, in such cases, generally followed by a hysterical affection unusually severe.” We coincide with Dr. M. in believing that most of those recorded cases of *periodical* hysteria belong to this class.

“It is one of the effects of this remittent, particularly when chronic or habitual, to produce those general derangements of the entire health which it would be tedious and equally useless to enumerate, as they are familiar to every one; while, with that, the temper, and even the moral character, as it may be considered, become also permanently or habitually injured.”

Among the symptoms or consequences of the obscure malarious disorder now under

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\* While enumerating the *causes* of dyspepsia, Dr. Johnson makes the following remark:—“Where air, imbued with millions of *miasmata*, exhaled from every thing in the animal, vegetable, and mineral kingdoms, is breathed, swallowed, and kept in contact with the skin, the effects are conspicuous in the sallow complexions, puny or capricious appetites, and *imperfect digestion* of the inhabitants.”—5th Edition, p. 53.

consideration, we must not pass unnoticed those menstrual errors generally presented in the shape of amenorrhœa and dysmenorrhœa, the former particularly being often attended by a chlorosis that is mistaken for an original disease. We agree with our author that these complaints of the uterine system are “much more commonly the results of some derangement of the health than the sources of that ill health by which they are accompanied.”

Dr. M. has hazarded an opinion, that the climacteric disease, described by Sir Henry Hallford, is a form of this chronic and obscure malarious malady. His arguments and illustrations will be found between page 105 and 108.

The original cause of this class of complaints is, of course, the malaria so ably investigated in a preceding volume.

“Besides this original cause of all the evil, however, numerous other causes inducing debility will reproduce the relapse, and thus tend to perpetuate the disease; and the more readily as it is a more confirmed habit. Such are ordinary cold, fatigue, intoxication, bleeding, the excessive use of saline purgatives, mental affections, with others unnecessary to name; all of them equally efficacious in recalling the returns of a chronic tertian or quartan. Of all those causes, I would particularly notice here the use of purging, and mental affections; as, respecting the other, no one doubts much, and as they are commonly avoided. That what is called ‘a course of salts,’ will very often reproduce an attack, I have abundant proof; and it is especially necessary to notice this, since it is commonly resorted to as a remedy for the imagined diseases to which the symptoms of this fever equally belong. Hence the frequently injurious effects of that fashionable folly, the frequenting of mineral wells; a practice resorted to by presumptuous patients, or by vulgar practitioners, as if it must be universally salutary, and was incapable of doing harm. And the common error in this case, as it is the especial cause of this erroneous and pernicious practice, is the mistaking the derangements which I formerly noticed for dyspepsia, as it is called, or liver complaints, or whatever else, under this received phraseology which is now so current; while the empirical practice to which I then alluded is applied without examination.”

The existence of local pains in this class of complaints often induces to blood-letting, which generally aggravates the symptoms it was meant to relieve, besides protracting their duration.

“As to the influence of mental affections, it is rather a matter of curiosity than use, as the injurious occurrence of these can scarcely be guarded against; but it is, in the philosophy of physic, an interesting fact to observe, how instantaneously sudden grief, fear, disappointment, or other strongly depressing passions, will bring on that relapse which will generally run the same course as all the preceding.”

Dr. M. remarks that, although this obvious

disease has been so much overlooked, he has no doubt that it will shortly appear very common, now that it is distinctly pointed out. England has, till very lately, been so free from intermittent fevers (which form the readiest road to the analysis of the present disease) that but a few, proportionally, of medical men, have seen an ague at all. Hence, too, their attention has been but little directed to the investigation of malaria, as its source. There can be no doubt also, that in numerous instances, fevers resulting from miasmata have been confounded with typhus.

#### PROXIMATE CAUSE OF MARSH FEVER.

Our satirical author remarks that, it would be well if the professor, who spends months in exciting the wonder and applause of a juvenile audience with phraseology which he does not himself understand, would substitute for all this waste of words and time, the confession of his own ignorance. "For never yet has philosophy thriven by dressing up fiction and vain speculation in the garb of truth and sense. Physic knows not how the poison of fever acts, nor on what it acts—what are the preliminary effects which produce the symptoms that are obvious to our senses." We cannot, he says, even conjecture why these actions should cease—why they should be renewed—or why they should cease to be renewed. But, although Dr. M. can offer no theory of his own, he takes leave to criticise those of others. Dr. Clutterbuck's doctrine of cerebral inflammation he passes over. The action of malaria, say the French, is stimulant, and the symptoms of debility which succeed, are the effects of previous exhaustion. On this the following remark is made.

"If indeed the action of prussic acid, or lightning, or a cannon shot, is sthenic, then the assertion will not be disputed: and thus, he who, under the action of malaria, falls down instantaneously with apoplexy, has died of over-excitement."

The fever of marshes, say others, is a gastro-enteric, or inflammation of the mucous membrane of the stomach or bowels—and every other effect and symptom is sympathetic or consequential—and the success of the practice is said to be confirmative of the theory. Dr. M. loudly protests against this exclusive theory; but he only adduces those arguments, which others, as well as ourselves, have repeatedly brought forward against the abuse of the doctrine. "Whatever dissections have taught, they have not taught us the cause of marsh fever." "They have taught us, that certain effects take place occasionally:—that is their use." The inflammations are effects not causes. Dr. M. admits the periodicity of inflammation in certain agues and neuralgias, but contends that these inflammations are not common, but specific inflammations; *sui generis*—and the circumstance of their being cured by bark and arsenic, proves them to be so. It is hardly necessary, after these observations, to say that our author gives up,

in despair, any attempt at a proximate cause of the disease under review.

*Treatment.*—Dr. M. sets out by renouncing all idea of the treatment of remittent fever, as it appears in tropical climates, or in aggravated forms in any climate. "The most opposite opinions have been entertained, and the most opposite practices followed. As happens in tetanus, all these modes have failed—all these modes have seemed to succeed."

"The conclusion of him who knows nothing of physic, will probably be that the imagined remedies have had no concern in the cures, though he will scarcely conclude that they have had none in the ill success; while a fatalist in medicine, as fatalists there are, will perhaps determine that the efforts of the physician are nugatory as to either event."

Dr. M. touches on the administration of emetics, at the very beginning of a remittent, with the view of "putting it out." He does not seem to have much confidence in such a procedure, and thinks that it often produces or aggravates that peculiar inflammation of the stomach, which accompanies the fever. "How often death has been the result of such emetics, given improperly, or pushed too far, is well known." The general antiphlogistic measures necessary in the early stages of fever are passed over, as universally agreed upon. In respect to purgatives, Dr. M. makes some judicious observations. Where bleeding and antiphlogistic measures are proper, the saline purgatives, by producing watery motions, lessen the whole circulating mass—and are thus useful; but where the disease will not bear, or does not require sanguineous depletion, the said saline purgatives are improper, as tending to induce debility. Those resinous purgatives, however, which act locally, as wine, and merely promote the natural evacuations, are beneficial in all fevers, and almost all stages of fevers. On calomel, Dr. M. also makes some comments. In the remittents of hot climates, he has no reason to doubt its efficacy—in the milder fevers of our own country, he does not suppose that it exerts any specific influence "beyond such as is derived from its power over the biliary system." In regard to the chronic or relapsing variety, to which our author appears to have paid much attention, the following observations are deserving of notice, especially as they come from "one of those physicians who consider that this medicine (calomel) has been greatly abused."

"This remark is, that in the relapsing disorder, even if the attack should occur at the end of winter, in patients free from all suspicion of deranged liver or biliary affections, and when not the slightest indications of these can be traced, and when, further, the relapse may be the twentieth or fortieth to that patient, the operation of calomel is to produce obvious effects, which, if I need not specify them, physicians know well to be those which never occur except under derangement of this secretion. And at the same time, what is the important point here, it will be found that

after every such effect of the medicine, the force of the disease diminishes, and that whenever the natural secretions recur, that particular relapse is about to terminate. Hence, therefore, I am led to consider, that even where it is least suspected, and indeed not to be believed present, there is often, in the chronic relapses, a derangement of the biliary functions; and that calomel, being the remedy for these, is apparently a remedy which cuts short or cures that relapse."

The use of the medicine is recommended as long as it is found to produce the evacuation of morbid bile:—when this disappears, the calomel becomes injurious.

On sudorifics, opium, cold affusion, and diet, some observations are made that do not require notice. Dr. M. very properly remarks, that food should never be given, however light, except in the intermissions or remissions. At all other times, even in the most mild and chronic remittent, it does much mischief by ruffling or irritating the stomach.

The grand point or question is that of administering tonics—and more especially bark, in this class of diseases. Dr. M. acknowledges that this is "a complicated question." There can be no doubt that there are instances where bark fails, or even aggravates the disease, even where no inflammatory symptoms are present. But, on the other hand, there are far more instances on record, where bark has cured the disease, during the actual and unequivocal existence of inflammatory action.

"It is as painful to a writer to leave his readers in suspense on points so essential as it would be presumptuous to decide; yet it may be suggested, that if, as will hereafter appear, the inflammatory affections of remittent are of a peculiar character, and not proper phlegmasiæ, and if certain visible and demonstrable ones are actually cured by this remedy and aggravated by evacuations, the question will not improbably be decided in favour of those who recommend it in all cases; and it may not be difficult then to discover that prejudice or incorrect observation will explain that testimony against it which has been thought to be derived from experience."

No doubt exists as to the utility of bark, where there are intermissions—or even pretty fair remissions, with general symptoms of debility. The very circumstance of there being a tendency to remission or intermission in a fever, is in favour of the utility of tonics, when not strongly contra-indicated by local phlogosis.

On the subject of wine, Dr. M. entertains some eccentric, or at least heterodox opinions. It is proved, says he, that "wine increases inflammation, when existing, or produces a tendency to it in healthy subjects, or in diseased ones?" He asks whether these opinions are grounded on observation, or only hereditary dogmas "established no one knows why, and followed because they have been followed."

"But, granting that there are cases of inflammation, or a species of inflammation which wine would increase, physicians know full

well that they are utterly ignorant of the real distinctions among inflammations which, to the sense or the eye, may appear the same; and that while there are some kinds or varieties which are to be cured by stimulants both local and general, as I shall hereafter show very fully, so are there inflammations, and apparently inflammatory states of the entire system with increase of circulation, where wine is a remedy instead of being injurious. Nor does it appear that the habitual use of wine produces a tendency to inflammation in healthy subjects; since it is notorious that among water-drinkers, the diseases of active inflammation are most frequent and require the most energetic treatment."

The remarks on blood-letting need not detain us. In robust subjects—at the first attack of the disease—the loss of blood is often useful in reducing that activity of the circulation, or that vigour generally, which renders the first portion of the paroxysm severe. When, also, there is unequivocal evidence of topical inflammation, we ought to have recourse to topical depletion. But, we need hardly say that Dr. M. is not among those physicians or surgeons who conceive that fever can be "put out" by venesection, however decisive, and however early employed,

"Were I indeed to indulge in that violence of generalization so usual with physicians, and so much too prevalent among those who undertake to point out or review a system of practice, it would be to assert that it were better that blood-letting should be utterly abolished in this fever, than that it should hold a place so egregiously abused."

From *VENESECTION* Dr. M. makes a rapid transition to *WINE*, the use of which in fever, says he, is "too often made a question of fashion and temper, rather than of rational and sober inquiry." How can we expect *sober* inquiry where wine is the topic? The following short quotation is all that is necessary on this point.

"That its singular combination of stimulant and sedative powers renders it one of the most convenient of the remedies generally classed under the vague term of tonics, seems to have been established by experience that can hardly be disputed; and the most determined theorist can scarcely deny to himself, that he has gained decided advantages from its use in the low or later stages of fever, and that it has often appeared to him the means of at least supporting the patient to a favourable termination."

Dr. M. after some remarks on subordinate agents in the treatment of fever, comes to the management of those milder varieties which he has described at such length in the volume before us. The following sarcastic observation is not destitute of foundation in truth—nor is it inapplicable to the *routine* practice of the day.

"A sweeping conclusion as to the ordinary simple fever of this character, would be, that it requires no remedies at all; and most assuredly, it is far better left to its own opera-

tions, or to nature, as the phrase is, than that it should be tampered with by intermeddling and routine practitioners. There is little good to be done by remedies; but it is not so as to the harm. Left to itself, its periods proceed in a very orderly manner to a favourable conclusion; but it is rarely so when a busy or active practice interferes. What the evils to be produced may be, can so easily be concluded from the preceding remarks, that I need not detail them."

A great deal of this routine and injurious POLYPHARMACY depends more on the wretched and distracted *system* of medical practice and education in this country, than on erroneous views of the disease. The medical man's *time* is his only fortune. Chronic diseases occupy far more of this valuable property than acute diseases; and as the general practitioner is not allowed to charge for his visits and his *advice*, he must necessarily send medicine in such proportion as will remunerate him for his *time*. Even when the physician or surgeon is called in, he must, as a matter of conscience, prescribe in the manner that repays the general practitioner for his attendance. He cannot, and indeed he ought not, to do otherwise—for, let it be remembered, that we are not to ruin ourselves for the sake of that public which persists in withholding from the profession the *proper mode* of remuneration. A patient will pay *only* for medicine—he considers the skill which directs that medicine as not deserving of any pecuniary remuneration. In God's name, then, let him have medicine—usque ad nauseam! The perversity of human nature, on this point, is really astonishing. The conscientious medical man, when he does not see clearly his way, and where no evident indication is to be fulfilled, would prescribe mere placebos—the common saline draught or camphor julep, which can do no harm, and by which he is remunerated for watching the disease. But the public is now got too enlightened to be put off with placebos. They must, forsooth, have medicines that will produce some ostensible *effect*, or no value is attached to the prescription! Assafoetida and camphor won't do. No! They must have something that will purge them, sweat them, make them sick, give or take away the appetite—in short, something that does violence to the constitution, or else the physician is an old woman, and the surgeon is picking their pockets by cramming down their throats a quantity of medicine, "which does them no good." Every medical man in *actual practice*—that is, who does not practise in the garret, with the gray-goose quill—can vouch for the truth of these observations. The natural and the inevitable consequence of such a state of things is, that all classes of medical practitioners are led into a system of prescribing *active* medicines, in *chronic diseases*, instead of managing the said complaints by a system of dietetics and general hygiene, that would be of far more use than medicine. Nothing but a general and consentaneous impulse and co-operation among medical practitioners can

effect a change in this system, *at present*; though we are confident that reason and common sense will, sooner or later, correct the evil. A common *consent* or co-operation among medical men can hardly be expected, for some time to come, since the prevailing mania of the day is any thing but *concord*. Into whatever circle of society a medical man now goes, the common remark which he hears is—"what a *row* there is among the doctors!" In short, the medical profession is becoming the bye-word of derision among all the more enlightened classes, and we predict that the day is not far distant, when they will go in "sack-cloth and ashes," for their stupidity in being led away from the dignified paths of science into the arena of scurrility and personal abuse. But to return from this digression.

Dr. M. does not mean to say that the disorder in question should be absolutely left to itself—he only cautions against heroic remedies. *Quietude* is considered an important measure.

"Thus when the whole visible paroxysm is nothing but a muscular debility which, with care and rest, would have been limited to a few hours, an exertion through that time will not only protract it through a considerable interval, but also produce other symptoms and greater inconveniences: such as, increase of pulse, headach, additional loss of appetite, and an augmented sense of general suffering. The same consequences also follow from mental exertion under the same circumstances; and if I need not repeat what I formerly said respecting sleep, it is plain that the reasoning is the same. In all these cases, that indulgence to the feelings or caprices of the patient which humanity ought to dictate, is also correct medical practice; much too often, however, controlled by the ascetic principle on one side, and by that of what, for want of a better term, I may call tyranny on the other."

The bowels should be kept as near a state of nature as possible, avoiding purgation, except where there are evident indications of morbid secretions or biliary derangement. "In this moderate fever, also, if wine is not absolutely necessary, it is always convenient or useful." There is one state, however, where active practice is necessary. It is by no means unusual for this anomalous disorder of health to take on a decidedly intermittent form, and then bark or arsenic is necessary, and is generally exhibited at once—thus terminating in a few days, a series of teasing symptoms, or a condition of dubious convalescence, which might otherwise have dragged on for a long time. It is in this class of disorders that a sudden cure is often effected by change of habits, or rather change of air, especially where the original cause is malaria. Dr. M. asserts, and probably with some foundation in truth, that the routine practitioner not unfrequently hits on the real cure, without knowing what the disease is; and, while he is prescribing for mere debility, which is only a symptom or consequence of the disease, he gives the patient a certain number of bark draughts, for recruiting his strength, and

this medicine strikes at the root of the disease, which is a veritable though obscure *remittent*. After giving vent to a severe philippic against writers and practitioners who have failed to remark (though we beg to say that they have not all failed to remark) the frequent termination of common continued fevers by an intermittent form, Dr. M. observes as follows:

"And if this particular fact, or the subsidence of a continuous fever to health through the intervention of an intermittent type, occurs very commonly in even the minor fevers, be their duration but a few days, or even one or two, then will it become additionally probable that even these fevers, be their technical names, or their imaginary causes, what they may, are dependent on the same cause as that which produces intermittents, or that they are true marsh or remittent fevers; since this is one of the essential and remarkable characters of remittent fever in its most unquestioned form. And if this particular mode of termination or evanescence never occurs in the fevers of contagion, or in the true typhus, which, from all my reading and observation is the fact, then is it at least proved, or rendered highly probable, that these minor fevers do not belong to typhus, however physicians may still determine to persist in referring them to the several doubtful or imaginary causes formerly discussed."

The great remedy, in all cases of obstinacy or relapse, is change of air, of habits, and of scene. Hence travelling, which combines all these, is equal to them all. In respect to medicine, the catalogue is small. The bowels should be regulated by the mildest means—the diet should be nutritious and plain—and light bitter tonics should be exhibited to improve the digestive process. These means would do more than all the farrago of drugs in Apothecaries' Hall.

We are so convinced of the fact, that a great number of disorders of the general health result from bad air, that we have taken great pains to concentrate Dr. Macculloch's views, and diffuse them widely through the profession. They are calculated to do much good, though some of them may have been carried too far. In succeeding articles we will take up the other subjects treated of in these volumes, and hope to render them still more interesting than the present—which, however, forms an indispensable preliminary to the investigation of malarious diseases generally.

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From the Medico-Chirurgical Review.

#### ON FUNCTIONAL AFFECTIONS OF THE HEART AND ARTERIES. By M. LAENNEC.

In our last number we noticed the subject of neuralgia of the heart, and also angina pectoris. We shall now advert to some other nervous affections of this organ.

*Palpitation*.—Every beating of the heart which is sensible and unpleasant to the indi-

vidual, and, at the same time, more frequent than natural, is termed palpitation. The pulsation is often audible by the patient, and even by the by-stander. If a person lies in a horizontal posture, during palpitation of the heart, he will hear, in that ear which is next the pillow, a pulsation double in number to that at the wrist. This arises from his hearing the alternate contractions of the auricles and ventricles. In many cases, there is only an increase of frequency in the actions of the heart, while the patient imagines, from his sensations, that there is also great increase of force. Laennec has known this kind of palpitation go on for eight days, the pulse remaining extremely small and weak, and from 160 to 180 in the minute. In healthy persons, and from the excitement of moral or physical causes, there will be an increase both of force and frequency in the heart's action. As the sound and sphere of the heart's pulsations are much increased during palpitation, we should never draw any conclusions from auscultation at such times. We must wait till the heart is in its usual rate of going.

But to speak of purely nervous palpitation, unaccompanied by any organic change in the heart, it may be observed that this kind is often much more troublesome and distressing to the individual than that which is dangerous from its cause. Far from being removed by the most complete repose, it is, in general, most distressing during the early part of the night. It often prevents sleep—and it is often removed, or at least mitigated, by moderate exercise. There is no complaint which is more liable to lead medical men, especially young practitioners, astray, than palpitation of the heart. The following passage from Laennec is in perfect accordance with our own observations.

"The purely nervous palpitations consist in an increase of the impulse, sound, and particularly of the frequency of the heart's pulsations. A feeling of internal agitation, particularly in the head and abdomen, always accompanies them; also a limpid watery condition of the urine. The duration of palpitations of this kind is very variable: they may be momentarily excited by mental emotion; while, at other times, they seem to originate without any obvious cause, and continue for several years, especially in young persons who are at the same time both nervous and plethoric.—It is commonly imagined that such an habitual over-action of the heart as such palpitations imply, must at length give rise to hypertrophy of this organ. This is possible; but I must say that I have never seen any proof of the accuracy of this opinion. On the other hand, I am acquainted with individuals who have been habitually subject to affections of this kind, and who nevertheless exhibited no positive sign, either of hypertrophy or dilatation."

The physical signs (as revealed by examination with the ear) which distinguish nervous palpitation from that dependent on organic disease, are thus characterized by Laennec.

"In nervous palpitation, the first impression conveyed by the stethoscope is that the heart

is not enlarged. The sound, though clear, is not heard loudly over a great extent of chest; and the impulse, although appearing considerable at first, is really not great, as it never sensibly elevates the head of the observer. This last sign seems to me the most important and certain of any, when taken in conjunction with the frequency of the pulsations. These are always quicker than natural,—being, most frequently, from eighty-four to ninety-six in the minute. Nervous palpitations are rarely accompanied by any sign of determination of blood to the head or chest, except in old persons."

Palpitation of the heart is often increased by the very means which are taken to subdue it. Dr. Parry made it fashionable to attribute all nervous diseases to increased fulness or impetus of the blood-vessels, and therefore, the lancet was freely used in palpitations. There can be no doubt that there are many sedentary females who eat a great deal too much, and walk a great deal too little, and who, consequently, have a plenitude of the vascular system, and a preternatural mobility of nerve. These, when affected with palpitation, are benefited by blood-letting, abstinence, and even purgation. But, in most other subjects, and especially in hypochondriacal and hysterical individuals, the depletory practice increases the palpitation. The complaint is, in fact, extremely indomitable, and it is a great object to be able to say that it is nervous palpitation, and unaccompanied by danger. When a patient is thoroughly satisfied on this point, he or she will be less anxious about a cure, and more easily reconciled to the presence of an uncomfortable companion. There are, however, some means which occasionally relieve or remove this complaint. As this affection seldom exists, without some cause which can be traced, by accurate investigation, to errors in regimen, mental distress, or derangement of some corporeal function, so this inquiry should never be omitted. At all events, there can be no safer or more effectual plan of treatment than that which is based on temperance, regularity, and the improvement of any deranged function. Failing in these efforts, we may prescribe a steady system of exercise—the shower-bath—and certain *sedatives*, in which we have so great faith. The mobility of the nervous system is best reduced by that which gives natural tone and strength to the whole system. Plain food, regular exercise, and early hours, will do more than all the *assafoetida*, *bark*, and *valerian*, in Apothecaries' Hall.

*Irregularities of Action.*—These may exist without palpitation. Irregularity of this kind is often met with in elderly people, without any disturbance of the general health. Sometimes, amid a series of pulsations, very unequal among themselves, a single one will occur one half shorter than the rest. This produces something like an intermission—and it completely resembles the latter if the pulsation be very weak. "These irregularities, as to frequency, take place most usually in persons affected with dilatation of the heart." The

variations under consideration occur only in the heart, as heard by the ear or stethoscope—they make no sensible impression on the pulse, as felt at the wrist. We shall, therefore, pass on to more palpable irregularities of the heart's action.

*Intermissions of Pulsation.*—By this we understand, of course, a sudden and momentary suspension of the pulse, during which the artery is no longer perceptible beneath the finger. The duration of this intermission is very variable—being sometimes longer, sometimes shorter, than a common pulsation. There are two kinds of intermission—one *real*, consisting in an actual suspension of the heart's contraction—the other *false*, resulting from contraction so feeble as to be incapable of perception by the finger on the artery. Intermissions of the first kind are most common—they are frequent in old age, even when the health is good—or when the indisposition is only slight. "In middle age, they are *only* observed in certain diseased states of the heart, particularly hypertrophy of the ventricles, and during palpitation." In this, we cannot agree with Laennec. We have seen many instances, "in middle age," where temporary intermissions of the pulse were occasioned by flatulence, indigestion, acidity in the stomach—nay, even by emptiness of that organ,—that we rarely attribute any importance to this phenomenon, unless it be accompanied by other symptoms indicative of disease in the central organ of the circulation. Our author informs us that, by means of the stethoscope, we can clearly ascertain that "this species of intermission always succeeds the contraction of the auricles—it therefore differs only from the natural quiescence after this contraction in the irregularities of its recurrence." "If," says he, "in our examinations, we content ourselves with feeling the pulse, without applying the stethoscope, we shall, of necessity, confound this true intermission with the false one formerly mentioned produced by variations in the duration and force of the heart's pulsations."

"The last species of intermission is that which consists in the absence of one complete pulsation, recurring sometimes with an exact periodicity, after longer or shorter intervals, the pulse being in other respects regular. This pulse constitutes, according to Solano, the precursor of a critical diarrhoea. This peculiarity of the circulation is by no means rare; I have observed it frequently in some epidemics, but not at all in others, owing no doubt to the particular constitution that prevailed. This kind of intermission corresponds more frequently to a contraction of the ventricles, much weaker than the rest, than to a real interruption of their action; and, indeed, in such cases we often perceive an extremely feeble pulsation in place of a total intermission."

In a note appended to this passage, Dr. Forbes states as follows:

"In certain cases of diseased heart I have observed this species of intermission under a form which was sometimes productive of cu-

rious results. Every second pulsation was so feeble as to be altogether or almost entirely imperceptible. In the former case, the pulse appeared to be quite regular and slow; but, while in the act of feeling it, the intermediate or *latent* pulsation (if I may use the expression) became suddenly distinct, and the pulse was instantly *doubled*. In this manner I have known the same patient with a regular pulse at fifty or sixty, and a regular pulse at one hundred or one hundred and twenty, within the space of three minutes."—*Transl.*

Dr. Johnson was the first to point out this species of intermission several years ago, in the case of a gentleman residing in Bond-street, a patient under the care of Mr. Cosgreave of this metropolis. In this case, the ventricular actions were usually double those of the tangible arteries. But when any feverish excitement took place, the pulse became double the usual number or more, at the wrist, and corresponded exactly with the pulsations of the heart against the ribs. In by far the greater number of cases where we have observed intermissions of the pulse, there was an action—evidently a ventricular action of the heart, at the moment of the intermission at the wrist. Laennec has filled some pages with very obscure, not to say unintelligible reasoning respecting certain peculiarities of the pulse, without attempting any explanation of the cause of intermissions. One of the conclusions to which this great pathologist has come, is, in our opinion, a great error—namely, an independent pulsation in the arteries, without any impulse from the heart.

From an attentive observation of this phenomenon (intermission of the pulse) and where we have had very good opportunities of investigation, we have come to the conclusion that, in all cases, it depends on an unsuccessful action or contraction of the ventricle—not on an intermission of the ventricular contraction. The causes, however, of this abortive action of the ventricle are various. In very many cases, it is dependent on sympathetic associations of the heart with other organs, especially with the abdominal viscera; in which case, the intermission of the pulse is not constant, but only temporary. Where there is a permanent irregularity in the action of the heart or in the pulse, we believe there is generally some valvular disease, or alteration of structure. It may admit of much doubt, indeed, whether disease of the semilunar valves, or of the mitral valve, be most productive of intermissions of the pulse. For our own parts, we are inclined to impute permanent intermissions more to imperfections in the former than in the latter apparatus. It is curious that neither Laennec nor his translator has alluded (as far as we can see) to valvular disease, as the cause of intermissions and other irregularities (we always mean *permanent* irregularities) of the pulse. Yet we think these irregularities, when not merely temporary nervous affections, are, in three cases out of four, dependent on this cause.

*Spasm of the Heart—with Bellows-sound*

*and Purring-thrill.*—Although the sounds above mentioned frequently attend organic diseases of the heart, yet it is certain that they may exist in consequence of a purely nervous affection. But in these cases, "it is always attended by symptoms which constitute a real state of disease." The bellows-sound is most commonly heard in hypochondriacs—particularly in those of a sanguine and plethoric temperament—in which cases the sound is usually heard in some of the arteries at the same time—frequently passing from one to the other. It is sometimes continuous—sometimes intermittent:—in the latter case, it recurs on the slightest agitation of the body or mind. The symptoms which accompany it are the more severe in proportion as the sound is greater, more continuous, and extending to a greater number of arteries.

"When it is very constant and distinct, but confined to the heart, there is almost always more or less dyspnoea, with a feeling of greater or less debility, so that the patient can, in many cases, hardly walk. These symptoms are still more marked, if the purring-thrill accompanies the bellows-sound. There is commonly but slight nervous agitation, particularly when the patient is quiet; but on attempting to walk rather quick, or for any length of time, he is soon out of breath, and, in the severer cases, the head becomes confused."

When this affection is unconnected with any organic lesion of the heart, it must be treated as a nervous complaint.

*Neuralgia of the Arteries.*—Pains more or less acute, continued, or intermittent, sometimes follow the course of the arteries, and appear to have their seat in the nervous filaments supplied to these vessels by the ganglionic centre. They occur most frequently in hypochondriacs and in nervous females. A blister to the part thus affected, is considered by Laennec the most effectual application.

*Præternatural Pulsations of Arteries.*—These are regarded by Laennec as convincing proofs "that the arteries have an action of their own, independent of that of the heart." When any one will show us a pulsation in an artery, when there is no corresponding ventricular contraction in the heart, then, and not till then, will we believe that the arteries can pulsate independently of any impulse from the heart. Such a phenomenon we have never yet seen—and we verily believe that such a phenomenon has never been seen or felt by man. But Laennec says that one carotid will be found to beat more strongly than another—and this is another proof of the dependence in question. We do not think so. If the arteries have a *pulse* independent of the heart, that pulse must consist in the *distention* of the vessel—for its contraction surely could not be felt by the finger. And is Laennec or any other man prepared to say that the arteries have a power of *self-distention* sufficient to constitute a pulse, or throb, independent of the rush of blood sent from the heart? If one artery is found to beat more strongly than another, are we sure that there is nothing which impedes the flow of

blood into the latter, or out of the former? We apprehend that the inequalities of pulsation in arteries are more dependent on such circumstances than on any power which an artery may possess of more strongly dilating at one time or place than at another. Laennec instances the increased pulsation of an artery leading to an inflamed part, as to a whitlow. Is not this a case directly in point, and corroborating our position?

From the London Medical and Surgical Journal.

- a. *Observations on the Nature and Treatment of Erysipelas, illustrated by Cases.* By W. LAWRENCE, Esq., F.R.S. &c. Surgeon to St. Bartholomew's Hospital.\*
- b. *On the Treatment of Erysipelas, by numerous Punctures in the affected part.* By R. DOBSON, M.D., Surgeon to the Royal Hospital, Greenwich.\*
- c. *Case of Erysipelas, with some Remarks.* By A. COPLAND HUTCHISON, Esq., F.R.S. L. and E. Surgeon to the Lord High Admiral, &c.\*

There is, perhaps, no disease in the whole catalogue of human maladies deserving more attention than erysipelas. The frequency of its occurrence, and the discrepancy of opinion among medical practitioners respecting its nature and treatment, render it an important subject of inquiry. It is well known that the treatment of this disease by different practitioners has varied according to their views of its pathological characters, and that it has been conducted upon principles totally opposite to each other. This is not much to be wondered at, when we consider the various aspects presented by the affection in different subjects; that its characters vary according to the constitution of the patient; according to his local situation; according to the exciting cause, and the seat of the malady; and according to numerous other causes under whose influence the patient may be placed. This circumstance has led some to regard erysipelas as a disease depending upon a debilitated state of the system, requiring tonics and stimulants for its removal; whilst others have considered it as an affection differing in no material respects from phlegmonous inflammation. The latter view leads, of course, according to the modern pathology, to a plan of treatment consisting of blood-letting, and other antiphlogistic agents.

Now, looking impartially upon these opposite views, and taking into consideration the principal facts connected with erysipelas in its various aspects under the influence of different causes, we cannot help regarding it as a disease, if not quite unconnected with, at any rate not essentially dependent on, either a plethoric or a debilitated state of the system. Without travelling further in search of proof to support this opinion, the circumstance of the disease occurring in every grade of constitution, as regards plethora and debility, is amply sufficient. Did it require further proof,

the fact that the malady is curable, in some instances, by the antiphlogistic treatment, and in others, by the stimulating and tonic plan, might be adduced. Plethora and debility are terms frequently used with the view of explaining particular conditions of the system which render it susceptible to particular diseases, but these terms have never, so far as we are aware, been defined in a manner which would render their meaning unequivocal. Indeed, if the word plethora mean vascular fulness, and a preponderance of nutrition over interstitial absorption, we frequently find it combined with very great debility; whilst, on the other hand, a condition of the vascular system, opposite to fulness, and the absence of interstitial fat, are found compatible with perfect health and strength. Every practitioner must have witnessed instances where stout, and apparently strong and plethoric, patients could ill bear the loss of blood, or much purging; subjects habituated to fermented and spirituous liquors often present physical characters of this description; but it is well known that such constitutions, though plethoric in appearance, and even in fact, so far as regards vascular fulness, are characterized often by debility, and that their diseases, although generally attended by inflammatory symptoms, still require tonics and stimulants for their treatment. We find, on the contrary, that many persons, who, from outward appearances, might be supposed to possess but little strength, and no more blood in their vessels than barely sufficient to carry on the functions of life, nevertheless bear the antiphlogistic plan of treatment well, even to the abstraction of a considerable quantity of blood.

These are facts which ought not to be disregarded in laying down principles of pathology and of therapeutics; otherwise they will stare to shame every theory, however ingenious, which may have the indiscreetness to neglect them. It has always appeared to us, that the *quality* of the blood has much more to do than its *quantity* in predisposing the system to disease. We are well aware that this view is contrary to the pathology of the present day, which attributes every disease to some change in the mechanical condition of the solids. We shall offer a few further remarks on this subject as we proceed.

Mr. Lawrence regards erysipelas as an affection essentially inflammatory, and he considers the notion, that the local seat of the disease, the constitution, or both, are in a state of debility, to be "completely erroneous, and the treatment founded on it, not only inappropriate, but injurious." By erysipelas, he understands "inflammation of the skin, either alone, or in conjunction with that of the subjacent adipose and cellular tissue." When the surface of the skin is alone attacked, without any sensible swelling or vesication, the affection is called *Erythema*. "*Simple erysipelas* is a more violent cutaneous inflammation, attended with effusion into the cellular substance, and, generally, with vesication.

\* Medico-Chirurgical Transactions, Vol. xiv.

*Phlegmonous erysipelas* is the highest degree of the affection, involving the cellular and adipose membrane as well as the skin, and causing suppuration and mortification of the former." So far as the local disease is concerned, we are not aware that its inflammatory nature has ever been doubted. In fact, the seat of the affection presents all the characters of inflammation, namely, pain, redness, increased heat, and swelling. The question respecting which pathologists have differed, and do still differ, in opinion, is, not whether there be inflammation in a part affected with erysipelas, but whether that inflammation be as local in its character as inflammation purely phlegmonous, and whether the treatment should be exactly the same as in phlegmon? If the inflammation be the same in both, why applying different terms to it? Why, sometimes, calling it phlegmon, and at other times erysipelas? In fact, the term inflammation conveys but a very imperfect idea of the nature of disease, and our constitutional treatment of some inflammations would be worse than useless if we allowed ourselves to be guided by the local appearances alone. A prick of the finger is followed by inflammation; the same effect follows the insertion of a virus under the cuticle; it follows the application of heat; the application of cold will also produce the same effect, if the part be afterwards exposed to a moderate temperature, a temperature naturally congenial to the feelings; inflammation takes place in different parts of the body spontaneously, or from some internal causes, of whose mode of acting we are ignorant. All these causes are capable of producing diseases presenting the same local characters; at any rate, presenting the characters of inflammation. But let us examine the next grade in the order of effects: the inflammation caused by a prick of the finger will generally end, in a few days, in resolution, or, if not, it will form an abscess, containing pus: that caused by virus will be succeeded by a diversity of effects, according to the nature of that virus; for instance, the vaccine virus will form a colourless vesicle; the syphilitic virus will produce a progressive destruction of the part to which it is applied; that of small-pox is followed by effects different from those of either of the former, although the first effect of the application of each to an absorbing surface is inflammation. Inflammation caused by the application of heat is succeeded by a detachment of the cuticle from the subjacent skin; whereas, that produced by cold runs suddenly on to mortification, seldom terminating in the formation of an abscess. With regard to the treatment found best adapted to these different affections, it varies nearly as much as the appearances themselves. In fact, if we were to regard the inflammation as the sole disease, the same treatment ought to be found to answer in every inflammation. Experience, however, proves that this will not succeed. We have already advanced the opinion that bleeding would, probably, be beneficial in

the majority of diseases, especially when the system at large is affected, if it could be performed without detriment to the functions of the organs generally. Upon the same principle, it would modify every variety of inflammation; but it will also modify diseases unattended by any inflammation, such as those allied with the nervous system. To suppose, however, that general bleeding will suffice to cure every variety of inflammation, or that it can be always resorted to in inflammatory diseases without increasing the derangement of the general functions, or even the functions of the seat of the inflammation, would be supposing a thing contrary to the testimony of experience. We can only regard inflammation as a system of a variety of diseases, differing materially in all their other characters, and requiring various modifications of treatment. Small-pox, measles, scarlatina, psora, and many other affections, produce inflammation of the skin; but the other characters of these maladies differ as much from those of each other, as they do from the characters of erysipelas, or of phlegmon. Indeed, we find precisely analogous differences between the characters of phlegmon and those of erysipelas, whether we look at the constitutional or at the local symptoms.

We have already stated that Mr. Lawrence divides erysipelas into *simple* and *phlegmonous*. "In simple erysipelas, the skin is preternaturally red and shining, having a light rosy tint in the early stage and slighter cases of the affection, while in other instances it is of a bright scarlet, or even of a deep and livid red." In this slight form of the disease there is hardly any perceptible swelling, and no tension. The affection, however, is seldom confined to the skin alone, for effusion soon takes place into the cellular texture, giving rise to a soft swelling. The inflamed part is hot, painful, and imparts a sort of smarting or stinging sensation. The pain is not so intense as in phlegmon, nor has it the same throbbing character. Serous effusion takes place from the surface of the cutis, elevating the cuticle into vesicles or blisters, or "raising it by a soft, yellow, jelly-like deposit, which remains slightly adherent to both cutis and cuticle, and exactly resembles the effect often produced by the common blistering plaster." The inflammation, so long as it is confined to the skin, does not produce suppuration. It may do so, however, when it becomes very severe at one particular point, "and we thus occasionally see the formation of abscess under the skin, towards the decline, or after the disappearance of the erysipelatous redness."

Mr. Lawrence further remarks, that "the local symptoms above described are preceded and accompanied by fever, which varies in its character according to the constitution, age, and general state of health." This fever has an inflammatory character in the young, strong, and those of full habit, and blood taken from a vein exhibits the inflammatory crust on its surface. The fever, in other instances, is of the typhoid type, particularly when ery-

sipelas attacks the head. The author thinks that the pain felt in the epigastric region, foul tongue, with bad taste in the mouth, nausea and constipation, indicate "disordered stomach and intestinal canal, to which, as its *cause*, the local affection must be referred." We grant that these symptoms are indicative of disorder of the stomach and intestines, but Mr. Lawrence should have given us some proof that the local affection is referrible to this disorder, as its *cause*. The head suffers pain as well as the stomach; what proof is there, then, that the local malady does not depend as much upon disorder of the brain as upon that of the stomach and intestines? Again, the heart is excessively irritable: if the disease of one seat must be referred to that of another, why is not the inflammation of erysipelas not as referrible to a disorder of the heart, as to gastric derangement? Headach and increased irritability of the heart both precede and accompany the local inflammation, as much as nausea, foul tongue, and constipation do. It appears to us, that there is no more reason to attribute the local disease to disorder of the stomach and intestines, than for attributing the disorder of these organs to the local disease. The stomach and intestines, as well as the brain and heart, bear a part in the general disorder, and every organ manifests a derangement of its function according to the nature of its office. The stomach proves itself deranged by nausea; the intestines, by constipation; the brain, by headach and delirium; the heart, by increased irritability and quickness of motion, with a reduction of its absolute strength; the nervous and muscular systems, by lassitude and a general feeling of weakness. In fine, every organ, and even every tissue, as far as its functions are cognizable by the senses, or can be inferred from external signs, exhibits derangement; and if the local affection were referrible, or were attempted to be referred, to a disordered state of any other seat than that alone in which it resides, we should find it rather a difficult matter to point out any one organ which could lay a greater claim than the rest to that local disease. There is often not even a priority of affection to be discovered on the part of the stomach and intestines; for headach, quickness and irritability of the pulse, as well as many other symptoms of disorder, present themselves to our notice quite as early as gastric derangement. But a priority of affection, could it even be proved, would constitute no proof of dependence of the local malady upon gastric disorder. Erysipelas often follows as an affect of injury: what reason is there here to refer it to disorder of the stomach? or to any one organ more than another?

The inflammation in phlegmonous erysipelas is more deeply seated, and in a higher degree, than in the simple form of the disease. It occupies the whole thickness of the skin and the subjacent adipose and cellular tissue, and it soon runs on in the latter tissue to supuration and sloughing. The general fever

also is more violent than in simple erysipelas. The nervous system is often considerably affected, and the symptoms occasionally assume the character of those indicative of the worst form of typhus fever. The local inflammation is of a dark red colour, often of a brownish or livid tint; effusion of serum takes place into the cellular membrane, producing considerable tumefaction. This yields under the pressure of the finger, and occasionally retains the mark of the impression for some time. At first the cellular texture contains a whey-like or whitish serum. The fluid gradually becomes yellow and purulent, and we often find it presenting all the characters of good pus, and very thick. The matter is sometimes deposited in small separate collections, forming little abscesses; but it oftener fills a large portion of the cellular membrane, without having any distinct boundary. This tissue frequently sloughs to a considerable extent. The skin also, thus losing its supply of blood, dies, sometimes all round a limb, and to a very great extent. When this is the case, the constitutional disturbance is, of course, very great, and the patient generally sinks under it.

Mr. Lawrence next speaks of the seat and nature of erysipelas. With respect to the seat of the disease, he considers it to be the skin and cellular tissue, and not the aponeurosis of muscles; as Mr. C. Hutchison thinks, or the subcutaneous tissue and fascia, as Mr. Earl believes. In this opinion we fully agree with Mr. Lawrence, that the skin and cellular membrane are the tissues which are primarily affected; but we have, nevertheless, reason to believe that the fascia often becomes involved in the disease in phlegmonous erysipelas. We much doubt that the aponeuroses of muscles are ever primarily or principally affected. These are of a texture similar to that of a tendon, and we have frequently noticed tendons, perfectly unaffected, exposed in, or traversing, chasms produced by the sloughing of the cellular tissue, and appearing as white and clean as if they had been dissected for demonstration. So far, then, we agree with Mr. Lawrence, that "erysipelas is merely a particular modification of cutaneous or cutaneous and cellular inflammation."

But Mr. Lawrence's attempt at comparison between erysipelas and phlegmon appears to us quite contradictory. In one paragraph we are told, that "the difference between erysipelas and phlegmon is not merely in the original seat or degree of the disturbance; there is also a difference in *kind*." Of this difference we entertain no doubt. But, in the very next paragraph, the author says that he "can, however, by no means agree with those who regard it (erysipelas) as a distinct *species* of inflammation, and as capable, in that character, of affecting various parts of the body as well as the skin." Surely, if erysipelas differ in *kind* from phlegmon, and from every other inflammation, it must likewise differ in *species* from every other. As we are by no means fond of hypercriticism, we shall not

quibble about the meaning of a word or two. When Mr. Lawrence says that erysipelas is not to be regarded as a distinct species of inflammation, he means that the term erysipelas, ought not to be applied to "certain inflammations of the conjunctiva, mouth, and fauces; of the respiratory and alimentary mucous surfaces; of the serous membranes in the head, chest and abdomen, and of the brain, abdominal and thoracic viscera." He says that the proof of the identity of these various inflammations would consist in showing, that the same peculiarities which distinguish erysipelas from other inflammations of the skin are found in certain inflammations of the parts just enumerated.

Now, we consider Mr. Lawrence's view in this respect erroneous, and it is probable, if he were to reconsider the subject, he would discover this error. He remarks, that "since the distinguishing characters of erysipelas are clearly referrible to the peculiarities of the cutaneous and cellular structures in which it occurs, we could not expect to meet with the same affection in parts so differently organized as serous membranes and the viscera." We may, in the first place, show, that the mucous and serous membranes do *not* consist of tissues so very differently organized from the skin and cellular membrane as to lead us to suppose, *a priori*, that they could not be subject to the same diseases. Mr. Lawrence himself admits that both the skin and cellular membranes are subject to erysipelatous inflammation. Now, there is a much greater difference between the organization of the skin and that of the cellular tissue, than between the skin and the mucous membranes. The latter are very nearly allied, and are, in fact, continuous with one another. With the exception of the cuticle, the membrane lining the mouth and fauces is pretty nearly the same, in organization, as that covering the outside of the lip and face. The former may, truly, be of rather a more delicate texture than the latter; but we find that the skin covering the inside of the thigh is much more delicate, and thinner, than that which coats the outside of the same limb. Again, let us compare the cellular, with the serous, membranes. The former of the two has the property of upholding two characters, and one of these identifies it with the *serous* membranes. One surface of the membrane is serous, and the other cellular, destined for the secretion of fat. The physiological characters, then, of this tissue are not so very different from those of the serous membranes generally as might be at first supposed. Again, some of the pathological characters of the two membranes are very nearly allied. We find dropsy of the cellular membrane accompanied with dropsy of the peritoneum, pleura, pericardium, and the serous membranes of the brain. These facts, therefore, render it highly probable that the difference in organization between the serous and cellular tissues, is not so great as to lead us to suppose the former not to be subject to erysipelatous inflammation.

Moreover, admitting, as Mr. Lawrence maintains, that the skin and cellular membrane are the only tissues subject to erysipelas, as the latter pervades almost every part of the system, interweaving the other tissues, and as it is similarly modified in every seat, Mr. Lawrence should show us some reason why it should not be as subject to erysipelas in one seat as in another. If that portion of it which unites the skin to the subjacent fascia be a principal seat of the disease, why should not other portions of the same membrane, intervening other tissues, such as the coats of the intestines, the pleura and lungs, the coats of the œsophagus, &c., be also liable to be attacked, if erysipelas be guided in its choice of seat, as we believe it is, in some measure, by the modification of the organization? He should also point out what these peculiarities are, "which distinguish erysipelas from other inflammations of the skin," and which are not to be found in certain inflammations of other parts. The only peculiarity we know of in this respect is vesication. But let us ask, how can vesication take place in parts which have no cuticle to form vesicles? In other respects we can perceive no material difference between the characters of cutaneous erysipelas, and, what we should call, erysipelas of internal parts. Erysipelas of the face is often accompanied by inflammation of the fauces, which the author seems to admit; but he, at the same time, states, that this inflammation of the fauces has "only one character in common with erysipelas, namely, redness. The swelling and vesications of erysipelas are not found in these inflammations, which, on the other hand, are frequently attended with *ulceration*, with the formation of an ash-coloured or tawny substance adhering to the surface, and with superficial sloughing."

Now, it is evident that no vesications can take place in the fauces, because the skin there has no cuticle; and, this being the case, is not *ulceration* the very effect that we ought to expect to occur in a part organized as this is, as a substitute for vesication? The sloughing, also, which occasionally takes place in the fauces, is perfectly in character with erysipelas. We are told that no *swelling* attends these internal inflammations. Now, Mr. Lawrence himself tells us that, in simple erysipelas, "if the skin alone be affected, *there is hardly any perceptible swelling*, and no tension." We could not, therefore, expect to find swelling in simple erysipelas of the fauces, or of any other superficial membrane. The swelling and tension are produced by effusion of fluid underneath the covering membrane, in the cellular tissue; so that the swelling cannot be very great in parts where there is a scantiness of the latter membrane, and especially if the inflammation be superficial. Upon the whole, then, we consider that Mr. Lawrence has completely failed to establish his point—that erysipelas is confined to the integuments and the subjacent cellular tissue alone. In fine, we may adduce the fact, that the mucous membrane of the

nostrils often becomes involved in the disease in erysipelas of the face. We perfectly agree with the author, that the term erysipelas ought not to be confined to inflammation of the skin alone, to the exclusion of that of the cellular membrane, as has been proposed by Mr. Earle and Mr. Arnott. There can be no difference here in the essential nature of the disease, inasmuch as both tissues are simultaneously involved in the affection. But when Mr. Lawrence endeavours to identify erysipelas with phlegmon, he appears to us to fail to establish his point. Of the essential nature of disease we know no more than what may be inferred from its phenomena, and from the causes which appear to give rise to it. But from every fact connected with phlegmon and erysipelas, we have ample reason to consider them essentially different. It is true that the inflammation caused by external injury may assume the characters of either phlegmon or erysipelas, according to the constitution of the patient, or to certain external causes, under whose influence he may be circumstanced; but we never find inflammation of a phlegmonous character propagating itself by contagion, or prevailing epidemically. We need only refer to the first number of this Journal, wherein a review is given of Dr. Gibson's essay on the epidemic erysipelas which prevailed at Montrose, in 1822, for proof of the contagious nature of this malady, and of the identity of erysipelas with certain inflammations of internal parts.

Mr. Lawrence says that he is quite at a loss to discover in erysipelas those marks of debility which some have so much insisted on. We said before, that we are by no means fond of hypercriticism when the subject of inquiry is of trifling import; but, as erysipelas is a disease deserving minute attention, we must be allowed to lay more stress upon, and be more particular about, certain terms than some might consider necessary in the analysis of a work of this description. Having thus apologized, we may be permitted to ask for a definition of the word *weakness*, according to the sense in which the author means here to apply it. We are not aware that any disease depends essentially upon *strength*, according to the common acceptation of the word. Strength and weakness are merely relative terms, and have, perhaps, less to do with the production of disease than is generally supposed. The strength of a man of thirty, comparatively very weak for his age, would be considered very great if possessed by a child of eight or nine years old. Again, a person may possess great muscular power with a weak heart, or, *vice versa*, he may possess a strong heart with little nervous energy. If a person be predisposed to disease, he is necessarily weaker than he would be, *ceteris paribus*, if he were not so predisposed; therefore, every person, when attacked with erysipelas, or with any epidemic disease, or any casual malady not caused by violence or injury, may be said to be in a state of weakness at the time, because a certain degree of predisposi-

tion, most probably, always precedes the full development of such maladies. Again, vascular fulness and muscular weakness are perfectly compatible with each other, and we find them associated every day in the same constitution. But, if we suppose the term weakness to relate to the standard of muscular strength and nervous energy possessed by the majority of mankind at a given age, we shall find that erysipelas selects its victims more frequently from among persons who rank below that standard, than from amongst those who rise above it. We, however, by no means attribute the disease to the weakness itself, but to another cause, of which the weakness, as well as the malady, is only the effect. Weakness, according to the general acceptation of the word, is itself an indication of a state of constitution at variance with perfect health, although it may not always be accompanied by disease in its full development, or by disease characterized by sensible and unequivocal external signs.

Mr. Lawrence remarks, that "however weak the patient, the local disturbance is one of excitement; there is increased activity in the circulation of the part clearly marked by all the symptoms. Indeed, speaking of the part," he is, "unable to recognise debility as the cause of any inflammation whatever; and in reference to the seat of disease," he regards "the expressions of passive and asthenic inflammation, and venous congestion, as either unmeaning, or calculated to convey erroneous notions."

Now, from the above passage, we can pretty clearly understand what Mr. Lawrence's views of the pathology of inflammation are: they are by no means peculiar; but any one, who will take the trouble to examine the condition of a part in a state of inflammation, may satisfy himself that they are erroneous. Instead of there being "increased activity in the circulation of the part," if that part be examined with the microscope it will be seen that the activity is much less than when it is in a healthy state. The motion of the blood is much slower in it than natural, as might be inferred, *a priori*, from an acquaintance with the laws of hydraulics. The visible characters of inflammation depend upon a preternatural enlargement of the caliber of the arteries of the part affected, which enlargement permits the vessels to contain more than their proportionate share of blood; but owing to this disproportion in the size of their canals, the velocity of the fluid through them is necessarily less than if their caliber were natural. This is a fact, as we have already stated, of whose truth any one may satisfy himself by microscopical examination. Even supposing the arteries to be active agents in the circulation of the blood, we may ask, by what power, or mode of action, could a part acquire more than its due share of blood, according to the notion of there being increased activity in the circulation through it? The arteries have never yet been considered endowed with a power of attraction. If, then, as

Mr. Lawrence must necessarily mean by increased activity, this activity is applied to the propulsion of the blood, an inflamed part, instead of containing a superabundance of blood, as it invariably does, ought to be paler than natural, and to contain less than its due share, because the increased activity would necessarily drive the contents of the vessels out of that part in which it resided. The only way in which the visible phenomena of inflammation, according to this view of the function of the arteries, could take place, would be, by the vessels *leading towards* the inflamed part assuming an increased activity, whilst those situated actually *in the seat of inflammation* remain in a state of comparative *inactivity*. Supposing this to be the case, though contrary to the fact, it would by no means prove that there is an increased activity in the inflamed part: on the contrary, it would only prove an increase in the activity of the vessels of a *contiguous* part, and a comparative decrease in that of the arteries of the actual seat of the disease.

Moreover, Mr. Lawrence acknowledges his inability of recognising debility as the cause of any inflammation whatever. Now, if debility mean a want or the absence of a power natural to a living part, we cannot understand how any inflammation can take place without debility. The only living power connected with the arteries, so far as we can recognise, is *contractility*. The arteries of an inflamed seat lose this property, in a great measure, and allow themselves to be preternaturally dilated by the pressure of the blood. Were it not for debility of their coats, or a reduction in the amount of contractile power natural to them, their caliber could not enlarge, as it does in vessels undergoing the process of inflammation. To this cause is to be attributed the redness of the inflamed part, as well as the swelling at the commencement of the attack, before effusion takes place into the interstices of the tissues. The pain, and the preternatural evolution of heat, in the seat of affection, are attributable to the organic derangement which takes place in the coats of the vessels, and which acts as the cause of their loss of tone or contractility. These latter phenomena, namely, the heat and pain, always precede the redness and swelling, inasmuch as they rank a grade higher in the order of causation. It is not requisite at present to trace causation further back than this derangement, because to do so would require to grasp at the original cause of disease, and would involve the fundamental principles of physiology and pathology.

Mr. Lawrence again says that, in reference to the seat of disease, he regards the expressions of passive and asthenic inflammation, and venous congestion, as either unmeaning, or calculated to convey erroneous notions. With respect to the terms active and passive, sthenic and asthenic, neither one nor all of them can convey any clear idea of the condition of an inflamed part, inasmuch as some of the functions concerned are more active than

natural, whilst others are less active than in the healthy state. Evolution of caloric is augmented, whereas the contractility of the arteries is diminished. These are two properties essential to inflammation, upon the latter of which, the preternatural redness of colour depends. As for the other two, namely, pain and swelling, we can perceive no relation between them and the terms passive and active, or asthenic and sthenic. The nervous energy is exalted in the seat of disease, as far as regards sensibility; but it is diminished with respect to natural sensation, and also to the power of motion when the inflammation is seated in the muscular tissue. Secretion is generally increased in the part affected, but there is reason to infer that absorption is diminished. Hence, we not only agree with Mr. Lawrence, that the terms passive and asthenic, as applied to inflammation, are unmeaning; we go further, and say that the terms active and sthenic are equally unmeaning.

But, Mr. Lawrence draws "venous congestion" also within the sphere of these unmeaning terms. If by congestion be meant a preternatural collection of blood in the vessels of a particular part, can Mr. Lawrence, or any one else, deny that such a preternatural collection does take place in inflammation? What else is it that gives the part inflamed the unnaturally red colour which forms one of the most characteristic properties of every species of inflammation? But it is to *venous* congestion that he applies the epithet unmeaning. Now, we do not maintain that *venous* congestion is, by any means, essential to inflammation; but, that it is a frequent attendant on inflammation, any one may satisfy himself by examining the state of the cerebral veins when the meningeal membranes are inflamed; or let him examine the mesenteric veins in subjects who have died of peritoneal or intestinal inflammation.

We shall not offer any remarks on Mr. Lawrence's Nosological Arrangement of Erysipelas. With respect to the *causes* of the disease, he thinks that the occurrence of the malady in the face may be traced in some instances to contagion. Our opinion is, that its occurrence in other parts also may be traced to contagion, although various other causes may contribute to produce it. This has been the opinion of almost all the most eminent writers who have treated of this subject, and the history of the malady affords ample proof in support of the notion of its contagious properties. Mr. Lawrence says, that there is really no difference as to causes between erysipelas and other inflammations. We are fully aware that an injury inflicted on a part may be followed by either phlegmon or erysipelas, according to the constitution of the patient, or according to other circumstances under which he may be placed; but do we not find also that the same external causes will produce simple inflammatory fever in one individual, and typhus fever, of the most contagious nature, in another? Erysipelas may follow as an effect

of injury inflicted on a part, but the disease may become contagious from that time, owing to some peculiarity in the constitution of the patient. Several facts connected with the history of the disease prove this to be the case. Some diseases possess the property of propagating their kind, either by contact, or by the diffusion of a principle conveyed from one individual to another through the atmosphere. Now this principle must have some origin, either in the body or out of it. If a mechanical injury can create such a disturbance in a constitution peculiarly modified, as to give rise to the formation of this contagious principle in the system, the disease in that system, or in a local seat, must evidently assume the same characters as if the contagious effluvia, already generated, had been applied to the body; and the newly-formed principle may go on progressively from this time, and propagate its kind in constitutions which may be in any way susceptible to the poison, and which are brought within the sphere of its influence. The contagious principle, like other forms of existence, must depend upon the union of certain causes, each of which, separately, may be perfectly destitute of the property of generating disease. It differs little whether the principle of contagion be formed in the body, by the union of the causes necessary to its formation, or imparted, already formed, to the body, from another individual labouring under the disease.

We next come to the treatment of erysipelas. It is this part of Mr. Lawrence's essay which renders it highly interesting.

The treatment of simple erysipelas must be modified according to the constitution of the patient and the degree of the local inflammation. Mr. Lawrence recommends general blood-letting in the young and robust only, local abstraction of blood by cupping or leeching being usually sufficient in the generality of cases. In addition to local bleeding, the patient is to be placed upon the antiphlogistic regimen. In a word, the plan which the author recommends is purely antiphlogistic, the same as that pursued in inflammation in general. But he admits, at the same time, that the practitioner is occasionally obliged to have recourse to stimulants and tonics. He must be guided here, of course, chiefly by the character of the constitutional symptoms. Tonic remedies are by no means incompatible with local abstraction of blood. In the treatment of inflammation, there are two points to be particularly attended to: the first consists in allaying the irritability of the system and in endeavouring to subdue that constitutional disturbance which tends to wear out the energy of the nervous system; the second consists in subduing the local malady, before it shall have time to run on so far as to produce such a change in the organization of the part as totally to destroy its function, or even its vitality. Now, the important question is, how is the general disturbance to be best allayed? Without reverting at present to the *modus operandi* of bleeding, we find that in

inflammation, if the patient can afford to lose blood from a vein, bleeding tends more than any other remedy to subdue the constitutional derangement. But there are limits beyond which blood cannot be safely abstracted, because a certain quantity of this fluid is necessary to the support of all the tissues and to the performance of their several functions. We know that the mass of blood is being constantly reduced by the several secretions and excretions, and that it can be but very slowly renovated when so little nourishment is taken into the system as is commonly the case when the general functions are so much disturbed. If then we abstract so much blood as to leave barely sufficient behind to carry on the functions of life, the mass will be further reduced by the causes already mentioned, and the organs will cease to perform their several offices. We are fully convinced that we have witnessed several cases of death produced by these causes. It is often a great object to take away as much blood as the system can well bear to lose; but if this be done, the energy of the constitution must be, at the same time, kept up by nourishing diet, and sometimes even by stimulants and tonics, otherwise it will sink for want of support. Sometimes the quantity of blood in the system, at the commencement of the attack, is no more than just enough to support the functions of the different organs, and would soon diminish so much as to be insufficient to do so, if not aided by stimulants and tonics. This state of the system may be accompanied by violent local inflammation, threatening to destroy the organization of the seat of affection. He who would resort to general blood-letting under such a circumstance, would soon find his patient slip through his hands.

But with respect to local blood-letting: an inflamed part of considerable extent deprives the general vascular system of a great proportion of blood. The caliber of the vessels in the seat of inflammation is much enlarged, so that the part constantly retains considerably more than its due share of blood. The support which this blood would impart to the system at large, if the fluid were equally distributed, according to the *natural* capacity of the vessels of the different seats, is now completely lost to it, whilst it is at the same time injurious to the part in which it resides. By bleeding locally, from the inflamed surface, we empty the vessels of that blood which is injurious to them, owing to its preternatural quantity, and, perhaps, owing also to its morbid condition; and we, by this means, afford their coats an opportunity of recovering their contractile property. We do this without reducing in any material degree the mass of blood circulating in the general system. Hence, as we before stated, local blood-letting, from the seat of inflammation, is perfectly compatible with the administration of nourishing diet, stimulants, or tonics, when these are found necessary to support the energy of the constitution.

If the extension of the local inflammation

cannot be arrested by the application of leeches and cupping, we have still a very powerful means in our possession, namely, "making incisions through the inflamed skin and the subjacent adipose and cellular textures, which are the seat of the disease." It is in this that the peculiarity of the plan of treatment pursued by Mr. Lawrence mainly consists. "These incisions are followed very quickly, and sometimes almost instantaneously, by relief, and cessation of the pain and tension; and this alleviation of the local suffering is accompanied by a corresponding interruption of the inflammation, whether it be in the stage of effusion, or in the more advanced period of suppuration and sloughing. The redness of the skin is visibly diminished during the flow of blood from the incisions; in twenty-four hours it has generally disappeared, and the skin itself is found wrinkled from the diminution of the general inflammatory tension." These incisions usually put a stop to the further extension of the local inflammation, and Mr. Lawrence says it has never failed to do so within his experience "when the case has been a proper one for the practice, and the state of the patient has admitted of its being fairly tried." There might be some difference of opinion respecting whether or not a case be a proper one for the practice. But no quibble can be raised upon this point, as Mr. Lawrence has given numerous cases to show which are proper and which are not.

Mr. Lawrence wishes to be understood, however, that he does not advise incisions in erysipelas generally. He confines their employment to cases of the phlegmonous kind. Much will, of course, rest upon the judgment of the practitioner, whether incisions be requisite or not. It is important to decide quickly upon this point, and to make the incisions without delay when they are considered necessary. When the disease "attacks the face, it is not attended with that serious inflammation of the subcutaneous structures which requires incisions." The author thinks, however, that they may be advantageously resorted to in the eyelids, when the inflammation is severe. This practice, although alluded to by some writers, several years back, is indebted for its recent introduction to Mr. Copland Hutchison. There is this difference between the manner of making the incisions, as recommended by these two surgeons, viz. Mr. Hutchison recommends a number of incisions, proportionate to the extent of the inflammation, and about an inch or an inch and a half in length, through the skin and cellular tissue; whereas, Mr. Lawrence's plan consists in making *one* incision, extending from one boundary to the other, through the centre of the inflamed part. Dr. Dobson, again, recommends a great number of punctures to be made in the part affected, at a short distance from each other. These three modes of treatment appear to us to be founded upon precisely the same principle, namely, that of giving vent to

the preternatural quantity of blood which distends the vessels in the seat of inflammation; and they appear to have proved equally successful from the account given of them by their several advocates. Each author, of course, thinks his own plan the best. This is very natural, if it has proved successful in his hands. Admitting that the three plans are equally efficient, the next question is, which is the most expedient. Looking at them impartially, it is our opinion that we ought to be guided mainly in this point by the situation and extent of the inflammation. When the face forms the seat of the inflammation, it is evidently of great importance to avoid incisions, which must necessarily leave scars behind, if punctures will answer the purpose as well. But when the disease is situated in a part generally covered by the clothes, we should prefer incisions, as they will give a freer vent to the congested blood.

When incisions are resorted to, it appears to us that their number ought to be determined by the *extent* of the inflammation. When it is of small extent, one incision, of some inches long, carried through the middle of the part, will sufficiently empty the vessels and relieve the limb of its tension. But sometimes the limb is inflamed all round, and to a very great extent. A single incision, extending from one end of a limb to the other, would present a terrible gash, and we doubt that it would afford the vessels the same facility of emptying themselves of their contents as a number of smaller incisions made in different parts of the inflamed surface would. By making a number of small incisions, the larger branches of the vessels may be avoided; for the efficacy of the practice cannot, evidently, depend as much upon the actual quantity of blood that may follow from the cut, as upon its being thrown out by the smaller vessels, whose coats have lost their contractility. If a large vessel be divided, the general system will be reduced by the loss of blood, without affording much relief to those which are preternaturally loaded. Indeed, the hemorrhage may be so profuse as to prove fatal.

Upon the whole, it appears to us that we ought not to allow ourselves to be prejudiced in favour of one of these plans more than the others, if they are found equally successful, and if the principle be the same in each, but to adopt that which may appear most applicable to particular cases. Whilst the local disease is thus treated, the constitutional derangement must also be attended to. The most valuable remedy in our possession for subduing inflammatory diseases, is mercury. This Mr. Lawrence highly recommends. Indeed, this will not interfere with the administration of other remedies, either antiphlogistic or tonic, as the case may require.

Mr. Lawrence, in the essay before us, relates thirty-eight cases of erysipelas. These are highly interesting; but having explained to our readers, his views of the nature and treatment of the disease, we do not deem it necessary to insert any of the cases.

From the *Lancet*.

# ON THE CIRCULATION OF THE BLOOD IN THE FŒTUS,

In Quarto, with Ten Lithographic Tables.\*

This is a most interesting work; and we regret that, from want of space, we are obliged to give our readers only a very concise account of it. In the *first section*, a rapid view is taken of the different opinions on fœtal circulation; they may be reduced to the following three:—

1. The blood is conveyed by the venæ cavæ into the right auricle, passes through the foramen ovale into the left auricle, and from thence through the left ventricle into the aorta; the small portion which, from the right auricle, passes through the right ventricle and into the pulmonary artery is, by the duct. arteriosus, carried into the aorta. This is the opinion of Harvey, Fabricius ab Aquapendente, Morgagni, Merry, and Haller.

2. The blood of the vena cava superior is separated from that of the vena cava inferior, by means of the Eustachian valve, by which the blood of the cava inferior is directed through the foramen ovale into the left cavity, whence it passes into the aorta ascendens and its branches, the innominate, left carotid, and subclavian; the blood of the cava superior goes directly into the right ventricle, and from thence through the pulmonary artery and duct. arterios. into the aorta descendens. According to this opinion, which is that of Nichols, Sabatier, Bichat, and Bordeu, the head and upper extremities receive arterial blood, while the lower half of the body is supplied with venous blood. By this circumstance, Bichat endeavoured to account for the early development of the head, and superior extremities of the fœtus. As, however, the circulation must considerably differ according to the different conditions of the heart, and as this organ, during its formation, undergoes important changes in its form and mechanical arrangement, it appears that both opinions are defective and erroneous, because they overlook these metamorphoses.

Thus the different size of the cavities of the heart, in different periods, evidently contradicts the *opinion* of Sabatier, &c., that through the whole fœtal life all the blood of the cava inferior passes through the right auricle into that of the left side, and the blood of the cava superior goes directly into the right ventricle; for, *originally*, the left cavity of the heart is by far the largest of the two; at a *later period* they gradually become equal, and *shortly before birth* the right cavity surpasses that of the left side; whereas, the quantity of the blood of the cava inferior, exceeds, at *all periods*, that of the cava superior; so that, if the blood of the former did constantly go into the left cavity, this ought likewise constantly to surpass

the other in capacity. It appears, consequently, that the opinion of Sabatier, &c., corresponds *only to the earliest periods* of embryonic life. Another very important objection is, the simultaneous contraction of both auricles, and their alternate motion, with that of the ventricles, so that neither the blood of the cava superior can go in an uninterrupted stream through the auricle into the left ventricle, nor the blood of the cava inferior be carried from one auricle into the other. Lastly, it is very *improbable*, that the head and upper extremities should be supplied with arterial blood, and the lower half of the body with venous blood, and the slower development of the lower extremities can by no means sufficiently account for such a difference.

With regard to the opinion of Harvey and Haller, the right auricle, especially in the earlier periods, is of too small a size to admit of all the blood of both venæ cavæ; at the same time the simultaneous contraction of both auricles prevents the passage of the blood from the one into the other. The use of the valvula foramen ovale cannot be to prevent the reflux of the blood into the right auricle, as during the simultaneous contraction of both auricles they are acting against each other, and thus mechanically preclude every passage but that into the ventricles. It appears that the structure of the *valvula foraminis ovalis*, and of the *Eustachian valve*, was not sufficiently known to the physiologists, and that the use of these organs was completely misunderstood.

We proceed to the *third opinion* on fœtal circulation: the vena cava has two openings, the one into the right, the other into the left auricle; the latter is the so called foramen ovale. By means of this arrangement, the blood of the cava inferior goes separately into each auricle, and no passage of the blood from one auricle into the other takes place. Originally, and up to the third month, the cava inferior opens *only* into the left auricle, and even, for a considerable time afterwards, this opening is by far *the larger* of the two; so much so, that the foramen ovale may be justly regarded as the principal opening of the vein into the heart, and that its subsequent exclusion is, in fact, nothing but the gradual obliteration of the opening of the cava inferior into the left auricle. The right auricle, and the Eustachian valve, must be considered as a continuation of the right side of the cava inferior; and the left auricle, with the valvula foramen ovale, as a continuation of its left side.

It is the principal object of our author to confirm and develop this opinion, which was originally established by C. F. Wolff, in the *Acta Acad. Petropolit*, 1777. It regards only the passage of the blood through the heart; as to its way through the aorta ascendens and descendens, Mr. Kilian adheres to the opinion of Sabatier, &c.

The *second section* contains a comparative view of Haller, Malpighi, Spallanzani, Pander,

\* H. Fr. Kilian, Ueber den Kreislauf des Blutes, &c.

and Wolff's investigations on the formation of the heart in the incubated egg. The observations of the author on the same process in the human fœtus, refer particularly to the original condition of this organ as a simple cavity, and of the aorta and pulmonary artery as one single canal, and the subsequent metamorphoses of these organs into complicated cavities and several vessels. According to this view, the course of organic formation strikingly corresponds with the peculiar type observable in the general gradation of organizations; a view which, by a comparison of the heart and the principal vessels in fishes, the different genera of amphibia, and in birds, with that of the human fœtus in its different periods, is found correct to a remarkable extent.

We cannot omit giving our readers a brief extract of the author's anatomical researches on the condition of the fœtal heart at various times, as upon them his peculiar opinions on fœtal circulation are founded.

1. *Insertion of the Vena Cava Inferior into the Heart.*—Originally both auricles form a simple cavity, which, in fact, is nothing but a dilatation of the vena cava inferior; a duplicature of this vein begins gradually to be formed, and thus the simple cavity is divided in two. This duplicature represents in the right auricle the Eustachian, and in the left the valve of the foramen ovale. By the latter valve, the opening of the vein into the left auricle is gradually obliterated, while the opening into the right auricle enlarges in the same proportion.

2. *The Foramen Ovale* is, consequently, not to be considered as an aperture in the septum auriculorum, but as a prolongation of the venous coats; it gradually turns from the right to the left, in the same proportion as the right opening of the vein enlarges, and the left one contracts.

3. *The Pulmonary Artery, Ductus Arteriosus, and Aorta Descendens*, are to be considered as the abdominal aorta; whereas, that arising from the left ventricle exhibits the aorta cerebialis, so that in the fœtus there exists *two aortas*. This the author clearly proves by anatomical observations; it also bears a great analogy to the distribution of these vessels, as observed in the progression of red-blooded animals.

4. *The Pulmonary Arteries.*—Several physiologists have maintained that, previous to birth, no blood at all is carried through the lungs. Bichat first demonstrated the incorrectness of this assertion, and proved, that in the same proportion as the fœtus approaches to the period of birth, the pulmonary arteries enlarge, and that, considerable time before respiration commences, the blood passes through the lungs. This our author justly tends to confirm, although he seems to overrate the quantity of this blood; the size of the pulmonary arteries is, indeed, considerable; but we cannot from it exclusively infer the quantum of blood which they convey, as this, besides, depends on the velocity of its movement.

### 5. *The Umbilical Vein and Ductus Venosus.*

—With regard to the latter, the author differs from most writers; according to him, it is not a continuation of the umbilical vein, but belongs properly to the vena portæ; the umbilical vein goes into the left branch of the latter, which is commonly called sinus venæ portæ. The vena portæ is ingeniously compared to the pulmonary artery; both convey, originally, no blood to their respective organs; the same relation exists further between the abdominal aorta and the pulmonary artery, as between the vena portæ and the vena cava inferior, and the function of the ductus arteriosus corresponds exactly with that of the ductus venosus.

We are now fully enabled to follow our author in his description of fœtal circulation. In the liver the blood of the umbilical vein is divided into two unequal parts; the larger portion is carried through the substance of the liver, the smaller portion passes through the ductus venosus into the cava inferior, where it is mixed with the blood, which returns from the lower extremities, &c. In the heart the blood of the cava inferior is again divided, one part goes into the left, the other into the right auricle. The contents of the left auricle consist of a large portion of blood from the left opening of the cava inferior, and of a much smaller quantity from the pulmonary veins. The right auricle contains a small quantity of blood from the right opening of the cava inferior, and the blood of the cava superior. In this manner the blood of both auricles is of a similar mixture.

By the systole of the auricles the blood is carried into the ventricles; on their contraction, the blood of the left cavity is conveyed into the aorta ascendens, and its three branches; that of the right ventricle passes through the duct. arter. into the aorta descendens. Both aortæ have no communication between themselves, and the arterial system of the head, and the circulation of the upper extremities, is completely separate from that of the lower half of the body.

The most important difference in the fœtal circulation, from that in the adult, is the *insertion of the cava inferior in both auricles, and the double aorta*.

In the fœtus the left opening of the cava inferior performs the same function as the pulmonary veins in the adult; both are, consequently in the fœtus in an opposite proportion, so that previous to the existence of any pulmonary vein, the cava inferior goes entirely into the left auricle, and in the same degree as the pulmonary veins enlarge, the left cava superior diminishes in diameter, and, lastly, disappears entirely.

On the other side, the umbilical artery represents, in the fœtus, the pulmonary artery of the adult, and there exists the same relation between them, as between the left cava inferior and the pulmonary veins; for while the left cava inferior decreases, the right one proportionally enlarges, and so does the quantity of blood conveyed through the right ven-

tricle into the aorta ascendens; from that period the lower extremities begin to be formed, while up to this time the development of the head and the upper extremities prevailed. All these metamorphoses are ac-

companied by a decrease in the functions of the placenta; and it seems that in the latter period of fetal life, the liver is in some degree substituted for it, as appears from the beginning secretion of the bile.

## Medical and Philosophical Intelligence.

*Aneurism of the middle Artery of the Dura Mater.* By DR. KRIMER.—The subject of the case had a tumour about the size of a walnut, not painful upon pressure, hard, moveable, and covered by the sound integuments, situated upon the left temple. It arose from a blow upon that part, received about two years before, had slowly attained its present size, and during its progress had been attended with headach. It was supposed to be an encysted tumour, and an operation was proposed, and assented to by the patient. After cutting through the skin, the operator found that the tumour was situated beneath the temporal muscle, which was divided, carefully avoiding the external temporal artery. The tumour was found attached to the bone by a peduncle about the size of a writing quill; this was divided, and a profuse discharge of arterial blood immediately followed. Alarmed by the hemorrhage, and supposing that he had injured the deep temporal artery, the operator arrested the blood by means of a plug, and attempted to discover the injured vessel; no artery, however, was found within the circumference of an inch of the spot where the blood issued; beyond this distance the deep temporal was found perfectly safe, and upon careful examination, it was discovered that the blood issued from an opening in the bone itself. Not more than a pound and a half of blood was lost, but, notwithstanding all the means employed for his relief, the patient fell into a state of unconsciousness, and died in about two hours afterwards.

Upon examining the sac which formed the tumour, Dr. Krimer ascertained it to be an aneurismal pouch, the opening of which directly corresponded with the situation of the middle artery of the dura mater; the tumour had made its appearance between the squamous portion of the temporal, and the adjoining part of the parietal bones, and from the apparent solidity of its parietes, which were formed by a layer of cellular membrane, by the thickened pericranium, and lined internally by coagulable lymph, could hardly have escaped being taken for an encysted tumour. Permission to open the body being refused, Dr. Krimer broke with a pair of pincers, the inferior and posterior angle of the frontal bone, and a small portion of the temporal, in order to examine, as far as possible, the condition of the internal parts; the bone was found reduced to the third of a line in thick-

ness, and was quite flexible around the opening; for the distance of about three-fourths of an inch, the middle artery of the dura mater was so much dilated as to equal the finger in size, and must have exerted constant pressure on the brain; the pia mater, covered at this spot by an exudation of coagulable lymph, was strongly adherent to the dura mater. The brain itself appeared sound; about an ounce and a half of blood was found in the interior of the cranium, and the extravasation appeared to extend even to the base of this cavity; to the compression thereby induced, Dr. Krimer attributes the death of the patient.—*Jour. des Progres, &c. from Graëfe and Walther's Journal.*

*Croton Oil.*—It is known to most practitioners in this country that the purgative we possess in the Croton Oil is, in many cases, a powerful but dangerous remedy—to few that it may in any case be administered as a safe and gentle one. The object of this communication is to point out the form in which it can be best administered, to explain its operation, and the cases and constitutions in which it can be safely applied.

As far as my experience with it has gone, I have been decidedly led to prefer its exhibition in the form of pill, combined with compound extract of colocynth or extract of rhubarb, and a little oil of cinnamon. This form is much less objectionable than a fluid preparation, the acidity of which causes considerable uneasiness in the fauces, œsophagus, and stomach. The oil of cinnamon covers its disagreeable and nauseous smell. It may in some cases be employed advantageously mixed with gruel, and used as an enema. The quantity contained in the pill should vary from half a drop to two drops, and no more. The enema might, for an adult, contain two drops.

In about an hour or two after its passage into the stomach the patient usually complains of pain in that organ, accompanied with languor and lassitude, and soon afterwards nausea, retching, and vomiting. The vomiting is not an invariable effect. The pain extends to the abdomen, and becomes true griping, and the bowels are evacuated freely and copiously. Its operation is completed most commonly in three hours from the time of taking it: it appears to have the power of

completely clearing the alimentary canal of all that it had previously contained.

The cases and constitutions to which it is most adapted are those of obstinate constipation, without mechanical obstruction, in robust habits. It must have occurred to all in the practice of medicine to have met with constipation where our ordinary purgatives had no power whatever: in such cases the Croton is invaluable. It may be given to women in the constipation of pregnancy; but the practitioner should be aware that, in such cases, prudence should point out the greatest caution.\*—*Lond. Med. Gaz.*

#### *Effects of Galvanism upon the Nerves.*—

Among the numerous experiments which have been instituted, to prove the analogy of galvanism and nervous influence, those of Professor Weinhold are not the least interesting or curious—the following are some of the most remarkable.

He decapitated a cat, and after the pulse and muscular action had entirely ceased, removed the spinal marrow, and filled the vertebral canal with an amalgam of mercury, zinc and silver. Immediately the pulsation of the arteries returned, and muscular contractions were excited, which could not be distinguished from those produced through the influence of the spinal marrow; the animal began to jump, and did not cease till it had made several leaps. When the irritability appeared to be exhausted, Professor Weinhold, by means of a metallic arc, established a communication between the heart, voluntary muscles, and the artificial medullary substance, and again excited general contractions, which, however, were weaker than the first.

The cranium and vertebral canal of another cat which no longer evinced signs of life, was filled with the same amalgam, and for the space of about twenty minutes, such a state of vital tension (tension vitale) was induced, that the animal raised its head, opened its eyes, looked fixedly, attempted to walk, endeavoured to

raise itself after having fallen several times, and finally fell to rise no more. The circulation and pulse were very active during all this time, and continued a quarter of an hour after the thorax and abdomen had been opened. The secretion of the gastric juice was evidently greater than ordinary; the animal heat was perfectly re-established.

The Professor filled also the cranium of a dog with the above mentioned amalgam, and afterwards examined the principal functions of the senses; the pupil still preserved its power of contraction, the animal evinced a desire of avoiding the light when a candle was brought near him, and appeared to listen when the table was struck with a key.

Weinhold has also observed that sparks were evolved from the two extremities of a nerve divided transversely, when the divided ends were approximated to each other. He cut across the crural nerve of a cat, placed the extremities at the distance of a line, and made a communication by means of a metallic arc; at the moment the circle was complete, he saw at each extremity of the nerve a luminous point, which, however, did not pass from one to the other.

The hypothesis of a nervous atmosphere has been completely overturned by the experiment of Weinhold, in which, after having divided the crural nerve, he could not excite contractions of the muscles of the leg by means of galvanism, although the extremities of the nerve were placed at the distance of a line, and even of a fourth of a line. A ligature even, placed upon a nerve, prevents the transmission of galvanism. He observes, moreover, that the nervous pulp is the sole conductor of the galvanic fluid, while the coat is altogether destitute of that faculty.

Weinhold has also investigated the material changes which take place in the nervous system during the action of galvanism. Having isolated the crural nerve of a frog, he observed that the medulla of the nerve, which was almost transparent, shrunk during the contraction of the muscles excited by the galvanic irritation, and that this shrinking alternated with the dilatation. He exposed the tracheal nerve of a rabbit, and observed that after having excited twenty or thirty rapid contractions of the extremities by means of the galvanic pile, the nerve diminished in volume, lost its cylindrical form, and ultimately became a simple, white, and compressed tube. This loss of substance of the nervous medulla during the action of the nerves, was, in the space of twenty or twenty-five minutes repaired by the augmentation of the pulsations of the heart coinciding with the violent contractions of the muscles, so that after a time, the nerve had recovered its cylindrical form. When, on the contrary, the heart had been removed, and the reparation of the nervous substance could not be effected through the medium of the circulation, the nerve did not recover its primitive form. The same loss of substance was also observed in the portion of the spinal marrow which gives rise to the

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\* The tree yielding this oil has been recently discovered in Peru, by our countryman, Dr. Burroughs, and he has transmitted to the United States, a specimen of the oil obtained by cold expression, which he thinks preferable to that brought from the East Indies, inasmuch as the latter is always obtained by the aid of heat, and is frequently adulterated. He states, that he has given it in doses of from two to three drops, in the fever of the coast, and in this quantity has always found it to prove a certain, prompt, and safe purgative: he has never seen it productive of unpleasant consequences—in a full dose it sometimes occasions a slight sensation of heat about the fauces and at the pit of the stomach, which, however, goes off very soon after the bowels have been moved. A number of the beans accompanied the specimen.—*Ed. Jour. of For. Med.*

nerves of the thoracic extremities, when the muscles of these extremities were thrown into contraction by the violent and continued action of a galvanic pile upon their nerves. During the action of the nerve, not only the quantity of the nervous substance diminished, but even its consistence. When a nerve was cut across and long subjected to the galvanic influence, he observed that the medulla became progressively softer, and finally distilled *guttatim* from the extremity of the divided trunk.—*Jour. des Progres, &c.*

*On the Medicinal Properties of Taraxacum.*

By Mr. HOULTON.—The most uniform and active preparation of this plant, I believe, may be obtained by carefully evaporating spontaneously\* the expressed juice of the roots taken up in August and September. The extract formed in this manner I have found to be a valuable medicine, both in my own person and in practice, and have the concurrent testimony of practitioners of different departments of the profession who have used it with success. A physician, who had been long resident in India, observed to me, after taking some of the extract prepared as above stated, "I have never before found any benefit from taraxacum." It is a tenacious, saponaceous mass, not ductile; it keeps remarkably well if evaporated sufficiently. It is a valuable anodyne, deobstruent, slightly aperient and diuretic. In some cases of chronic diarrhoea it has soothed the bowels, and has given that relief which no other medicine was found to afford.

In cases of chronic disorder of the digestive organs, not produced by intemperance, its efficacy is frequently very decided. In visceral derangements from intemperance I have not found it of much service; but in females, and other persons of sober habits and of studious and sedentary pursuits, it has been very beneficial, increasing the flow of bile, and allaying that uneasiness which the dyspeptic frequently experience about the hepatic region. If practitioners interested in the advancement of pharmacology and therapeutics, would employ the taraxacum in the form here proposed, I feel confident that a proper estimate of its virtues would be ascertained, and that the opinions of the filii apollinis would, respecting this medicine, cease to be discordant.—*Lond. Med. and Surg. Jour.*

*Hepatitis and Abscess of the Liver produced by Traumatic Lesions.*—The Ephémérides of Montpellier for March, 1828, contains a series of cases of hepatitis and abscess of the liver, all consecutive of traumatic lesions, collected in the practice of Professor Lallemand.

\* This is effected by placing it in shallow vessels, exposed to a current of dry air, or if placed in a situation artificially warmed in wet seasons, a similar preparation will be produced, one pound of root yielding two ounces of extract.

The frequency of these affections of the liver after injury of the brain, had long been known, but physicians had limited themselves to the observance of the connexion between these two organs, up to the period when M. Velpeau further generalized the question, and demonstrated in a work on tuberculous abscesses occurring after great operations, or profuse suppurations, that the liver and lungs are the organs in which these abscesses are especially developed. The following cases are adduced by the physician last mentioned, in corroboration of this opinion.

A soldier, æt. 24, who had been subject to epilepsy for fifteen years, received a wound on the head, in consequence of a fall while in a state of intoxication. He complained of pain in the head, for which he was bled; the abdomen became painful, and recourse was again had to antiphlogistic remedies. He appeared convalescent, when one evening, after exposure to the cool air of the yard of the hospital, he was attacked with erysipelas of the face, pain and tension in the right hypochondrium, with a general icteric tinge. Auscultation revealed, besides the affection of the liver, pleuro-pneumony of the right, and pleurisy of the left side. The disease continued its course, uncontrolled by the remedies employed; the brain became affected, and the patient died. In the liver, which was covered with yellowish spots, surrounded with black areolæ, abscesses were found with a membranous lining; its substance generally was softened; the condition of the lungs confirmed the diagnosis; the brain was injected.

Another soldier, æt. 22, had a jaundiced hue, with tension of the right hypochondrium and vomiting, in consequence of having undergone a very painful operation for the cure of fistula in perineo. These primary symptoms ceased when the catheter was withdrawn, and again recurred upon its re-introduction. Nausea now supervened, with vomiting of thick, greenish bile; the skin resumed its jaundiced tint; the features were sunken; pulse contracted; the right hypochondrium excessively painful on the slightest touch; irregular shiverings, and violent pain in the right knee. The patient died.

The liver was found in the same condition as in the preceding case; the other viscera were sound, but purulent matter was found in the right tibio-femoral articulation. It rarely happens, observes the narrator of the case, that patients die of an uncomplicated disease; most commonly the affection of an important organ induces that of several others; here the viscera escaped, but the articulations were attacked. In this instance there was a singular coincidence between the introduction of the catheter and the development of the symptoms of hepatitis. (Vide the works of MM. Dumas and Prevost, and the experiments of M. Simon De Metz.)\*

\* Journal of Foreign Medicine, Vol. I. page 399.

In the third case, the abscess of the liver was the consequence of unavailing attempts at lithotripsy, during which a portion of the mucous membrane of the bladder was removed in the grasp of the instrument. The symptoms were not so well marked as in the preceding cases, there was neither vomiting nor tumefaction of the right hypochondrium, but only a light icteric tint; acute pain in the lumbar and gluteal regions of the right side and internal part of the corresponding knee; at a later period, infiltration of the whole right inferior extremity, and almost complete suppression of urine.

In these three cases, the substance of the liver evinced different degrees of inflammation; its tissue, softer than natural, was in some places of a deep red colour. Professor Lallemand has long considered softening of the liver as a proof, and a consequence of inflammation of this organ. At a greater depth the pus appeared diffused through its substance, and flowed guttatim when pressure was made upon it; still deeper, genuine abscesses were found. The progress of inflammation, observes M. Velpeau, may thus be traced; 1st, by the softening and redness; 2d, by the purulent infiltration; 3d, by the formation of abscesses. If abscesses, so frequent in the liver, are rarely found in the lungs, this arises from the difference of structure of the respective organs. The areolar tissue of the lungs is not so well adapted to the formation of purulent collections, the pus having a greater tendency to infiltrate its substance. They are so rare, that both Broussais and Bayle deny their existence. Professor Lallemand has seen two examples.—*Journal des Progres, &c.*

*Case of Poisoning by Belladonna, followed by Scarlatina.* By M. JOLLY.—M. N——, æt. 46, took by mistake forty-four grains of the powdered plant; about an hour afterwards, he was attacked with violent headach, seated chiefly about the orbital fossæ, and soon followed by excessive redness of the eyes and face, which gradually extended over the whole surface of the body. The cutis presented a uniform red colour, exactly resembling that observed in scarlatina; moreover, his throat was of a deep red colour, and the seat of an acute sensation of heat, which appeared to extend throughout the alimentary canal. A circumstance not less remarkable, was the great irritation of the urinary passages, and especially of the neck of the bladder; the patient, in the midst of a loquacious delirium, which turned principally upon the pain which he experienced in this part, was continually making efforts to evacuate his urine, which was very red and bloody, and came away guttatim. He was bled largely, and demulcent beverages and emollient enemata, frequently repeated, were directed; with fomentations to the abdomen. The irritation of the bladder continuing, twenty leeches were applied to the hypogastrium, and after a few hours some relief was obtained. The patient slept during the night, and the

following morning complained only of a sensation of general *malaise*, which soon disappeared.

Among the reflections arising from this case, there is one to which it may be well to direct the attention of physicians; it is evident that the principal symptoms were those which characterize scarlatina; the cephalalgia, the uniform scarlet redness which showed itself successively upon the whole surface of the body, the angina which preceded, and the inflammation of the digestive and urinary passages which accompanied it, would seem in fact, to constitute a kind of artificial scarlatina. On several occasions, observes M. Jolly, I have seen the powder, and especially the extract of belladonna, produce the same scarlet redness of the skin, but never before had I observed this phenomenon carried to so great an extent, or accompanied by the other symptoms which I have mentioned.

M. Jolly does not infer from this case, that there is an identity of nature between the artificial scarlatina, produced by belladonna, and that arising naturally; still less would he attribute to the former, a preservative property against the latter; he has, however, deemed the fact worthy of record, at a time that such a doctrine has recently been promulgated by the German physicians.—*Nouvelle Bibliotheque Medicale.*

*Ossification of the Peritoneal Coat of the Liver.* By M. ROBERTS.—Dr. Baillie has recorded instances where the peritoneal coverings of the spleen and liver were converted into cartilage, more especially the former; and quotes a case from Morgagni where laminæ of bone were found in the midst of it. His words are as follow:—"I have also seen in some instances small spots of cartilage over the whole surface of the spleen. It would appear that ossifications are sometimes found in this cartilage; but in the cases which have come under my own observation, bony matter was not to be observed."

Now, in this case which I opened, the peritoneal covering of the liver was not studded with small spots of cartilage, but converted into one mass of it, being at the thinner parts one-eighth of an inch in thickness, and in many places half an inch; and in the midst of it were several scales of bone, one as large as a half-crown piece.

This is worthy of remark, as indicating that bone is one of the ulterior products of inflammation in serous membranes, and not, as Baillie suggests, a natural process misplaced. In this instance the peritoneum lining the flank was thickened, showing inflammation in its first stage; that covering the liver was cartilaginous, showing it in its second; and some portions of this last were ossified, showing it in its third.

It will also illustrate Dr. Ayre's Pathological Views of Dropsy: inflammation having arisen in a chronic form in the liver (which in this case had a granular appearance,) extended

to its peritoneal covering, and thence throughout the sac generally; thus displaying in different parts the various duration of the inflammatory process.—*Lond. Med. Gaz.*

*Rupture of the Uterus at the time of Quickening.* By Dr. ELSE.—Mrs. —, æt. 20, lost her life under the following circumstances:—She had been married about fifteen months, and, until the time of her conception, had enjoyed tolerable health; but since that period had suffered considerably from deep-seated pain in the back and uterine region, together with other symptoms threatening abortion.

Before her marriage, and up to the time of conception, she had experienced an unusual degree of pain at each menstrual period; and the catamenial discharge was exceedingly scanty. Her death appeared in some measure accelerated by an excursion to Greenwich, in company with her husband, as shortly after her arrival there she was attacked with vomiting and syncope, and in less than an hour she ceased to exist.

Upon examination it was discovered that a rent of about five inches in length had taken place in the uterus, extending itself from the cervix upwards at its anterior part, and rupturing a portion of the placenta. The fœtus lay in front of the uterus, enveloped by its internal membrane, and surrounded by coagulated blood, a quantity of which was also found between the intestines and in the cavity of the pelvis. The uterus itself was covered with dark-coloured spots, and easy lacerable; the ovaries were also in a state of disease—the one containing hydatids, the other with the same dark-coloured spots as the uterus. The fœtus appeared healthy, and is supposed by its movements to have caused the rupture of the uterus.—*Lond. Medical Gazette.*

*Gangrene of the Skin of the Lower Extremity.*—Professor Graefe has recently published the following interesting case, extracted from an official report, by Dr. Wassoefuhr, physician general to a military division. A musketeer was twice attacked in the course of last year with intermittent fever, and on both occasions completely recovered. In November he suddenly experienced a sensation of heat, very soon followed by pain, in the loins and lower extremities, which at the same time, began to tumefy. Shortly afterwards the skin of the latter was covered with spots of a blackish blue colour, a nervous fever supervened, the spots enlarged, united, and finally presented a gangrenous character, involving a great part of the integuments, but not passing beyond them. In addition to these symptoms, hydrothorax and ascites made their appearance, and the patient died on the seventeenth day of the disease.

On opening the body, the liver and spleen were found diseased; the latter especially, was greatly disorganized, its volume was con-

siderable, and it weighed two pounds and a half. It is evident, therefore, that the gangrenous affection of the extremities depended upon the internal disease.—*Graefe and Walther's Journal.*

*Suppuration of the Spleen.*—The following case is recorded in the *Osservatore Medico*, published at Naples. A miller, æt. 29, had for some time been affected with an obstruction of the spleen, consequent upon intermittent fever. Subsequently to some excesses in diet and exercise, the tumour increased in volume, and became more painful, presenting all the characters of well marked splenitis. Notwithstanding the repeated application of leeches, the employment of venesection, purgatives, and tartarized antimony, the disease continued its course unchecked; the tension of the hypochondrium increased; the pain became more severe, and was accompanied by shivering followed by heat, nocturnal sweats, &c. The induration of the spleen now disappeared, that organ increased in volume, and became softened at its inferior portion, indicating the existence of the suppurative process. Poultices were applied, and at the expiration of six days the fluctuation was so distinct, that it was decided to give vent to the matter by means of a trocar, which was accordingly introduced at the distance of about four inches from the linea alba. About three pounds of thick, fetid pus, of a dirty-white colour at first, and afterwards reddish, immediately flowed through the instrument. Great relief followed the operation; the wound, which was maintained open for several days, closed in less than a fortnight, and the patient entirely recovered.

*Extirpation of the Parotid Gland.*—This operation has recently been performed in two cases of scirrhus, by Mr. Gensoul, of Lyons. During the first operation eleven arteries were tied, the external carotid included. The wound healed within three months, and the patient at this time seemed perfectly cured. After six months he died, from frequent dietetic excesses; and the examination exhibited several disorganizations of the liver and the stomach, which, entirely independent of the previous operation, accounted for his death. No trace of the parotid was found. At the second operation, the external carotid was also tied, and the internal carotid, jugular vein, and the pneumo-gastric nerve, were laid bare up to their entrance into the skull. The patient was perfectly cured, and enjoyed good health for three years afterwards, except a paralytic affection of the left side of the face, the portio dura of the seventh nerve having been divided during the operation.—*Lancet.*

*On the Advantages of Graduated Compression in Ascites.*—A woman was admitted into the Clinical Institute of Parma, labouring

under ascites, which commenced several months before, and appeared to have been the consequence of peritonitis following difficult labour. The patient was much debilitated, had a slow fever, with great disturbance of the digestive functions; the urine was in small quantity and turbid, little thirst, great emaciation, &c. Squills, drastic purgatives, mercurials, &c., had been tried ineffectually. Professor Speranza did not perform the operation of paracentesis, not entertaining a very favourable opinion of the operation in any case, and from the state of the patient, deeming it particularly inapplicable in the present instance. The observations of Jenniker, Godelle, Recamier, and more recently of Moulon, induced him to employ graduated compression, by means of Monro's bandage. The consequence was a great increase in the quantity of urine, which became more and more perceptible, till ultimately it amounted to more than fifteen pounds in twenty-four hours. In the space of three weeks the swelling of the abdomen had entirely subsided. Under the use of squills, the sulphate of iron, and a more nourishing diet, the fever disappeared, and the patient left the hospital in a state of perfect health.—*Annali di Medicina*.

*Ablation of the Penis by means of Ligature*; by Dr. BIER.—The patient, an old man, æt. 62, had long laboured under extensive disease of the penis, which involved the whole of that organ, from its extremity to some distance below the symphysis, and rendered its removal necessary. The extent of the disease, the age and debilitated condition of the patient, the dread of hemorrhage, &c., led to the employment of the ligature, or constrictor of Dr. Mayor, recommended by that gentleman for the removal of certain tumours, &c. in preference to the scalpel. The following is the account of the operation.

An incision was made through the sound skin around the root of the penis, and the integuments separated to some distance beyond this part, by means of the fingers, assisted by the bistoury to divide some aponeurotic fibres and the suspensory ligament. In this stage of the operation four small arteries were divided, and immediately secured. The constrictor was then placed as far down as possible upon the penis, at the point of bifurcation of the corpora cavernosa, which was easily effected by drawing the organ forward; an incision four lines in length was then made in front of the constrictor, laying open the urethra, and permitting the introduction of a short silver tube, which was passed about an inch beyond the constrictor, and the instrument tightened as much as the patient could readily bear; the physician proposing to make still greater constriction on the morrow. Little pain followed the operation; the urine flowed through the tube, and in the course of three or four days, the whole of the penis included by the ligature, was thrown off in a state of

mortification. The patient entirely recovered.—*Revue Médicale*.

*Calculus Concretion in the Lacrymal Sac*; by Dr. KRIMER.—A woman, æt. 32, had been labouring under fistula lacrymalis for the space of nine months, when she consulted Dr. Krimer. This gentleman, introducing a probe into the fistula, found that the sac was not ulcerated, but that the obstruction arose from a hard body, which he supposed to be an osseous exudation, situated in the nasal canal. He attempted to overcome the obstruction by means of a pointed probe, but not succeeding, withdrew the instrument, in doing which, he met with considerable resistance, and was not a little surprised to find, attached to its point, a calculous concretion about the size of a small pea. The obstruction of the canal was now entirely removed; catgut bougies were introduced, and the cure was completed after the lapse of fifteen days.—*Graëfe and Walther's Journal*.

*Meliceris*.—A tumour about the size of an orange, situated upon the left cheek, was removed by Professor Weinhold, of Halle; a rapid and complete cicatrization followed its ablation. Such an excessively fetid odour was exhaled from the contents of the tumour, that the surgeon and his assistants were affected with nausea. Fourteen days afterwards the patient was attacked with intermittent fever, which resisted the most powerful remedies during six weeks, and did not yield until there appeared upon the left leg, an ulcer which secreted a matter, in odour perfectly similar to that of the contents of the tumour.—*Bibliothek der practischen Heilkunde*.

*Traces of Iodine in the Blood drawn from the vein of a person, who had employed, for some time, frictions with an ointment made from this substance*; by M. BENNERSCHIEDT.—No vestige of iodine was found in the serum of the blood, but traces of it were observed in the crassamentum, too slightly marked, however, to permit it to be obtained in an isolated state. The starch merely contracted a light blue shade. M. B. proposes to repeat his experiments upon larger quantities of blood.—*Archiv. des Apotheker Vereins, &c.*


*Vegetable Gelatine, and Albumen*.—M. Berzelius has lately examined gluten, and says that the gliadine and zymoma of Taddei are nothing else than the well known and ordinary principles of vegetables named above. Boil gluten with successive portions of alcohol until the latter ceases to become turbid upon cooling; mix these solutions with water, and distill; as the aqueous residuum cools, a glutinous coherent mass will separate, resembling gluten. It is *vegetable gelatine*, and the same substance as that separated by Einhof's

process from barley, &c. The substance insoluble in alcohol is vegetable albumen.

*Vegetable gelatine* is grayish-yellow in colour, adhesive, glutinous and elastic, having no taste, but a peculiar odour. It dries into a transparent, shining substance. It dissolves in alcohol; if cold alcohol be used, a viscid foreign substance is separated, not gelatine. It dissolves in vinegar, leaving also a viscid insoluble matter; when precipitated by an alkali, it resumes its viscid state. The mineral acids, with the exception of the phosphoric, form glutinous compounds insoluble until the excess of acid has been removed. This principle combines with and neutralizes alkalies, forming solutions, which, when evaporated, yield a transparent matter. Earths and oxides form insoluble compounds.

*Vegetable albumen* is almost perfect in its resemblance to white of egg. It dissolves in alkalies, and when in excess, the solutions are neutral. It then coagulates slightly by heat, but the principal part is retained in solution; it combines with acids, and when exactly saturated the substance remains soluble, but excess of acid (except the acetic and phosphoric) precipitates it. Before the action of potash, the vegetable albumen dissolves feebly in vinegar or phosphoric acid, but by ebullition with these acids, it forms a transparent colourless jelly of considerable volume.

The azoted principle contained in emulsive seeds has been considered analogous to the coagulum of milk. Souberian has shown that that from almonds has all the properties of white of egg; it is, in fact, the same substance as vegetable albumen.—*Ann. de Chimie*, xxxvii. 215.

*Reduction of Sulphuret of Arsenic*.—Sulphuret of arsenic is occasionally required to be reduced, when in very small quantities, in medico-chemical investigations. Berzelius remarks, that it may frequently be successfully performed, by putting it at the bottom of a small glass-tube, placing a small piece of steel-wire before it, and subliming it over the latter; the iron takes the sulphur; the arsenic condenses a little in advance. When the quantities are very small, this process sometimes fails; then Berzelius recommends the following:—The sulphuret is to be introduced into an open quill glass-tube, about four or five inches long, and being held obliquely, thus , is to be heated by a spirit-lamp, so that the hottest part shall be a little above the sulphuret, and the vapour be obliged to pass by it; the operation should be conducted slowly; the sulphur will burn into sulphurous acid and escape, and the arsenic into arsenious acid, which will condense in the upper cool part in crystals. The tube is then to be softened in the lamp, and drawn out below the arsenious acid; a little piece of charcoal is to be introduced, and then the arsenious acid passed across it in vapour, to the narrow elongated part of the tube; it will be reduced by the charcoal in its passage, and metallic

arsenic will appear. This process never fails.—*Annalen der Physik*.

*Decomposition of Ammonia by Metals*.—M. Savart found that 141.90 grains of thin copper wire became 142.382 grains, or acquired an increase of 0.472 in weight, when used for four hours to decompose ammonia: as the wire was in a slight degree oxidized, the experiment was repeated; and when every precaution was employed, the increase amounted to  $\frac{1}{2} \frac{7}{8}$ , and 0.105 of an unknown substance was absorbed by the copper, and its specific gravity was diminished from 8.8659 to 7.7919.

Iron also increases in weight, and diminishes in specific gravity by similar treatment, and will strike fire with flint like ordinary steel.—*Ann. de Chimie*.

*Effect of Elevation upon the Pulse and Breathing*.—Dr. Brunner, in ascending Mount Etna, in 1826, found that at Nicolosi, 3200 feet above the level of the sea, his pulse was 72; at Casa Gemmellara, 9300 feet high, it was 80; and at the summit of the mountain, 10,152 feet, it was 84; his natural pulse on the plain being 62–63. Notwithstanding the tenuity of the air at the above elevation, he experienced no inconvenience in respiration. These observations correspond to some made by Dr. Parrot on the Pyrenees.—*Foriep's Notizen*, No. 6.

*Native Iodide of Mercury*.—M. Del Rio has already mentioned that he has discovered iodide of silver in America, and he has mentioned its locality. He has since discovered another iodide; and he is of opinion that the metal in combination with it is mercury. It perfectly resembles dark-coloured cinabar, except that its colour is deeper and its streak paler; it is however certain, that it accompanies and earthy iodide, which M. Del Rio believes to be the metal of magnesia mineralized by iodine.—*Hensman's Repertoire de Chimie*.

*New Vegetable Alkali*.—Dr. Nicholas Mill, of Bogota, Columbia, has communicated to the Editor of the Quarterly Journal of Science and Art, that he has discovered a new vegetable alkali in the Quina Blanca of Mutis (*Cinchona Ovalifolia*, *Cinchona Macrocarpa* of Vohl,) which he calls Blanquinine, to distinguish it from others, and to convey an idea whence it proceeds. He is now engaged in examining its salts, and he promises to publish, through the same medium, the results of his researches on this interesting substance.

*Committee for Investigating the Plague*.—*Academie Royale de Medecine*. This body has elected a committee of five members for the study of the plague in the Levant. MM. Panjet and Champollion are amongst the number. They were to embark at the end of last month at Toulon, for Alexandria.—*From La Clinique*.

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